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Interventions for adhesive capsulitis: systematic review and network meta-analysis of randomised controlled trials

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ABSTRACT

OBJECTIVE

To compare the benefits and harms of therapeutic interventions for adhesive capsulitis.

DESIGN

Systematic review and network meta-analysis of randomised controlled trials.

DATA SOURCES

Medline, Cochrane Library, Embase, Web of Science, and Scopus were searched from inception to 1 May 2025, with an update in November 2025.

ELIGIBILITY CRITERIA FOR SELECTING STUDIES

Randomised controlled trials enrolling adults with adhesive capsulitis, comparing active treatment with another intervention, placebo, or usual care.

MAIN OUTCOME MEASURES

Pain, function, range of motion, and adverse events.

RESULTS

A total of 189 randomised controlled trials involving 12 988 participants and 59 interventions were included. Most trials contributing to principal outcomes were at high risk of bias (71% for pain, 72% for function, and 71% for range of motion). At short term follow-up, moderate certainty evidence suggested that extracorporeal shockwave therapy probably reduced pain compared with usual care (mean difference -1.97 on a 0-10 scale, 95% confidence interval (CI) -2.97 to -0.97). Low certainty evidence suggested that extracorporeal shockwave therapy may improve function (mean difference -11.42 on a 0-100 scale, 95% CI -16.68 to -6.15), although direct and indirect estimates were limited by important incoherence. For short term range of motion, moderate certainty evidence suggested that manipulation under anaesthesia combined with intra-articular steroid injections probably improved flexion (mean difference 33.29°, 95% CI 11.19° to 55.39°), as did manipulation under anaesthesia alone (26.16°, 8.19° to 44.13°). Low certainty evidence suggested that extracorporeal shockwave therapy may improve flexion (22.42°, 12.99° to 31.84°) and abduction (18.62°, 8.09° to 29.14°), but both comparisons were limited by important incoherence. At mid-term follow-up, evidence for possible improvements in function with extracorporeal shockwave therapy and flexion with passive mobilisation was of low certainty, while evidence for pain reduction with extracorporeal shockwave therapy was of very low certainty. Most other comparative estimates were supported by low or very low certainty evidence. Comparative safety remained highly uncertain because adverse events were incompletely and inconsistently reported; the absence of reported adverse events should not be interpreted as evidence of absence of harm.

CONCLUSION

Moderate certainty evidence suggests short term pain reduction with extracorporeal shockwave therapy and improved short term flexion with manipulation under anaesthesia, with or without intra-articular steroid injections. Most other treatment effects remain uncertain because of low certainty and incoherence, and comparative safety and mid-term effects are poorly established. The available evidence is mainly short term and outcome specific. Longer term comparative effectiveness, overall benefits and harms, and applicability beyond predominantly primary adhesive capsulitis remain uncertain.

SYSTEMATIC REVIEW REGISTRATION

PROSPERO CRD420251070468

WHAT IS ALREADY KNOWN ON THIS TOPIC

Adhesive capsulitis causes substantial shoulder pain, functional impairment, and restriction of active and passive range of motion

Many conservative, injection based, procedural, and surgical treatments are available, but their comparative benefits and harms remain uncertain

Previous network meta-analyses have focused on selected groups of interventions, leaving uncertainty about the comparative effects of the full range of available treatments

WHAT THIS STUDY ADDS

Moderate certainty evidence suggests that extracorporeal shockwave therapy probably reduces short term pain, while manipulation under anaesthesia, with or without intra-articular steroid injections, probably improves short term flexion compared with usual care

Apparent benefits of extracorporeal shockwave therapy for short term function and range of motion remain uncertain because of low certainty evidence and inconsistency between direct and indirect estimates

Evidence for most other interventions, outcomes beyond three months, and comparative safety is of low or very low certainty. Therefore, the review does not establish a definitive treatment hierarchy, long term comparative effectiveness, or overall benefit-harm profiles

Introduction

Adhesive capsulitis, or frozen shoulder, is a disabling musculoskeletal disorder characterised by gradual onset of shoulder pain and progressive restriction of active and passive motion in the glenohumeral joint, resulting in major impairment of daily activities and reduced quality of life.[1 2] Adhesive capsulitis affects approximately 2-5% of the general population, most commonly occurring in people aged 40-60 years, and is more prevalent in women than in men.[3 4]

