

Review began 10/04/2026
Review ended 10/14/2026
Published 10/18/2026

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DOI: 10.7759/cureus.118089

Reverse Shoulder Arthroplasty Versus Non-operative Treatment for Displaced Three- and Four-Part Proximal Humeral Fractures in Older Adults: A Systematic Review and Meta-Analysis

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Abstract

Reverse shoulder arthroplasty (RSA) is increasingly used for displaced three- and four-part proximal humeral fractures in older adults, but comparative evidence has until recently been predominantly observational. Three randomised trials now permit a synthesis restricted to randomised evidence. MEDLINE, Embase, and Cochrane CENTRAL were searched from inception to 15 September 2026 for studies comparing RSA with non-operative treatment in adults aged 60 years and over with an acute displaced Neer three- or four-part fracture. The primary outcome was the unadjusted total Constant-Murley score at 12 months, pooled as a mean difference using a random-effects model with restricted maximum likelihood estimation of τ^2 and a Hartung-Knapp confidence interval, with the modified (ad hoc) Hartung-Knapp interval reported alongside. Risk of bias was assessed with RoB 2 and certainty with the Grading of Recommendations Assessment, Development and Evaluation (GRADE) approach. Three randomised trials (190 analysed participants; 90 RSA, 100 non-operative) and seven observational cohorts, six of which contributed to a narrative synthesis, were included. All three trials favoured RSA at 12 months. The pooled mean difference was 8.49 points (95% CI: 0.53-16.46; $p = 0.044$; $I^2 = 0\%$) with the protocol interval and 8.49 points (95% CI: -0.55 to 17.53; $p = 0.056$) with the modified interval, so statistical significance depended on the variance estimator. The estimate lay below the 9.3-point minimal clinically important difference, which was used as an approximate guide. Sensitivity analyses using the alternative Constant-Murley values published within one trial gave 8.76 and 9.33 points, with confidence intervals including no difference. Complications occurred in 5 of 90 arthroplasty participants and 4 of 100 non-operative participants. Reoperation or revision occurred only after arthroplasty (2 of 90), whereas 5 of 40 participants allocated to non-operative treatment in one trial crossed over to surgery. Certainty was low for the primary outcome and very low for harms. Randomised evidence favours RSA in one-year shoulder function, but the size and clinical importance of the benefit remain uncertain.

Categories: Trauma, Sports Medicine, Orthopedics

Keywords: conservative treatment, constant-murley score, meta-analysis, older adults, orthopaedic trauma, proximal humeral fracture, randomised controlled trial, reverse shoulder arthroplasty

Introduction And Background

Proximal humeral fractures are among the most frequent fragility fractures of the upper limb, accounting for approximately 4%-10% of all adult fractures and ranking as the third most common osteoporotic fracture in older people after fractures of the wrist and hip [1,2]. Their incidence rises with age and continues to increase as populations age [3]. The fracture is also a marker of skeletal fragility that often goes unrecognised: in one population-based series, only 6.5% of patients were receiving treatment for osteoporosis when they sustained it [1], and a proximal humeral fracture is associated with a more than fivefold increase in the risk of hip fracture during the following year [4]. Its management therefore includes secondary fracture prevention, as well as treatment of the shoulder [5].

Fractures are commonly described using the Neer classification, which counts the number of displaced segments among the four principal fragments (articular head, greater tuberosity, lesser tuberosity and shaft), a segment being regarded as displaced when it is separated by more than 1 cm or angulated by more than 45° [6]. Between one-quarter and one-half of proximal humeral fractures are minimally displaced [1,7], and these unite reliably with a sling and early mobilisation. A substantial minority are displaced three- and four-part injuries, which made up 30.9% and 6.9% of fractures, respectively, in a recent population-based series [1]. These behave differently: the tuberosities separate from the head and shaft, the bone is osteoporotic and poorly suited to holding metalwork, and the vascular supply to the articular segment may be disrupted. This subgroup accounts for much of the long-term disability attributable to the injury, and it is the subgroup in which the best treatment remains unresolved [8,9].

How to cite this article

Kussad A, Mahmoud A M, Al Zareeni O T, et al. (October 18, 2026) Reverse Shoulder Arthroplasty Versus Non-operative Treatment for Displaced Three- and Four-Part Proximal Humeral Fractures in Older Adults: A Systematic Review and Meta-Analysis. *Cureus* 18(10): e118089. DOI 10.7759/cureus.118089

Three broad options exist. Non-operative treatment avoids surgical risk in a frail population but frequently results in union in malposition, with shortening, varus angulation, and tuberosity displacement that can limit rotation and elevation. Open reduction and internal fixation restores anatomy but depends on bone that will hold a screw; in patients over 60 years, it continues to carry high rates of complication and reoperation, reported in one large series as 44% and 11%, respectively, with reoperation rising to 18% after four-part fractures, including screw cut-out, loss of reduction, and avascular necrosis [10]. Arthroplasty removes the fractured articular segment from the equation, but the outcome of hemiarthroplasty is strongly dependent on healing of the tuberosities, which is unreliable in osteoporotic bone. Reverse shoulder arthroplasty was adopted in this setting because its semi-constrained design medialises and lowers the centre of rotation, lengthening the deltoid lever arm and tensioning the deltoid so that it can elevate the arm without a functioning rotator cuff [11]; function therefore depends less on tuberosity union and rotator cuff integrity. Its use for acute fracture in older patients has risen substantially: in the United States, reverse arthroplasty increased from 13% to 24% of operatively treated proximal humeral fractures in patients aged 65 years and over between 2011 and 2013, and from 32.2% to 53.3% of arthroplasties performed for this injury [12].

That adoption has outpaced the randomised evidence supporting it. The PROFHER trial, the largest randomised comparison of surgical and non-surgical treatment for displaced proximal humeral fracture, found no difference between the two at two and five years [8,13]. Its applicability to the present question is nevertheless limited. Most surgical procedures were open reduction and internal fixation rather than arthroplasty, four-part fractures were uncommon, adults of all ages were eligible, and patients judged to have a clear indication for surgery were not randomised [8]. Pooled analyses have since argued that trials defined by a specific fracture configuration and procedure are more likely to inform practice [14], and a Cochrane review concluded that the evidence base remains limited [9].

Existing systematic reviews of reverse arthroplasty against non-operative treatment have been constrained by the evidence available to them. Bosch et al. searched to January 2023 and pooled three comparative studies [15], and Lu et al. searched to October 2023 and included six [16]; each identified only a single randomised trial [17], so neither could restrict synthesis to randomised evidence, and both therefore pooled cohorts in which the treating surgeon selected the treatment. Lai et al. combined reverse arthroplasty with hemiarthroplasty, which answers a different question [18]. A further review by Chowdhury et al., published in September 2026 from searches run in December 2024, combined two randomised trials with seven retrospective cohorts, admitted patients from 50 years of age, assessed study quality with the Methodological Index for Non-Randomised Studies and did not grade the certainty of evidence [19]. Confounding by indication is the central difficulty: patients offered and accepting arthroplasty differ systematically in function, cognition and comorbidity from those managed non-operatively, and that difference biases in the same direction as the effect being estimated.

Two further randomised trials and a long-term follow-up of the original randomised cohort have since been published [20–22], making a synthesis restricted to randomised evidence possible for the first time. This review adds to the most recent synthesis in three respects: it includes the third randomised trial, published after that review's search [21], together with the long-term randomised follow-up; it restricts quantitative synthesis to randomised evidence and pools the Constant-Murley score as a mean difference on its native scale; and it grades the certainty of the evidence. All studies included in that review were checked against our eligibility criteria (Appendix 4).

We therefore set out to estimate the effect of reverse shoulder arthroplasty compared with non-operative treatment on shoulder function at one year in adults aged 60 years and over with an acute displaced three- or four-part proximal humeral fracture, to quantify harms, and to judge the certainty of that evidence. Function was measured with the Constant-Murley score, a 100-point composite that combines patient-reported pain (15 points) and activities of daily living (20 points) with clinician-measured range of motion (40 points) and strength (25 points), higher scores indicating better function [23,24]. It was the primary outcome of two of the three trials and was reported by all three.

Review

Methods

Protocol and Eligibility

This review was conducted and reported in accordance with PRISMA 2020 [25]; the completed checklist is provided in Appendix 2. A protocol specifying the population, eligibility criteria, primary outcome, effect measure, and planned analyses was finalised before analysis. It was written after eligible studies had been identified during a preceding evidence-gap assessment, and the review was not prospectively registered. Choices of primary outcome, time point, and minimal clinically important difference (MCID) could therefore have been influenced by knowledge of the trial results; this is considered in the certainty assessment and the limitations. The protocol and deviations from it are provided in Appendix 3. No change was made to the population, eligibility criteria, primary outcome, effect measure, or primary statistical model after analysis began.

Eligible participants were adults aged 60 years and over with an acute displaced Neer three- or four-part proximal humeral fracture, or an equivalent complex pattern under another validated classification (Arbeitsgemeinschaft für Osteosynthesefragen/Orthopaedic Trauma Association (AO/OTA) type 11-B or 11-C); mixed-age studies were eligible only where older participants were separately extractable. The intervention was primary reverse shoulder arthroplasty for the index fracture, and the comparator was non-operative management. Case series, reviews, biomechanical studies, comparisons whose only surgical arm was hemiarthroplasty or internal fixation, protocols without results, and abstracts with insufficient data were excluded. Meta-analysis was restricted to randomised trials; observational cohorts were summarised narratively. The full inclusion and exclusion criteria are shown in Table 1.