The management of adhesive capsulitis primarily focuses on symptom relief, namely pain reduction and the restoration of joint mobility and function. Various treatment options are available, including conservative interventions, pharmacological interventions, pharmacological and conservative interventions combined, and surgical interventions with or without adjunct treatments. Despite the diversity of interventions, a lack of systematic evidence remains comparing their relative efficacy and guiding optimal selection. Many guidelines[5-8] proposed management strategies for adhesive capsulitis based on literature reviews, surveys, or expert consensus, but they provide limited comprehensive and objective comparisons of different interventions. The British Elbow and Shoulder Society guideline,[9] published in 2025, conducted traditional meta-analyses on specific questions. However, the guideline included cohort studies and randomised controlled trials, the total number of studies was limited, and a full comparison of all interventions was not performed. Although several network meta-analyses[10-16] have been conducted, most focus on specific types of interventions, such as pharmacological treatments, corticosteroid injections, arthroscopic surgery, or non-surgical treatment strategies, leaving a comprehensive comparison of all available interventions largely unaddressed.

Considering the current lack of comprehensive and systematic evidence based data evaluating treatments for adhesive capsulitis, we conducted a network meta-analysis of randomised controlled trials to compare the effects of various interventions on pain relief, functional scores, and improvements in range of motion at short term and mid-term follow-up. This study aims to provide clinically relevant insights to guide treatment decisions.

Methods

This meta-analysis was conducted and reported in accordance with the PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) guidelines.[17] The study protocol has been registered in PROSPERO (registration number CRD420251070468). The systematic review team consisted of sports medicine physicians, physiotherapists, and orthopaedic surgeons.

Data sources and search strategy

A comprehensive systematic literature search was conducted to identify studies on the treatment of adhesive capsulitis. Databases searched were Medline, Embase, Web of Science, Cochrane Library, and Scopus, with a search timeframe from inception to 1 May 2025. After the initial analysis, the search was updated to November 2025. A detailed search strategy is provided in the supplementary materials (appendix 1). Additionally, the reference lists of relevant reviews and meta-analyses were examined.

Study selection and inclusion criteria

After removing duplicates, two trained reviewers independently screened the titles and abstracts to identify eligible randomised controlled trials. The same reviewers subsequently assessed the full texts of potentially eligible studies.

The inclusion criteria were predefined before the study selection process. Studies that met the following criteria were included: participants aged ≥18 years with a clinical diagnosis of adhesive capsulitis, including primary and secondary adhesive capsulitis; randomised controlled trial design with participants allocated to receive active treatment interventions or control interventions, which could be another active treatment, placebo, sham surgery, or usual care.

Studies were excluded if they met any of the following conditions: non-English publications; sample sizes that were too small, defined as fewer than 10 participants in any treatment group; full text not available; insufficient information to calculate an effect estimate for any prespecified outcome and follow-up window; or no eligible between-treatment comparison.

All interventions were independently reviewed and classified by two clinical experts. Interventions involving physical therapy were further classified in detail owing to major heterogeneity across studies. Specifically, treatments like extracorporeal shockwave therapy, laser therapy, and heat therapy, representing fundamentally different approaches, were classified separately based on their specific techniques. For studies involving placebo interventions, control groups were classified based on their characteristics: if the control group received only inactive or sham treatments, it was categorised as placebo; if the placebo control group also received the same background exercise as the intervention group, it was still recorded as a placebo group. Any disagreements were resolved through discussion to reach a consensus.

Intervention node construction and comparator classification

Intervention arms were independently reviewed and coded by clinical experts according to the principal therapeutic modality, active treatment components, route of administration, anatomical target, and mode of delivery. Treatment nodes were defined according to the actual intervention received in each randomised arm rather than whether an arm was labelled as experimental or comparator in the original study reports. Active interventions were therefore retained as their corresponding treatment specific

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nodes, whereas usual care and placebo or sham conditions were represented separately because these comparator conditions were not considered clinically exchangeable.