Criterion	Inclusion	Exclusion
Population	Adults aged 60 years and over.	Studies confined to younger adults; mixed-age studies in which participants aged 60 years and over could not be separately extracted.
Fracture type	Acute displaced Neer three- or four-part proximal humeral fracture, or an equivalent complex pattern under another validated classification (AO/OTA type 11-B or 11-C).	Undisplaced, two-part or pathological fractures; fracture sequelae, malunion or non-union; mixed fracture populations in which eligible patterns could not be separately extracted.
Intervention	Primary reverse shoulder arthroplasty performed for the index fracture.	Hemiarthroplasty or internal fixation as the only surgical arm; arthroplasty performed for failed previous treatment.
Comparator	Non-operative management.	Studies without a non-operative comparator group.
Study design	Comparative randomised controlled trials and comparative observational cohort studies.	Case series, narrative and systematic reviews, biomechanical studies, protocols without results, and conference abstracts with insufficient data.
Outcomes	Unadjusted total Constant-Murley score at 12 months (primary). Secondary: DASH, WOOS, Subjective Shoulder Value, pain on a visual analogue scale, active forward elevation, complications, reoperation or revision, crossover to surgery (surgery after allocation to non-operative treatment) and mortality.	Studies reporting none of the listed outcomes in a form permitting comparison between treatment groups.
Synthesis	Meta-analysis restricted to randomised controlled trials; observational cohorts summarised narratively (SWiM).	Observational cohorts at critical risk of bias on ROBINS-I were not included in the synthesis.

TABLE 1: Inclusion and exclusion criteria

AO/OTA: Arbeitsgemeinschaft für Osteosynthesefragen/Orthopaedic Trauma Association; DASH: Disabilities of the Arm, Shoulder and Hand; ROBINS-I: Risk Of Bias In Non-randomised Studies of Interventions; SWiM: Synthesis Without Meta-analysis; WOOS: Western Ontario Osteoarthritis of the Shoulder Index

Search Strategy

MEDLINE (PubMed), Embase (Ovid), and Cochrane CENTRAL (Wiley) were searched from inception to 15 September 2026 without language, date, publication-type, or study-design restrictions. Each search combined a fracture concept with a reverse-arthroplasty concept using controlled vocabulary and free-text terms; comparator, outcome, study-design, and age terms were not applied as search restrictions and were instead assessed at screening (Table 2). The complete database-specific search strings, with line-by-line result counts, are provided in Appendix 1, reported in accordance with PRISMA-S [26]. ClinicalTrials.gov and CENTRAL were searched for ongoing and unpublished trials, and forward and backward citation searching was performed on all included reports and previous systematic reviews. The review was conducted in stages. The protocol, eligibility criteria, and screening workflow were established during an earlier round of searching, and the searches were then re-executed in full on 15 September 2026. All reports identified in the earlier round were retrieved again by the final search, and all records were screened against the same criteria; the flow diagram reports the final search only.

Concept	Controlled vocabulary	Representative free-text terms	Applied in search
Fracture	MEDLINE: Shoulder Fractures; Humeral Fractures. Embase: exp humerus fracture. CENTRAL: Shoulder Fractures; Humeral Fractures (exploded)	proximal humer* fracture*; humeral head fracture*; shoulder fracture*	Yes (combined with OR)
Reverse arthroplasty	MEDLINE: Arthroplasty, Replacement, Shoulder. Embase: exp shoulder arthroplasty. CENTRAL: Arthroplasty, Replacement, Shoulder (exploded)	reverse shoulder arthroplasty; reverse total shoulder arthroplasty or replacement; reverse prosthesis; RSA; RTSA; Delta prosthesis	Yes (combined with OR)
Combination	-	Fracture concept AND reverse-arthroplasty concept	Yes
Comparator, outcomes, design, age	-	non-operative; conservative; Constant-Murley; randomised; cohort; aged 60 and over	No - assessed at screening
Limits	-	-	None (language, date, publication type or design)

TABLE 2: Search concepts as executed

Searches were run on 15 September 2026 from database inception in MEDLINE (PubMed; 961 records), Embase (Ovid; 2,044 records), and Cochrane CENTRAL (Wiley; 67 trial records). Comparator, outcome, design, and age terms were not applied as search restrictions because terminology in this literature is variable and applying them reduces sensitivity. The complete line-by-line strings for each database, reported in accordance with PRISMA-S [26], are in Appendix 1.

MeSH: Medical Subject Headings; RSA: reverse shoulder arthroplasty; RTSA: reverse total shoulder arthroplasty

Study Selection and Data Extraction

Two reviewers (AK and OTAZ) independently screened titles and abstracts and assessed full texts against the eligibility criteria, with disagreements resolved by discussion.

Data Collection and Data Items

Two reviewers (AK and OTAZ) independently extracted data, recording the source of every value; discrepancies were resolved against the source publication. The data items included study design, setting, age criteria, numbers of participants randomised and analysed, fracture classification, implant type and tuberosity management, rehabilitation protocols, timing of follow-up, and outcome-specific data. For each outcome, the extracted data included the mean, standard deviation, and number of participants analysed in each arm or the number of events and the corresponding denominator. Outcomes without a reported or derivable measure of dispersion were reported narratively. Missing standard deviations were not imputed for any primary or secondary analysis, and missing participants were not imputed; each analysis used the participants analysed by the trial. Where a report gave inconsistent values for the same outcome, the alternatives were carried into sensitivity analyses. Where more than one report described the same cohort, only the most complete contributed to any analysis. The arm sizes analysed by Miquel et al. were not stated and were derived arithmetically from reported percentages (Table 3 [20]; every derivation is described in the table footnotes. The extraction sheet and analysis code are available from the corresponding author on request.

Risk of Bias Assessment

Two reviewers (AK and AM) independently assessed risk of bias at outcome level using RoB 2 for randomised trials [27] and ROBINS-I for non-randomised studies [28], with disagreements resolved by discussion. Studies at critical risk were excluded from synthesis.

Outcome Measures

The primary outcome was the unadjusted total Constant-Murley score [23,24] at 12 months, using a protocol-specified 9-18 month window; where more than one assessment fell within the window, the one closest to 12 months was to be used. Twelve months was chosen because it was the primary or principal follow-up time point of all three trials. Age- and sex-adjusted Constant-Murley scores are on a different

scale and were not pooled with unadjusted scores. Secondary outcomes were the Disabilities of the Arm, Shoulder and Hand score (DASH); Western Ontario Osteoarthritis of the Shoulder index (WOOS); subjective shoulder value, pain on a visual analogue scale, active forward elevation, complications, reoperation or revision, crossover to surgery, and mortality. Crossover was defined as surgery on the injured shoulder after allocation to non-operative treatment. Following peer review, it was also combined with reoperation after arthroplasty as unplanned surgery in either arm.

Synthesis Methods

Analyses were performed in R version 4.4.1 (R Foundation for Statistical Computing, Vienna, Austria) using the metafor package [29]. Continuous outcomes were pooled as mean differences on the native scale using a random-effects model with restricted maximum likelihood estimation of τ^2 and a Hartung-Knapp confidence interval [30]. Because the plain Hartung-Knapp interval can be narrower than a fixed-effect interval when the observed dispersion between studies is smaller than expected ($Q < k - 1$), the modified (ad hoc) Hartung-Knapp interval, which does not allow the standard error to fall below its fixed-effect value, was also reported following peer review [31]; the protocol-specified plain interval was retained as the primary analysis. A 95% prediction interval was calculated for the primary outcome [32]. Where only two estimates contributed (DASH and the leave-one-out analyses), a conventional (Wald-type) random-effects interval was used, because a Hartung-Knapp interval based on two studies rests on a t distribution with one degree of freedom and is uninformative; Hartung-Knapp results for these analyses are reported for completeness. Mean differences are arthroplasty minus non-operative, so positive values favour arthroplasty for the Constant-Murley score, WOOS, subjective shoulder value and forward elevation, and negative values favour arthroplasty for DASH and pain.

Dichotomous outcomes were pooled as Mantel-Haenszel risk ratios and risk differences without continuity correction. Trials with no events in either arm contribute no information to a risk ratio and were excluded from that estimate; where one arm had no events in any trial, the risk ratio was not estimable, so the risk difference was reported, with the Peto odds ratio as a sensitivity analysis [33]. Heterogeneity was quantified using τ^2 , I^2 and Cochran's Q , recognising that these estimates are unreliable with three trials. Secondary outcomes were analysed without adjustment for multiple comparisons and are interpreted as supportive rather than confirmatory. Pooled effects were interpreted against published MCIDs for the Constant-Murley score of 5.7 [34] and 9.3 points [35], the latter specified in the protocol as the primary benchmark. Both were derived for within-patient change after elective shoulder arthroplasty and are not validated thresholds for between-group differences in acute fracture, so they were used as an approximate guide only. A responder analysis (the proportion of participants reaching the MCID in each arm) was considered but was not possible, because no trial reported it and individual participant data were not available.

Synthesis of Observational Evidence

Observational cohorts were not pooled. They were summarised narratively in accordance with the Synthesis Without Meta-analysis (SWiM) guideline [36]: cohorts were grouped by outcome domain (function, harms and secondary surgery, and mortality), the direction of effect on each cohort's principal reported outcome was tabulated, and no vote counting based on statistical significance was used.

Reporting Bias Assessment

Funnel plots and tests for small-study effects were not used, as fewer than 10 studies contributed to any analysis [37]. Trial registries were searched for completed but unpublished trials, and the registered primary outcomes of included trials were compared with those reported.