Treatments administered equally to all randomised groups were regarded as shared background cointerventions and were not used to define the difference between treatment nodes. When all groups received the same exercise, physiotherapy, rehabilitation, or conservative programme and only one group received an additional active intervention, the node was defined by that additional intervention. Therefore, the resulting relative effect represented the incremental effect of the intervention in addition to the shared background care. Interventions were grouped within the same node when they represented clinically comparable variants of the same therapeutic modality and were expected to have sufficiently similar mechanisms and clinical applications. Minor differences in dose, treatment frequency, session duration, device settings, or implementation were retained within the same node when they were considered variations of the same clinical intervention. Treatments were separated when they differed materially in their active components, route or anatomical site of administration, procedural technique, or intended mechanism of action.

When two or more active treatment components beyond the shared background treatment were administered only to one randomised group, the complete regimen was classified as a separate combination node. Estimates for combination nodes represent the effect of the complete treatment package and should not be interpreted as the independent effect of an individual component or as evidence of additive or synergistic interactions.

Usual care, placebo or sham treatment, and active treatment comparators were treated as distinct nodes. Studies were assigned to the usual care node only after reviewing the actual content of the comparator treatment rather than relying solely on the terminology used by the original authors. Two clinical reviewers independently classified usual care comparators as multimodal, exercise based, physiotherapy based, analgesic or other conservative care, or minimal or unclear usual care, with disagreements resolved through discussion.

Background treatment balance was assessed according to whether shared co-interventions were provided similarly across randomised groups. Studies rated as clearly, probably, or largely or mostly balanced were grouped as broadly balanced, whereas those rated as partly balanced or unbalanced, absent, or unclear were not.

Data extraction

Reviewers independently extracted data from the included studies using a standardised data extraction form. The collected information included study characteristics (first author, publication year, country, and funding source), participant demographics (sample size, age, sex), and disease related characteristics (stage of adhesive capsulitis, symptom duration). Detailed information on intervention and control measures was systematically recorded. For studies including a usual care comparator, we also extracted the components of usual care and whether shared background treatments were applied similarly across randomised groups.

Outcomes and follow-up windows

The outcomes of interest were pain, shoulder function, range of motion, treatment related adverse events, and health related quality of life. Pain was primarily assessed using the visual analogue scale or numerical rating scale. When a study reported more than one type of pain outcome at the same follow-up time, data were extracted according to the following prespecified order: overall or average pain, pain during movement, and resting or night pain.

Shoulder function was assessed using validated functional instruments. When more than one functional scale was reported, the Shoulder Pain and Disability Index was prioritised, followed by the Constant-Murley Score and other validated shoulder specific functional measures.

Range of motion outcomes included flexion, extension, abduction, external rotation, and internal rotation. Passive range of motion measurements were prioritised when passive and active measurements were reported. Active measurements were extracted when passive measurements were unavailable, and measurements for which the assessment type was not specified were retained in the primary analysis. For internal rotation, only measurements reported in degrees were included in the quantitative synthesis.

Adverse events were defined as the number of participants experiencing at least one treatment related adverse event or complication during the intervention or reported follow-up period. Health related quality of life outcomes, including the physical and mental component summaries of the 36 Item Short Form Health Survey, were extracted when available.

Outcomes were classified into two prespecified follow-up windows: short term (defined as up to three months after start of treatment) and mid-term (defined as more than three months and up to six months). When a study reported more than one eligible assessment within the same follow-up window, the assessment closest to three months was selected for the short term analysis, and the assessment closest to six months was selected for the mid-term analysis.

Risk of bias assessment

Two reviewers independently assessed risk of bias for each outcome specific result using the revised Cochrane Risk of Bias tool.[18] The five domains and overall risk of bias were rated as low risk, some concerns, or high risk according to the tool guidance. Disagreements were resolved through discussion.

Data preparation and effect measures

Pain outcomes were linearly transformed to a 0-10 scale, function outcomes to a 0-100 scale, and range of motion outcomes were expressed in degrees.[19] When necessary, scales were reoriented so that higher scores consistently indicated greater pain or worse function. These transformations were used to facilitate quantitative synthesis and do not imply equivalence among the original instruments.