Certainty of Evidence

Certainty of evidence was rated using the Grading of Recommendations Assessment, Development and Evaluation (GRADE) approach [38].

Results

Study Selection

The search identified 3,073 records. After removal of 999 duplicates, 2,074 records were screened and 32 full texts assessed, of which 19 were excluded. Thirteen reports met the eligibility criteria, describing three randomised trials in four publications and seven observational cohorts in nine publications (Figure 1). One observational cohort was excluded at critical risk of bias, leaving six for narrative synthesis. The excluded cohort was that of Amin et al., in which the non-operative group comprised patients who had been offered arthroplasty and declined it [39]. Of the nine studies included in the review by Chowdhury et al. [19], seven are included here, one is a conference abstract duplicating an included cohort, and one was excluded because participants aged 60 years and over could not be separately extracted (Appendix 4).

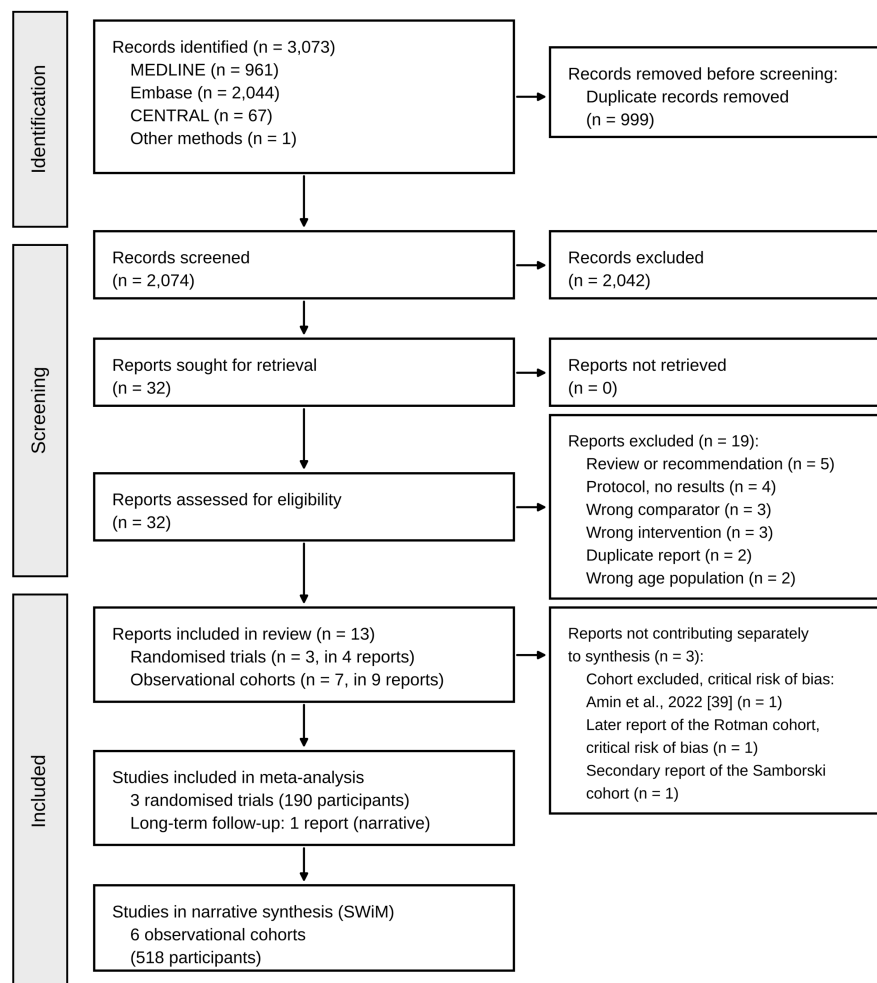


FIGURE 1: PRISMA 2020 flow diagram

The flow of records through the review is reported in accordance with PRISMA 2020 [25]. MEDLINE, Embase, and Cochrane CENTRAL were searched on 15 September 2026 from database inception, without language or date limits.

Study Characteristics

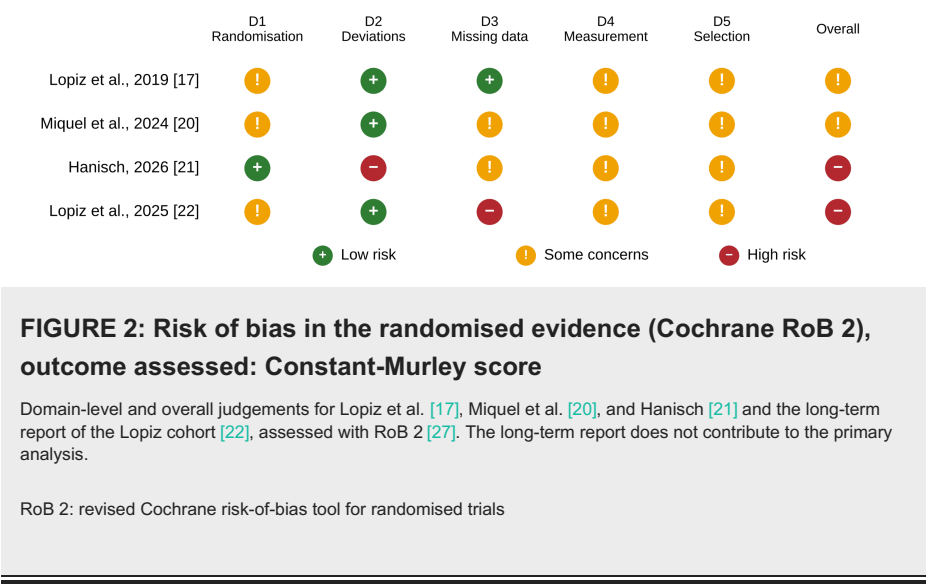
The three trials randomised 223 participants and analysed 190 at 12 months, 90 in the arthroplasty groups and 100 in the non-operative groups. Lopiz et al. randomised 62 patients aged 80 and over at a single Spanish centre [17]. Miquel et al. randomised 81 patients aged over 70 across multiple Spanish centres and analysed 66 by intention to treat [20]. Hanisch allocated 80 patients aged 60-90 at a single Danish unit, delivering arthroplasty in two implant subgroups; 5 of 40 patients allocated to non-operative treatment underwent delayed surgery [21]. All three trials assessed outcomes at 12 months, so no measurement from elsewhere in the 9-18-month window was used.

Mean age and tuberosity healing differed between trials; the latter was reported in approximately 52% of arthroplasties by Lopiz et al. [17] and in 92.3% and 100% of the Hanisch subgroups [21]. Rehabilitation differed in timing between arms. In the trial by Hanisch, arthroplasty patients began exercises on the first postoperative day, whereas non-operative patients were immobilised in a sling for about two weeks before pendulum exercises; thereafter both arms followed the same physiotherapy programme, with written instructions and supervised and self-guided exercises [21]. The information available to us for the trials by Lopiz et al. and Miquel et al. did not allow the duration of immobilisation or the progression of active exercise to be compared [17,20].

Risk of Bias

Lopiz et al. and Miquel et al. raised some concerns overall [17,20], and the trial by Hanisch was judged at high risk (Figure 2) [21]. That trial reported no intention-to-treat analysis despite crossover to surgery; loss to

follow-up was 18.75% and differential, and the calculated sample size of 58 per group was not reached. Measurement of outcome raised concerns in all three trials, as participants cannot be blinded to surgery and the Constant-Murley score is partly observer-administered [23]. All six observational cohorts in the narrative synthesis were at serious risk of bias, principally from confounding by indication (Table 3).



Study	Design, country	Age criterion	RSA/Non-op analysed	Follow-up	Risk of bias
Lopiz et al., 2019 [17]	RCT, Spain, single centre	≥80 y	29/30	12 mo	Some concerns
Miquel et al., 2024 [20]	RCT, Spain, multicentre	>70 y	31/35 a	12 mo	Some concerns
Hanisch, 2026 [21]	RCT, Denmark, single unit	60-90 y	30/35	12 mo	High (domain 2)
Lopiz et al., 2025 [22]	Long-term report of Lopiz et al., 2019	≥80 y	17/12	7.4-7.6 y	High (domain 3)
Chivot et al., 2019 [40]	Retrospective, France	>70 y	28/32	32 mo	Serious
Rotman et al., 2018 [41]	Retrospective, Israel	≥75 y	62/83	12 mo	Serious
Samborski et al., 2022 [42]	Retrospective, USA	≥60 y	24/41	12 mo	Serious
Matheron et al., 2025 [43]	Retrospective, UK	>60 y	35/64	35 mo	Serious
Solans et al., 2024 [44]	Ambispective, Spain	≥75 y	15/16	12 mo	Serious
Bozca et al., 2026 [45]	Retrospective, Türkiye	≥65 y	57/61	12 mo	Serious

TABLE 3: Characteristics of the included studies

Randomised trials were assessed with RoB 2 [27] and observational cohorts with ROBINS-I [28]. Of the nine observational reports describing seven cohorts, two were at critical risk of bias, corresponding to one excluded cohort [39]; the second critical-risk report was a later report of the Rotman cohort, and one duplicated an included cohort (a secondary report of the Samborski cohort); the six index reports that contribute to the narrative synthesis are listed. All three trials reported the primary outcome at the 12-month assessment.

a Arm sizes derived arithmetically and corroborated by that trial's report of two major complications representing 6.5% of the intervention arm [20].

RCT: randomised controlled trial; RSA: reverse shoulder arthroplasty

Reporting Biases

No results publication was identified for three registered randomised trials of this comparison, PROFHER-2

(ISRCTN76296703), the Nordic DeltaCon trial (NCT03531463) and ReShAPE (ACTRN12616000345482), and a fourth (NCT03599336) was recorded as terminated in 2020 because of slow enrolment, without results. Miquel et al. registered the Constant-Murley score at two years as their primary outcome (NCT03610113), whereas the published report gives 12-month results [20].

Primary Outcome

All three trials favoured arthroplasty at 12 months: 6.00 points (95% CI: -0.25 to 12.25) [17], 8.80 points (95% CI: 1.67-15.93) [20], and 12.70 points (95% CI: 4.16-21.24) [21]. The pooled mean difference was 8.49 points (95% CI: 0.53-16.46; p = 0.044), with no detectable heterogeneity (τ² = 0.00, I² = 0%, Q = 1.55, df = 2, p = 0.46) (Table 4, Figure 3). Because Q was smaller than its degrees of freedom, the plain Hartung-Knapp interval was narrower than the corresponding fixed-effect interval under the same t distribution; with the modified interval, the result was 8.49 points (95% CI: -0.55 to 17.53; p = 0.056) [31]. Statistical significance therefore depended on the variance estimator. The 95% prediction interval (-15.02 to 32.01 points) rests on a t distribution with one degree of freedom and is uninformative with three trials. The point estimate lies just below the 9.3-point MCID used as a guide [35].

Table with 5 columns: Study, RSA n, mean (SD), Non-op n, mean (SD), Mean difference (95% CI), Weight. Rows include individual studies (Lopez et al., Miquel et al., Hanisch) and pooled results using REML with Hartung-Knapp protocol and modified Hartung-Knapp.

TABLE 4: Primary outcome - unadjusted total Constant-Murley score at 12 months
τ² = 0.00; I² = 0%; Q = 1.55 (df = 2, p = 0.46); test of overall effect p = 0.044 (Hartung-Knapp, protocol analysis) and p = 0.056 (modified Hartung-Knapp [31]); 95% prediction interval -15.02 to +32.01 [32]. Positive values favour RSA. MCID of 9.3 Constant-Murley points [35] used as an approximate guide only. Mean differences were recomputed from published means, standard deviations and analysed arm sizes; Miquel et al. report their own estimate as +8.84 (95% CI +1.57 to +16.11), the difference arising from rounding and derived denominators [20].
CI: confidence interval; MCID: minimal clinically important difference; REML: restricted maximum likelihood; RSA: reverse shoulder arthroplasty; SD: standard deviation

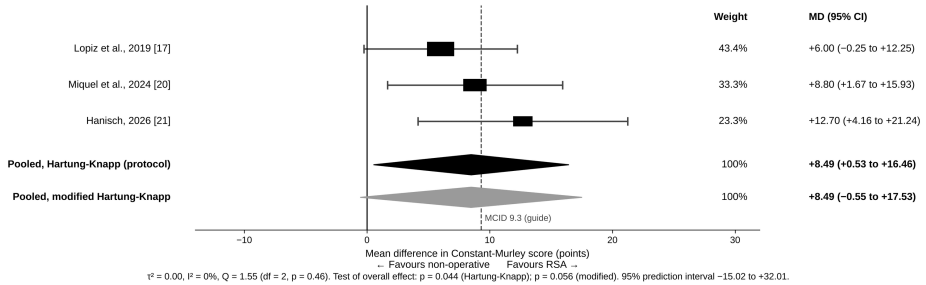


FIGURE 3: Constant-Murley score at 12 months, randomised trials
Forest plot of the primary outcome, pooling Lopez et al. [17], Miquel et al. [20], and Hanisch [21]. Squares are study-level mean differences sized by weight, horizontal lines are 95% confidence intervals, and the upper diamond is the protocol random-effects estimate with the Hartung-Knapp interval; the lower diamond shows the same estimate with the modified (ad hoc) Hartung-Knapp interval [31]. Positive values favour RSA; the dashed line marks the minimal clinically important difference of 9.3 points [35], shown as an approximate guide only.
MCID: minimal clinically important difference; REML: restricted maximum likelihood; RSA: reverse shoulder arthroplasty

Sensitivity Analyses

Hanisch reports three non-identical sets of Constant-Murley values: 51.4 (SD 15.8) versus 38.7 (SD 19.3) in the results narrative, 51.1 (SD 14.9) versus 36.7 (SD 16.7) in the abstract, and 51.38 versus 38.06 in the

outcome tables [21]. The narrative values were used, being the only source giving a combined arthroplasty mean with its standard deviation. Substituting the alternatives gave 9.33 and 8.76 points, with intervals including no difference (Table 5). The fixed-effect and random-effects estimates coincided because τ^2 was zero. In leave-one-out analyses with conventional intervals, as specified for two-study analyses, estimates ranged from 7.22 to 10.40 points, and no interval included zero; omitting the trial by Hanisch, which carries 23% of the weight, gave 7.22 points (95% CI: 2.51-11.92). Varying the derived arm sizes of the trial by Miquel et al. changed the pooled estimate by less than 0.01 points [20].

Analysis	k	Mean difference	95% CI	p
Primary: REML + Hartung-Knapp (protocol)	3	+8.49	+0.53 to +16.46	0.044
REML + modified (ad hoc) Hartung-Knapp	3	+8.49	-0.55 to +17.53	0.056
REML/fixed effect, conventional CI	3	+8.49	+4.37 to +12.61	<0.001
Hanisch abstract values [21]	3	+9.33	-1.13 to +19.79	0.062
Hanisch tabulated values [21]	3	+8.76	-0.10 to +17.61	0.051
Hanisch tabulated values, modified Hartung-Knapp	3	+8.76	-0.17 to +17.68	0.052
Leave-one-out: omit Lopiz et al. [17]	2	+10.40	+4.93 to +15.88	<0.001
Leave-one-out: omit Miquel et al. [20]	2	+8.69	+2.26 to +15.13	0.008
Leave-one-out: omit Hanisch [21]	2	+7.22	+2.51 to +11.92	0.003

TABLE 5: Sensitivity analyses of the primary outcome

Because τ^2 was zero, the random-effects and fixed-effect estimates coincide. For the tabulated values, the combined arthroplasty standard deviation (15.91) was derived from the two reported implant subgroups. With the abstract values τ^2 was 4.78 and the plain and modified Hartung-Knapp intervals were identical. Leave-one-out analyses use conventional (Wald-type) random-effects intervals, as specified in the Methods for analyses with two studies; with the Hartung-Knapp method, which rests on one degree of freedom at $k = 2$, the intervals were -13.98 to +34.79, -33.05 to +50.44 and -10.42 to +24.85 [30]. No analysis reverses the direction of effect. Analyses were performed in R using metafor [29].

CI: confidence interval; REML: restricted maximum likelihood

Secondary Outcomes

DASH favoured arthroplasty in both reporting trials, by 8.1 points in Lopiz et al. [17] and 21.4 points in the trial by Hanisch [21], with a pooled mean difference of -14.43 points (95% CI: -27.49 to -1.38; conventional interval) and $I^2 = 73\%$; with a Hartung-Knapp interval, the result was uninformative (-99.08 to 70.21). Hanisch reported WOOS, its own primary outcome, at 20.73 points (95% CI: 10.30-31.16), above the MCID of 12.4 points cited in that trial, together with the subjective shoulder value at 29.08 points (95% CI: 17.58-40.58) and active forward elevation at 28.30° (95% CI: 10.84-45.76) [21]. Pain on a visual analogue scale was reported with dispersion by Lopiz et al. alone, at -0.70 points (95% CI: -1.55 to 0.15) [17].

Harms

Complication and reoperation rates were calculated using the participants analysed at 12 months as denominators (90 and 100), because the trials reported these events for analysed participants; crossover and mortality used the participants allocated in the only trial reporting them. Complications occurred in 5 of 90 arthroplasty and 4 of 100 non-operative participants (risk ratio: 1.39, 95% CI: 0.38-5.12; risk difference 1.6 percentage points, 95% CI: -4.5 to 7.6), and reoperation or revision in 2 of 90 and none of 100 (risk difference: 2.3 percentage points, 95% CI: -0.8 to 5.3; Peto odds ratio: 8.57, 95% CI: 0.53 to 137.88) (Table 6, Figure 4). Crossover to surgery occurred in 5 of 40 participants allocated to non-operative treatment in the trial by Hanisch [21]; Lopiz et al. and Miquel et al. reported no secondary surgery in their non-operative arms [17,20]. Counting secondary surgery in both arms, unplanned surgery occurred in 2 of 90 arthroplasty participants (2.2%) and in at least 5 of 105 non-operative participants (4.8%), with the denominator for Hanisch being those allocated; the report does not make clear how the participants who crossed over were handled in its 12-month analysis [21]. Complication definitions differed between trials, and none used the published core set of unfavourable events for this injury [46]. Mortality was reported by Hanisch alone, with 2 of 40 deaths after arthroplasty and 1 of 40 after non-operative treatment, all attributed as unrelated to treatment [21].