For pain, function, and range of motion outcomes, treatment effects were analysed using change from baseline. Negative mean differences favoured the intervention for pain and function, whereas positive mean differences favoured the intervention for range of motion. Continuous outcomes were summarised as mean differences with 95% confidence intervals, and adverse events as risk ratios with 95% confidence intervals. Standardised mean differences were examined in sensitivity analyses.

When a study did not directly report the mean change but provided baseline and follow-up means, the mean change was calculated by subtracting the baseline mean from the follow-up mean. When the corresponding standard deviation of the change score was unavailable, it was estimated from the baseline and follow-up standard deviations using the method recommended in the Cochrane Handbook, assuming a baseline to follow-up correlation coefficient of r=0.50.[20] The primary analysis using r=0.50 was prespecified in the PROSPERO protocol. Because no directly applicable empirical correlation estimate was available for patients with adhesive capsulitis, r=0.50 was adopted as a pragmatic working assumption informed by previous methodological practice.

When studies reported medians with interquartile ranges and/or minimum and maximum values rather than means and standard deviations, means and standard deviations were estimated using established statistical methods.[21-24] Studies were not included in the quantitative synthesis when the reported information was insufficient to derive the required summary statistics reliably.

Statistical analysis

Conventional pairwise meta-analyses were conducted when at least two studies reported the same direct treatment comparison for the same outcome and follow-up window. Random effects models were used throughout because clinical and methodological heterogeneity was anticipated across studies. Between study variance was estimated using the restricted maximum likelihood method,[25] and confidence intervals for the pooled effects were calculated using the Hartung-Knapp adjustment.[26]

Frequentist random effects network meta-analyses were conducted to combine direct and indirect evidence. Within each outcome and follow-up specific network, a common between study heterogeneity variance was assumed across treatment comparisons. Network meta-analysis was performed only when the evidence network was connected and included at least 10 studies.

All eligible treatment arms from multi-arm trials were retained. Arm level data were converted into all possible pairwise comparisons, and comparisons originating from the same trial were assigned the same study identifier. Within study correlations between comparisons sharing a common treatment arm were accounted for using the multi-arm reweighting procedure implemented in the netmeta package. Each multi-arm trial was counted only once when reporting the total number of studies, whereas participants were counted according to their original randomised treatment arms.

Network meta-analysis estimates were presented as mean differences or risk ratios with 95% confidence intervals for all estimable treatment comparisons. Usual care was used as the common reference treatment in the principal presentation of the network estimates.

P scores and treatment ranks were calculated for each outcome and follow-up specific network as supplementary descriptive measures.[27] Higher P scores indicate more favourable relative rankings but do not represent the probability that an intervention is the best treatment.

All analyses were performed using the meta and netmeta packages in R.[28] Statistical tests were two sided, and P values <0.05 were considered statistically significant.

Transitivity and consistency assumptions

The plausibility of transitivity was assessed by comparing prespecified potential effect modifiers across direct comparisons within each outcome and follow-up specific network, including age, sex, primary versus secondary adhesive capsulitis, diabetes, baseline outcome values, and comparator and background-treatment characteristics. Disease duration and clinical stage were described but were not formally compared because reporting was incomplete or inconsistent. Because usual care was an important anchor node, its content and background-treatment balance were examined separately, including descriptive assessments of geographical and temporal variation.

At the comparison specific level, transitivity was assessed within the GRADE (Grading of Recommendations, Assessment, Development, and Evaluation) evaluation of indirect evidence by comparing baseline outcome values, mean age, and the proportion of male participants across the direct comparisons contributing to the dominant indirect pathway. Prespecified thresholds were the clinically important effect threshold for the relevant baseline outcome, 10 years for age, and 20 percentage points for the proportion of male participants. Exceeding these thresholds prompted further consideration but did not automatically lead to downgrading; indirect evidence was downgraded only when the observed differences

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were judged likely to materially affect the indirect estimate.

Global inconsistency was assessed using the design-by-treatment interaction model, and local inconsistency was evaluated using net splitting analyses comparing direct and indirect estimates.[29 30] When statistically significant inconsistency was identified, the original data and study characteristics were re-examined, and exploratory analyses were conducted after excluding studies considered potential contributors.