Trial	Complications, RSA	Complications, non-op	Reoperation or revision after RSA	Secondary surgery after non-op (crossover)	Deaths, RSA/non-op
Lopez et al., 2019 [17]	2/29 (suprascapular nerve injury)	1/30 (non-union)	0/29	0/30	Not reported
Miquel et al., 2024 [20]	2/31 (periprosthetic infection; axillary nerve palsy)	0/35	1/31 (infection)	None reported	Not reported
Hanisch, 2026 [21]	1/30	3/35	1/30	5/40 allocated	2/40/1/40 allocated
Total	5/90 (5.6%)	4/100 (4.0%)	2/90 (2.2%)	≥5/105 (4.8%)	2/40/1/40
Pooled estimate	RR 1.39 (0.38 to 5.12); RD +1.6 pp (−4.5 to +7.6)		RD +2.3 pp (−0.8 to +5.3); Peto OR 8.57 (0.53 to 137.88); RR not estimable	Unplanned surgery in either arm: 2/90 vs ≥5/105	Single trial; not pooled

TABLE 6: Harms in the randomised trials

Complications and reoperations use the participants analysed at 12 months as denominators; crossover and deaths in the trial by Hanisch use the participants allocated. Lopez et al. had no reoperation in either arm; the trial contributes a difference of zero to the pooled risk difference but no information to a risk ratio. Mantel-Haenszel risk ratios and risk differences, without continuity correction [33]. Complication definitions differed between trials [46].

OR: odds ratio; pp: percentage points; RD: risk difference; RR: risk ratio; RSA: reverse shoulder arthroplasty

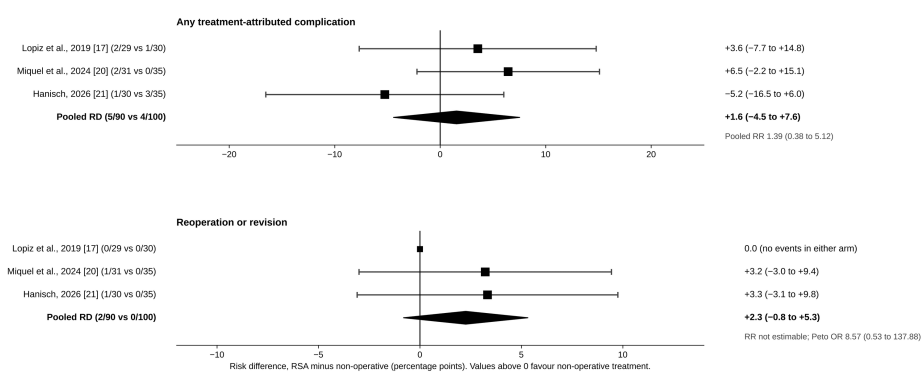


FIGURE 4: Harms in the randomised trials

Complications and reoperation in Lopez et al. [17], Miquel et al. [20], and Hanisch [21], shown as risk differences in percentage points, which are estimable in every trial. The pooled risk ratio for complications is also shown. Crossover to surgery and mortality were reported by a single trial and are given in Table 6 rather than plotted [21]. Values above 0 favour non-operative treatment. Diamonds are Mantel-Haenszel pooled estimates [33]. Lopez et al. recorded no reoperation in either arm; the trial contributes a difference of zero to the pooled risk difference but would contribute no information to a risk ratio [17].

RSA: reverse shoulder arthroplasty

Long-Term Follow-Up

Lopez et al. reassessed 29 of 62 randomised patients at a mean of 7.6 and 7.4 years, reporting Constant-Murley scores of 62 versus 51 (p = 0.039), DASH of 16 versus 25 (p = 0.069) and pain of 1.5 versus 1.4 (p = 0.274) [22]. Standard deviations were not reported and the surviving groups differed in age.

Observational Evidence

The six observational cohorts (518 participants) are summarised in Table 7. All five that reported a functional outcome favoured arthroplasty in the direction of effect on their principal functional measure: the Constant-Murley score in Chivot et al. [40], Solans et al. [44], and Bozca et al. [45]; PROMIS physical function in Samborski et al. [42] and the Oxford Shoulder Score in Matheron et al. [43]. Chivot et al.

nevertheless reported a worse QuickDASH score after arthroplasty, the only functional result in this review favouring non-operative treatment [40], and in Bozca et al. the advantage was confined to patients with higher deltoid muscle attenuation [45]. Secondary surgery after non-operative treatment was reported in two cohorts, in 2 of 41 participants by Samborski et al. and 13% by Matheron et al., against none of 24 and 2.9% after arthroplasty [42,43]. Rotman et al. reported only mortality, at 8.1% one year after arthroplasty versus 10.8% [41]. Because treatment was selected by the surgeon in every cohort, these results are reported as context rather than as evidence of effect.

Cohort	n RSA/non-op; follow-up	Principal functional outcome, RSA vs non-op	Direction	Harms and secondary surgery, RSA vs non-op
Chivot et al., 2019 [40]	28/32; ≥24 mo (mean 32)	Constant-Murley 82.1 vs 76.8 (p = 0.03); QuickDASH 38.7 vs 31.2 (p = 0.11)	Favours RSA (Constant-Murley); QuickDASH favours non-op	Complication rate higher after RSA
Rotman et al., 2018 [41]	62/83; 12 mo	Not reported	Not applicable	One-year mortality 8.1% vs 10.8% (p = 0.56)
Samborski et al., 2022 [42]	24/41; 12 mo	PROMIS physical function 46.0 vs 37.9 (three-group p = 0.01)	Favours RSA	Complications 1/24 vs 4/41; secondary surgery 0/24 vs 2/41 (conversion to arthroplasty)
Matheron et al., 2025 [43]	35/64; mean 35 mo	Oxford Shoulder Score 42 vs 36 (p = 0.048); QuickDASH 14.4 vs 25.4; SSV 84 vs 70	Favours RSA	Complications 10% vs 51%; reintervention 2.9% vs 13%
Solans et al., 2024 [44]	15/16; 12 mo	Constant-Murley 75.1 (SD 10.3) vs 51.9 (SD 12.4) (p = 0.001)	Favours RSA	No infection, instability or revision after RSA; one transient brachial plexus palsy. Avascular necrosis 3/16 non-op
Bozca et al., 2026 [45]	57/61; 12 mo	Constant-Murley, derived overall 45.67 vs 41.12; higher deltoid attenuation 59.28 vs 49.58 (p < 0.001); lower attenuation 31.57 vs 31.14 (p = 0.78)	Favours RSA (higher-attenuation subgroup only)	Not reported

TABLE 7: Narrative synthesis of the six observational cohorts (SWiM)

Synthesis followed the SWiM guideline [36]. Cohorts were grouped by outcome domain and the direction of effect on each cohort's principal reported functional outcome was tabulated; no vote counting based on p values and no pooling were performed. All six cohorts were at serious risk of bias on ROBINS-I [28], principally from confounding by indication. Overall values for Bozca et al. were derived from the two reported deltoid-attenuation subgroups; percentages for Matheron et al. are as reported [43]. The ORIF arm of Samborski et al. is not shown [42].

ORIF: open reduction and internal fixation; PROMIS: Patient-Reported Outcomes Measurement Information System; QuickDASH: shortened Disabilities of the Arm, Shoulder and Hand score; RSA: reverse shoulder arthroplasty; SSV: Subjective Shoulder Value; SWiM: Synthesis Without Meta-analysis

Certainty of Evidence

Certainty was low for the Constant-Murley score, WOOS, pain and active forward elevation, and very low for DASH, complications, reoperation, mortality, and the long-term outcome (Table 8). The post-hoc timing of the protocol was considered under risk of bias for the primary outcome but did not lead to a further downgrade, because the primary outcome and time point were those of the trials themselves and the evidence was already rated low certainty.

Outcome at 12 months	Participants (studies)	Effect (95% CI)	Certainty
Constant-Murley score (higher better)	190 (3 RCTs) [17,20,21]	MD +8.49 (+0.53 to +16.46); modified Hartung-Knapp -0.55 to +17.53	⊕⊕○○ Low ^a
WOOS (higher better)	65 (1 RCT) [21]	MD +20.73 (+10.30 to +31.16)	⊕⊕○○ Low
Pain, VAS (lower better)	59 (1 RCT) [17]	MD -0.70 (-1.55 to +0.15)	⊕⊕○○ Low
Active forward elevation	65 (1 RCT) [21]	MD +28.30° (+10.84 to +45.76)	⊕⊕○○ Low
DASH (lower better)	124 (2 RCTs) [17,21]	MD -14.43 (-27.49 to -1.38), I ² = 73%	⊕○○○ Very low
Any complication	190 (3 RCTs) [17,20,21]	RR 1.39 (0.38 to 5.12); RD +1.6 pp (-4.5 to +7.6)	⊕○○○ Very low
Reoperation or revision	190 (3 RCTs) [17,20,21]	RD +2.3 pp (-0.8 to +5.3)	⊕○○○ Very low
Mortality	80 (1 RCT) [21]	2/40 vs 1/40	⊕○○○ Very low
Constant-Murley at ≈7.5 years	29 of 62 randomised (1 report) [22]	62 vs 51 (p = 0.039)	⊕○○○ Very low

TABLE 8: GRADE summary of the findings

Population: adults aged ≥60 years with an acute displaced three- or four-part proximal humeral fracture; intervention: primary RSA; comparator: non-operative treatment. Certainty assessed using GRADE [38]. Downgraded once for risk of bias throughout, since the trial by Hanisch contributes 23% of the weight and was analysed per protocol despite crossover [21], and the other trials raise concerns from unblinded assessment and attrition without imputation [17,20]; once for imprecision in functional outcomes and twice in harms. Inconsistency was downgraded for DASH only.

^a The protocol was finalised after the trials had been identified and was not registered, so the choice of primary outcome, time point and MCID could have been influenced by knowledge of the results. This was considered under risk of bias but did not lead to a further downgrade, because the outcome and time point were those of the trials themselves and the evidence was already rated low. The MCID was used only as an approximate guide, as it was derived for within-patient change after elective arthroplasty.

CI: confidence interval; MD: mean difference; pp: percentage points; RCT: randomised controlled trial; RD: risk difference; RR: risk ratio; RSA: reverse shoulder arthroplasty; VAS: visual analogue scale; WOOS: Western Ontario Osteoarthritis of the Shoulder index; GRADE: Grading of Recommendations Assessment, Development and Evaluation

Discussion

Principal Findings

All three randomised trials favoured RSA over non-operative treatment for shoulder function at one year. The pooled difference falls just below the 9.3-point MCID [35]; its confidence interval extends from a negligible difference to one exceeding that threshold, and with the modified Hartung-Knapp interval, it includes no difference. The randomised evidence therefore favours RSA in direction, but the size and clinical importance of the benefit remain uncertain. Statistical significance should not be equated with clinical importance, and a mean between-group difference below the MCID does not mean that no individual patient benefits.

Comparison With Previous Reviews

Earlier syntheses were limited by the evidence then available. Bosch et al. and Lu et al. each identified one randomised trial, so neither could restrict synthesis to randomised evidence [15,16]. Lu et al. pooled the Constant score as a standardised mean difference of 1.83 (95% CI: -2.38 to 6.03) from two studies using the same 0-100 scale, which prevents comparison against a published MCID, and assessed funnel-plot asymmetry with four to six contributing studies, where such tests have low power [16,37]. Chowdhury et al. combined randomised and observational evidence and reported earlier improvement in range of motion and pain after arthroplasty [19]; the present analysis, restricted to randomised trials, is consistent with a functional advantage at one year but cannot establish its size.

Tuberosity Healing and Rehabilitation

Tuberosity healing was lower in Lopiz et al. (approximately 52%) than in Hanisch (92%-100%), and the effect

estimates were smaller and larger, respectively [17,21]. This pattern is hypothesis-generating only. It rests on an ecological comparison of three trials with no detectable statistical heterogeneity, and other differences between the trials could explain it: Lopiz et al. enrolled only patients aged 80 years and over, implants and tuberosity fixation differed, rehabilitation could not be fully compared, and the trial by Hanisch [21] was at high risk of bias. A biomechanical rationale nevertheless exists. RSA restores elevation through the deltoid, but external rotation depends on the posterior rotator cuff, notably teres minor, which inserts on the greater tuberosity, and internal rotation is rarely restored [11]. Meta-analyses of observational series found better range of motion and functional scores when the greater tuberosity healed after RSA for fracture [47,48], although in one included cohort Constant-Murley scores were similar whether or not the tuberosities healed in a correct position [44]. RSA is therefore less dependent on tuberosity union than hemiarthroplasty, although tuberosity healing may still affect rotation and overall function [11,47,48]. Whether it modifies the benefit of RSA over non-operative treatment can only be tested within trials, by analysing participants according to tuberosity status.

Rehabilitation may also influence the comparison. In the trial by Hanisch [21], arthroplasty patients started exercises on the first postoperative day, whereas non-operative patients were kept in a sling for about two weeks [21]; differences in the timing and progression of mobilisation between arms and between trials could affect early recovery and, possibly, function at one year. Randomised evidence on the timing of mobilisation after non-surgical treatment of these fractures is limited and inconclusive [9], and future trials should report the duration of immobilisation and the progression of active exercise in both arms.

Implications for Practice

From a surgical perspective, this uncertain functional benefit must be weighed against the risks of surgery. Secondary outcomes favoured RSA in direction, with WOOS exceeding the MCID cited in the single trial that assessed it, whereas pain scores showed no meaningful difference between groups. Reoperations occurred only in the arthroplasty groups, while some non-operative patients crossed over to surgery [21]. The trial by Hanisch [21], which carries 23% of the weight, was at high risk of bias, was analysed per protocol despite crossover, and reported non-identical Constant-Murley values in different sections; omitting it lowered the estimate to 7.22 points without changing its direction, and the alternative value sets changed statistical significance but not direction. The observational cohorts point the same way but were not pooled, since treatment was selected by the operating surgeon and confounding by indication biases towards the observed result. They are reported as context rather than as evidence of effect.

Implications for Research

Larger randomised evidence is expected. PROFHER-2 is comparing RSA, hemiarthroplasty, and non-surgical treatment in older adults with three- and four-part fractures and aims to recruit 380 participants, more than the 223 randomised across the three published trials combined [49]; its results should help establish whether the benefit observed here is clinically meaningful. Future trials should analyse by intention to treat, report unadjusted Constant-Murley scores with standard deviations alongside any adjusted values, report the proportion of participants reaching an MCID, tuberosity healing, rehabilitation protocols, crossover and mortality as standard outcomes, and adopt a standardised core set of unfavourable events so that harms are comparable between studies [46].

Limitations

Only three randomised trials with 190 analysed participants were available, so all estimates are imprecise, and heterogeneity statistics are unreliable. The trial contributing the largest effect was at high risk of bias, and significance depends on which of its reported value sets is used [21] and on the variance estimator, since the modified Hartung-Knapp interval included no difference. Between 5% and 19% of randomised participants were missing from each analysis and were not imputed, outcome assessment was unblinded throughout, complication definitions were not standardised, and mortality and crossover were each reported by a single trial.

Age thresholds, implant designs, tuberosity management, and rehabilitation protocols differed between trials, and rehabilitation could not be compared in detail. The MCIDs describe within-patient change after elective shoulder arthroplasty and were used only as a guide for a between-group difference [34,35]; neither was derived in acute fracture or non-operative care, and a responder analysis was not possible. The protocol was finalised after the trials were identified and was not registered, so choices of primary outcome, time point, and MCID could have been influenced by knowledge of the trial results. Web of Science, Scopus, and two trial registries named in the protocol were not searched (Appendix 3), and one registered trial of this comparison was terminated without reporting results. The arm sizes analysed by Miquel et al. [20] were derived rather than reported, although varying them had a negligible effect on the pooled estimate.

Conclusions

Randomised evidence favours reverse shoulder arthroplasty over non-operative treatment for shoulder function at one year in older adults with displaced three- and four-part proximal humeral fractures, but the

size and clinical importance of the benefit remain uncertain. The pooled benefit fell below the minimal clinically important difference used as a guide; its statistical significance depended on the method of analysis, certainty of evidence was low, and reoperation occurred only after arthroplasty, while some non-operative patients crossed over to surgery. Arthroplasty is therefore a reasonable option for selected, functionally active older adults rather than a default treatment; the criteria for selection remain to be defined, and the decision should rest on functional demand, comorbidity, and patient preference. Larger randomised trials are needed before the size and durability of the benefit can be regarded as established.

Appendices

Appendix 1: Database search strategies

Reported in accordance with PRISMA-S [26]. All searches were run on 15 September 2026 from database inception, with no language, date, publication-type or study-design limits. Comparator, outcome and design terms were not applied as restrictions, because terminology in this literature is variable and applying them reduces sensitivity; these criteria were applied at screening.

A1.1 MEDLINE (PubMed): 961 Records

("proximal humeral fracture"[tiab] OR "proximal humeral fractures"[tiab] OR "proximal humerus fracture"[tiab] OR "proximal humerus fractures"[tiab] OR "humeral head fracture"[tiab] OR "humeral head fractures"[tiab] OR "shoulder fracture"[tiab] OR "shoulder fractures"[tiab] OR "Shoulder Fractures"[Mesh] OR "Humeral Fractures"[Mesh])
AND
("reverse shoulder arthroplasty"[tiab] OR "reverse total shoulder arthroplasty"[tiab] OR "reverse shoulder replacement"[tiab] OR "reverse total shoulder replacement"[tiab] OR "reverse prosthesis"[tiab] OR "reversed total shoulder"[tiab] OR "reverse arthroplasty"[tiab] OR "inverted prosthesis"[tiab] OR RSA[tiab] OR RTSA[tiab] OR rTSA[tiab] OR "Delta prosthesis"[tiab] OR "Arthroplasty, Replacement, Shoulder"[Mesh])

A1.2 Embase (Ovid, Embase 1974 to 2026 September 11): 2,044 Records

The Embase strategy as executed is shown in Supplementary Table S9.

Line	Search	Results
1	exp humerus fracture/	19,399
2	(proximal adj2 humer* adj2 fracture*).ti,ab,kw.	4,790
3	(humeral head adj2 fracture*).ti,ab,kw.	299
4	(shoulder adj2 fracture*).ti,ab,kw.	968
5	1 or 2 or 3 or 4	20,893
6	exp shoulder arthroplasty/	11,501
7	(revers* adj3 (shoulder or total shoulder) adj3 (arthroplast* or replacement or prosthes*)).ti,ab,kw.	5,757
8	(RSA or RTSA or rTSA).ti,ab,kw.	13,508
9	(delta adj2 prosthes*).ti,ab,kw.	52
10	6 or 7 or 8 or 9	21,940
11	5 and 10	2,044

TABLE 9: Embase (Ovid) search strategy as executed on 15 September 2026

A1.3 Cochrane CENTRAL (Wiley): 67 Trial Records

The CENTRAL strategy as executed is shown in Supplementary Table S10.