Small study effect evaluation

For conventional pairwise meta-analyses, potential small study effects were assessed when at least 10 studies contributed to the same direct treatment comparison for a given outcome and follow-up window. Funnel plots were visually inspected, and Egger's regression test was used to assess funnel plot asymmetry. These analyses were not performed for comparisons involving fewer than 10 studies because of the limited statistical power of asymmetry tests in small meta-analyses.

For each outcome and follow-up specific network meta-analysis, comparison adjusted funnel plots were generated to explore possible small study effects across treatment comparisons. In addition, exploratory meta-regression analyses were conducted using the total sample size of each study as a study level covariate to examine whether estimated treatment effects varied systematically according to study size.[31] These analyses were performed only when enough studies were available.

Sensitivity and meta-regression

Sensitivity analyses assessed the stability of findings by restricting analyses to intention-to-treat results, excluding studies at high risk of bias or involving secondary adhesive capsulitis or populations with ≥50% diabetes, excluding studies before 2010 with usual care comparators, restricting follow-up to 8-12 weeks, and retaining only single component interventions.

An additional exploratory sensitivity analysis assessed the influence of usual care content and implementation by retaining studies with structured usual care and broadly balanced background treatment; studies with minimal, inadequately described, or unbalanced usual care were excluded. Studies without a usual care comparator were retained.

Outcome specific sensitivity analyses were restricted to selected pain definitions, visual analogue scale measurements, the Shoulder Pain and Disability Index or Constant-Murley Score, the Shoulder Pain and Disability Index alone, and passive range of motion measurements. Sensitivity analyses also included analyses using standardised mean differences to assess the influence of pooling different measurement instruments. Separate sensitivity analyses using alternative baseline to follow-up correlations of r=0.25 and r=0.75, which were not prespecified in the protocol, were conducted to assess the robustness of the findings to the assumed correlation used for imputing missing standard deviations of change scores.

When sufficient studies were available, study level meta-regression examined baseline outcome values, mean age, the proportion of male participants, and sample size as potential sources of heterogeneity or small study effects. Exploratory subgroup analyses were conducted when meta-regression indicated a statistically significant association and clinically meaningful subgrouping was possible.

Minimal important effect thresholds and GRADE assessment

To support the clinical interpretation of treatment effects, prespecified minimal important effect thresholds were set at 1 cm on a 0-10 pain scale[32-34] and 10 points on a 0-100 function scale.[32 35 36] Because no patient anchored minimal important difference has been validated for range of motion or treatment related adverse events in patients with adhesive capsulitis, pragmatic clinically important effect thresholds of 10°[37 38] and 50 events per 1000 patients,[39-41] respectively, were used.

The certainty of direct, indirect, and network estimates was assessed separately using the GRADE approach.[42-49] Depending on the type of evidence and the circumstances of each comparison, the domains considered included risk of bias, indirectness, heterogeneity, publication bias, intransitivity, incoherence, and imprecision. The initial certainty of indirect evidence was based on the direct comparisons contributing to the dominant indirect pathway and was further downgraded for intransitivity when observed differences in effect modifiers were judged likely to materially affect the indirect estimate. Limitations already accounted for in the direct evidence were not downgraded again when assessing the indirect or network estimates.

Risk of bias judgments were made separately for each comparison, considering the contribution of studies at high risk of bias and the likely impact of identified limitations on the specific outcome.

Imprecision was assessed according to the position of the confidence interval relative to the null effect and prespecified decision thresholds, together with the amount of available information. For continuous outcomes, half of the corresponding clinically important effect threshold was used as the decision threshold, and 400 participants were used as a reference information size rather than an automatic downgrading criterion. For binary outcomes, the optimal information size was calculated using the prespecified clinically important absolute risk difference, control group event risk, a two sided α of 0.05, and 80% power. Failure to meet the reference information size alone did not result in downgrading; limited information was considered alongside the confidence interval and the number of contributing studies and participants.