Line	Search	Results
1	((proximal NEXT humer) NEAR/2 fracture):ti,ab,kw	449
2	("humeral head" NEAR/2 fracture*):ti,ab,kw	9
3	(shoulder NEAR/2 fracture*):ti,ab,kw	347
4	MeSH descriptor: [Shoulder Fractures] explode all trees	239
5	MeSH descriptor: [Humeral Fractures] explode all trees	269
6	#1 OR #2 OR #3 OR #4 OR #5	801
7	MeSH descriptor: [Arthroplasty, Replacement, Shoulder] explode all trees	149
8	(revers* NEAR/3 shoulder NEAR/3 (arthroplast* OR replacement OR prosthes*)):ti,ab,kw	274
9	(RSA OR RTSA OR rTSA):ti,ab,kw	1,149
10	(delta NEXT prosthes*):ti,ab,kw	1
11	#7 OR #8 OR #9 OR #10	1,366
12	#6 AND #11	68 (67 trials, 1 Cochrane review)

TABLE 10: Cochrane CENTRAL (Wiley) search strategy as executed on 15 September 2026

The 67 trial records were exported; the Cochrane review was identified through MEDLINE and Embase.

A1.4 Other Sources

ClinicalTrials.gov and Cochrane CENTRAL were searched for ongoing and unpublished trials. Backward and forward citation searching was performed on all included reports and previous systematic reviews until no further eligible studies were identified; this identified one additional record.

A1.5 Summary of Sources

Records retrieved from each source are summarised in Supplementary Table S11.

Source	Platform	Date searched	Records
MEDLINE	PubMed	15 September 2026	961
Embase	Ovid	15 September 2026	2,044
Cochrane CENTRAL	Wiley	15 September 2026	67
Database total			3,072
Citation searching and registers		15 September 2026	1

TABLE 11: Records retrieved from each source

Appendix 2: PRISMA 2020 checklist

The completed checklist is shown in Supplementary Table S12. Section names refer to the revised manuscript.

#	Item	Reported in					
1	Title: identify the report as a systematic review	Title					

2	Abstract	Abstract					
3	Rationale	Introduction & Background, paragraphs 1-6					
4	Objectives	Introduction & Background, final paragraph					
5	Eligibility criteria	Methods: Protocol, registration and eligibility; Table 1					
6	Information sources, with date last searched	Methods: Information sources and search strategy; Table 2; Appendix 1 (15 September 2026)					
7	Search strategy (full strategies for all databases)	Appendix 1					
8	Selection process	Methods: Study selection (two independent reviewers, AK and OTAZ)					
9	Data collection process	Methods: Data collection and data items					
10a	Outcomes sought, time points	Methods: Outcome measures					
10b	Other variables sought	Methods: Data collection and data items					
11	Study risk of bias assessment	Methods: Risk of bias assessment					
12	Effect measures	Methods: Synthesis methods					
13a	Processes for deciding eligibility for each synthesis	Methods: Protocol, registration and eligibility; Synthesis of observational evidence; Table 1					
13b	Data preparation (handling of missing data, conversions)	Methods: Data collection and data items; footnotes to Tables 3 and 7					
13c	Tabulation and display of results	Tables 3-8; Figures 1-4					
13d	Synthesis methods and rationale; model; heterogeneity; software	Methods: Synthesis methods; Synthesis of observational evidence (SWiM)					
13e	Methods to explore heterogeneity	Methods: Synthesis methods (heterogeneity quantified; subgroup analysis and meta-regression not possible with three trials)					
13f	Sensitivity analyses	Methods: Synthesis methods; Results: Sensitivity analyses; Table 5					
14	Reporting bias assessment	Methods: Reporting bias assessment					
15	Certainty assessment	Methods: Certainty of evidence					
16a	Results of search and selection, with flow diagram	Results: Study selection; Figure 1					
16b	Studies that appear to meet criteria but were excluded, with reasons	Results: Study selection; Appendix 4					
17	Study characteristics	Results: Study characteristics; Table 3					
18	Risk of bias in studies	Results: Risk of bias; Figure 2; Table 3					
19	Results of individual studies	Tables 4, 6 and 7; Figures 3 and 4					
20a	Characteristics and risk of bias of studies in each synthesis	Results: Primary outcome; Observational evidence; Tables 3 and 7					
20b	Results of statistical syntheses	Results: Primary outcome; Secondary outcomes; Harms; Tables 4-6					
20c	Investigations of possible causes of heterogeneity	Discussion: Tuberosity healing and rehabilitation (hypothesis-generating only)					
20d	Sensitivity analyses	Results: Sensitivity analyses; Table 5					
21	Reporting biases	Results: Reporting biases					
22	Certainty of evidence	Results: Certainty of evidence; Table 8					
23a	Interpretation in context of other evidence	Discussion: Principal findings; Comparison with previous reviews					
23b	Limitations of the evidence	Discussion: Limitations					

23c	Limitations of the review processes	Discussion: Limitations							
23d	Implications for practice, policy and research	Discussion: Implications for practice; Implications for research							
24a	Registration	Methods: Protocol, registration and eligibility (not registered)							
24b	Where the protocol can be accessed	Appendix 3							
24c	Amendments to the protocol	Methods: Protocol, registration and eligibility; Appendix 3 (deviations)							
25	Support	Additional Information (declarations)							
26	Competing interests	Additional Information (declarations)							
27	Availability of data, code and other materials	Trial-level data in Tables 4 and 6; extraction sheet and analysis code available from the corresponding author on request							

TABLE 12: PRISMA 2020 checklist

Appendix 3: Review protocol and deviations

The protocol below was finalised before any analysis was run, but after the eligible studies had been identified during a preceding evidence-gap assessment. The review is therefore not prospectively registered and holds no PROSPERO registration number. Section 3 of the protocol (named contact and review team) corresponds to the author list on the manuscript title page and is not reproduced here.

1. Review title

Reverse shoulder arthroplasty versus non-operative treatment for displaced three- and four-part proximal humeral fractures in older adults: an updated systematic review, meta-analysis and cumulative analysis of randomised evidence.

2. Anticipated start and completion

Start: September 2026. Anticipated completion: on verification of the extracted dataset by the review team.

4. Review question

Among adults aged ≥ 60 years with an acute displaced three- or four-part proximal humeral fracture, does primary reverse shoulder arthroplasty improve function, pain, range of motion, quality of life, complications, reoperation, mortality and long-term outcomes compared with non-operative treatment?

Secondary methodological question: has the accumulation of randomised evidence since the most recent dedicated systematic review materially changed the estimated treatment effect? This is addressed by cumulative meta-analysis and by structured comparison with previous reviews, not by selective inclusion of studies or outcomes.

5. Searches

Databases: MEDLINE/PubMed, Embase, Cochrane CENTRAL, Web of Science, and Scopus. From inception to the date of the final search. No language or date restriction. Trial registries: ClinicalTrials.gov, ISRCTN, ANZCTR, WHO ICTRP. Supplementary: backward and forward citation searching of all included studies and of previous systematic reviews; related-article searching. The search will be re-run immediately before submission.

6. Condition and population

Inclusion: adults aged ≥ 60 years with an acute displaced Neer three- or four-part proximal humeral fracture, or an equivalent complex category under another validated system (AO/OTA type 11-B or 11-C).

For mixed-age studies, inclusion requires either that participants aged ≥ 60 are separately extractable, or that the study's own stated inclusion threshold is ≥ 60 . This rule was fixed before final screening and was not revised after results were examined.

Exclusion: pathological fracture (other than osteoporotic), open fracture, established joint infection, pre-

existing neurological lesion of the affected limb, pre-existing severe glenohumeral arthritis, fracture sequelae, or delayed presentation beyond the acute window as defined by the study.

7. Intervention

Primary acute reverse shoulder arthroplasty / reverse total shoulder arthroplasty performed as treatment for the index fracture. Timing from injury, implant, approach, tuberosity management, and rehabilitation are extracted where reported.

8. Comparator

Non-operative/conservative management: sling immobilisation, analgesia, physiotherapy, progressive mobilisation or structured rehabilitation. The actual protocol is extracted where reported.

9. Study designs

Primary synthesis: randomised controlled trials only. Secondary synthesis: prospective and retrospective comparative cohort studies, analysed separately and never pooled with randomised evidence.

Excluded: case reports; case series without a comparator; narrative and systematic reviews as primary data; editorials; technical notes; biomechanical and cadaveric studies; comparisons in which the only surgical arm is hemiarthroplasty or open reduction and internal fixation; protocols and registry entries without published results; conference abstracts with insufficient data.

Multi-arm studies are eligible if the RSA and non-operative groups are separately extractable.

10. Outcomes

Primary outcome: Unadjusted total Constant-Murley score at approximately 12 months. Time window 9-18 months; where more than one measurement falls in the window, the one closest to 12 months is used. Adjusted (age- and sex-normalised) Constant scores are on a different scale and are not pooled with unadjusted scores.

Secondary outcomes: Constant-Murley at other time points; DASH; QuickDASH; ASES; WOOS; Oxford Shoulder Score; PROMIS; Subjective Shoulder Value; EQ-5D; SF-12/SF-36; pain (VAS/NRS); active and passive forward elevation, abduction, external and internal rotation; overall complications; infection; dislocation; nerve injury; nonunion; malunion; scapular notching; implant loosening; periprosthetic fracture; instability; revision; reoperation; crossover from non-operative treatment to surgery; mortality at 30 days, 90 days, one year and beyond; outcomes at ≥ 2 and ≥ 5 years.

Time strata (pre-specified, not pooled across strata): early ≤ 6 months; primary ≈ 12 months; intermediate ≈ 24 months; long term ≥ 5 years.

11. Data extraction

Two independent extraction passes for every included report, then discrepancy adjudication against the source with the page, table or figure recorded for each value. A raw extraction dataset is kept separate from the analysis dataset; raw values are never overwritten.

12. Risk of bias

Cochrane RoB 2 for randomised trials, applied at outcome level. ROBINS-I for non-randomised studies. Inability to blind participants and surgeons in a surgical trial is not, by itself, treated as high risk; it is assessed within the RoB 2 framework for the effect of assignment to intervention. Studies at critical risk of bias on ROBINS-I are excluded from synthesis and from conclusions.

13. Synthesis

Random-effects meta-analysis, REML estimator for τ^2 , Hartung-Knapp adjustment to confidence intervals. Mean difference on the native scale where all contributing studies use the same instrument; standardized mean difference only where different instruments measure a sufficiently similar construct, and never as a substitute for an available same-scale analysis. Risk ratios for dichotomous outcomes, with absolute event counts always reported; Mantel-Haenszel methods for rare events without indiscriminate continuity correction; studies with zero events in both arms are excluded from risk-ratio pooling, and this is stated. Heterogeneity reported as τ^2 , I^2 and Cochran's Q , interpreted in context rather than by fixed thresholds. Prediction intervals where $k \geq 3$.

Cumulative meta-analysis of the primary outcome ordered by year of publication.

Formal assessment of small-study effects will not be performed if fewer than approximately 10 studies contribute to an analysis, and its absence will be stated.

14. Subgroup analysis

Pre-specified but conditional on sufficient data: three-part versus four-part fracture; age ≥ 75 versus younger; randomised versus observational evidence; follow-up duration; timing of RSA; fracture dislocation versus fracture without dislocation. Subgroup analyses will not be performed where the number of contributing studies makes them uninterpretable, and all are exploratory.

15. Sensitivity analysis

RCTs only versus all comparative studies; leave-one-out; exclusion of studies at high or critical risk of bias; fixed versus random effects; Hartung-Knapp versus conventional confidence intervals; exclusion of imputed or converted means and standard deviations; exclusion of studies using non-equivalent fracture classifications.

16. Certainty of evidence

GRADE for Constant-Murley, pain, forward elevation, complications, reoperation or revision, and mortality. Heterogeneity that is modest and does not alter the direction of effect will not by itself trigger an inconsistency downgrade.

17. Dissemination

Peer-reviewed publication. Full dataset, analysis code and audit trail released with the manuscript.

18. Conflicts of interest and funding

No financial support was received from any organisation for this review. The authors have no financial relationships at present or within the previous three years with any organisation that might have an interest in the work, and no other relationships or activities that could appear to have influenced it.

Protocol deviations and clarifications

Apart from completing item 18 (declarations, as stated in the manuscript) and clarifying the wording of item 2, the protocol text above has not been altered or backdated. The conduct of the review differed from it as follows.

1. Web of Science, Scopus, ANZCTR and the WHO International Clinical Trials Registry Platform, listed in the protocol, were not searched; trial registry records were identified through Cochrane CENTRAL and ClinicalTrials.gov.
2. The protocol states that observational evidence would be analysed separately; this refers to the principal synthesis. Observational evidence was summarised narratively using the SWiM guideline [36].
3. The sensitivity analysis using alternative Constant-Murley values reported by Hanisch [21] was not pre-specified. It was added after full-text review showed non-identical values in different sections of that report.
4. Added in response to peer review: the modified (ad hoc) Hartung-Knapp interval [31] reported alongside the protocol interval, which is retained as the primary analysis; the conventional interval specified for two-study analyses applied consistently to the leave-one-out analyses (previously reported with Hartung-Knapp intervals); risk differences reported for complications as well as risk ratios; and crossover combined with reoperation as unplanned surgery in either arm. A responder analysis was considered but was not possible from the published data.
5. The protocol lists cumulative meta-analysis and several subgroup analyses. With three trials, subgroup analyses were not performed, as the protocol allows, and the cumulative analysis adds nothing to the three-trial forest plot, so it is not presented.

No change was made to the population, eligibility criteria, primary outcome, effect measure, or primary statistical model after analysis began.

Appendix 4: Cross-check of studies included by Chowdhury et al. and

full-text exclusions

A4.1 Studies Included by Chowdhury et al. [19]

Chowdhury et al. searched MEDLINE, CINAHL, Embase and CENTRAL in December 2024, admitted patients from 50 years of age, and included nine studies (two randomised trials and seven retrospective cohorts). Each was checked against the eligibility criteria of this review (Supplementary Table S13).

Study by Chowdhury et al.	Design	Status in this review	Reason			
Amin SJ, et al. Semin Arthroplasty JSES 2022 [39]	Retrospective	Included in review; excluded from synthesis	Critical risk of bias on ROBINS-I: the non-operative group comprised patients who had been offered arthroplasty and declined it			
Chivot M, et al. J Shoulder Elbow Surg 2019 [40]	Retrospective	Included; narrative synthesis	Eligible			
Haws BE, Samborski SA, Karnyski S, et al. J Orthop Trauma 2023;37:e247-52	Retrospective	Included as a secondary report of the Samborski cohort; not used separately	Same institution and first three authors as Samborski et al. [42]; eligibility of its sample (age threshold, fracture types) could not be confirmed			
Solans López MC, et al. Rev Chil Ortop Traumatol 2024 [44] (listed as "Lopez" by Chowdhury et al.)	Ambispective	Included; narrative synthesis	Eligible			
Lopiz Y, et al. J Shoulder Elbow Surg 2019 [17]	Randomised trial	Included; meta-analysis	Eligible			
Matheron G, et al. JSES Int 2024;8:1338 (conference abstract)	Retrospective	Excluded as duplicate report	Conference abstract of the cohort reported in full by Matheron et al. 2025 [43], which is included			
Miquel J, et al. J Shoulder Elbow Surg 2024 [20]	Randomised trial	Included; meta-analysis	Eligible			
Roberson TA, Granade CM, Hunt Q, et al. J Shoulder Elbow Surg 2017;26:1017-22	Retrospective	Excluded at full text	No age threshold; participants aged 52 to 88 years with no separately reported outcomes for those aged 60 and over; arthroplasty group combined acute and delayed (more than 30 days) procedures			
Samborski SA, et al. JSES Int 2022 [42]	Retrospective	Included; narrative synthesis	Eligible (AO/OTA 11C fractures in patients aged 60 years and over; only the RSA and non-operative arms are used)			

TABLE 13: Studies included by Chowdhury et al. and their status in this review

Studies in this review that were not included by Chowdhury et al.: Hanisch [21] (randomised trial published after their search), Lopiz et al. [22] (long-term randomised follow-up), Rotman et al. [41] (mortality only), Bozca et al. [45], and a later report of the Rotman cohort (excluded at critical risk of bias).

A4.2 Reports Excluded at Full Text (n = 19)

Reports excluded at full-text review are listed by reason in Supplementary Table S14.

Category (n)	Reports
Duplicate report (2)	Lopiz Y, et al. Long-term outcomes (journal-supplement republication). J Shoulder Elbow Surg 2026;35(9S):S32-S38. Matheron G, et al. Conserve or reverse? (conference abstract). JSES Int 2024;8(6):1338
Systematic or narrative review, or recommendation (5)	Lu VY, et al. 2025 [16]; Bosch TP, et al. 2024 [15]; Lai B, et al. 2024 [18]; Handoll HH, et al. Cochrane review 2022 [9]; Brazilian Society of Shoulder and Elbow Surgery. Evidence-based reviews and recommendations: reverse arthroplasty for complex proximal humeral fractures in elderly patients. 2025
Protocol without results (4)	Rangan A, et al. PROFHER-2 protocol 2023 [49]; Launonen AP, et al. Nordic DeltaCon trial protocol. BMJ Open 2019;9:e024916; Smith GC, et al. ReShAPE trial protocol. Trials 2017;18:91; NCT06444828 protocol and statistical analysis plan (protocol document of the trial by Hanisch)
Wrong comparator (3)	Fraser AN, et al. DelPhi trial (reverse arthroplasty versus plate fixation). J Bone Joint Surg Am 2020;102:477-85; Fjalestad T, et al. DelPhi five-year follow-up, 2024; Laas N, et al. SHERPA trial (reverse arthroplasty versus hemiarthroplasty), 2024
Wrong intervention (3)	Rangan A, et al. PROFHER 2015 [8]; Handoll HH, et al. PROFHER five-year follow-up 2017 [13]; Olerud P, et al. Hemiarthroplasty versus non-operative treatment. J Shoulder Elbow Surg 2011;20:1025-33
Wrong age population (2)	Erpala F, et al. Cureus 2021;13:e17516 (threshold of 50 years, no extractable subgroup aged 60 and over); Roberson TA, et al. 2017 (see A4.1)

TABLE 14: Reports excluded at full text, by reason

Additional Information

Author Contributions

All authors have reviewed the final version to be published and agreed to be accountable for all aspects of the work.

Concept and design: Adnan Kussad, Omar T. Al Zareeni

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Disclosures

Conflicts of interest: In compliance with the ICMJE uniform disclosure form, all authors declare the following: Payment/services info: All authors have declared that no financial support was received from any organization for the submitted work. Financial relationships: All authors have declared that they have no financial relationships at present or within the previous three years with any organizations that might have an interest in the submitted work. Other relationships: All authors have declared that there are no other relationships or activities that could appear to have influenced the submitted work.

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