Within the minimally contextualised framework, the full clinically important effect thresholds were used

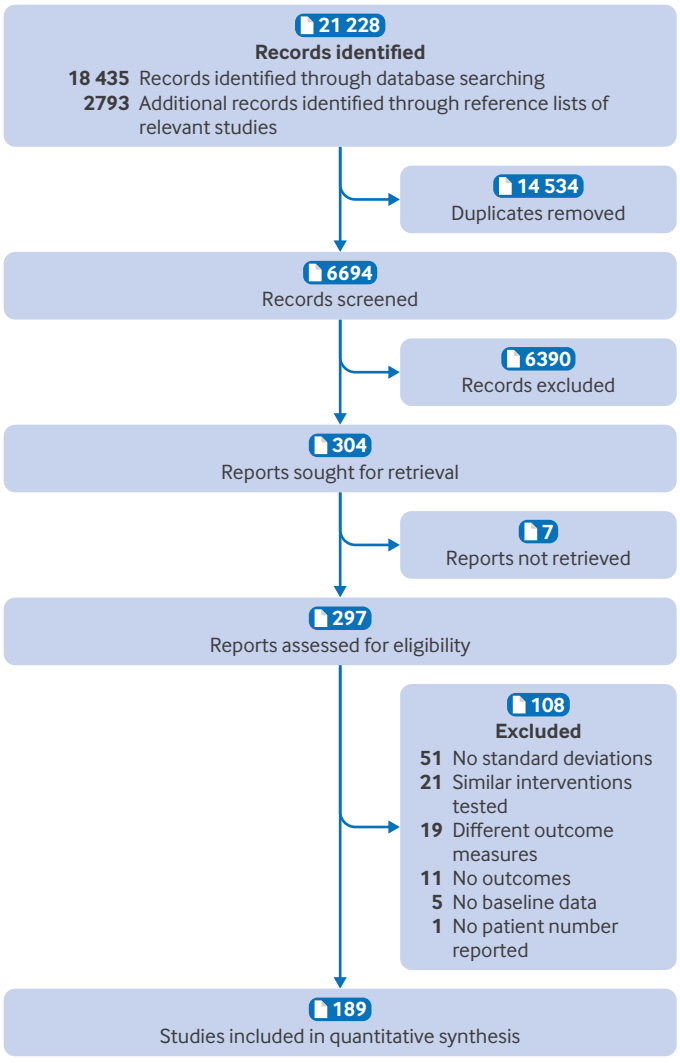


Fig 1 | PRISMA (Preferred Reporting Items for Systematic Reviews and Meta-Analyses) flow diagram of the study selection process

to classify the magnitude and clinical importance of treatment effects.[41]

Patient and public involvement

This study was a secondary analysis of published trial data. No patients or members of the public were involved in the design, conduct, reporting, or dissemination planning of the study. No dedicated funding or infrastructure for patient and public involvement was available for this project, and the study was initiated as a methodological evidence synthesis based entirely on published data. We acknowledge this as a limitation and recognise that future evidence synthesis in this area would benefit from public input, particularly in outcome prioritisation and dissemination.

Results

Study selection

The search identified 21 228 records, including 18 435 records from electronic databases and 2793 additional records from reference lists. After duplicate removal, 6694 records underwent title and abstract screening. Of 304 reports sought for retrieval, seven could not be obtained despite attempts to contact the authors (appendix 2). The remaining 297 full text reports were assessed for eligibility, of which 108 were excluded. Overall, 189 independent randomised controlled trials involving 12 988 participants and 59 intervention nodes were included (fig 1).

Study characteristics

All included studies used parallel group designs. The median trial sample size was 60 participants (interquartile range 40-72), and the median of the reported mean ages was 54 years (interquartile range 52-56). At baseline, the median pain score was 7.1 on a 0-10 scale and the median function score was 63 on a 0-100 scale after harmonisation. Median baseline range of motion was 105° for flexion, 85° for abduction, 33° for external rotation, and 40° for internal rotation, indicating substantial restriction across movement planes (table 1; appendix 3).

Most studies predominantly enrolled participants with primary adhesive capsulitis (87%); three studies (2%) focused specifically on secondary adhesive capsulitis. Eight studies (4%) specifically enrolled participants with diabetes, and five studies (3%) included populations in which at least half of the participants had diabetes. Among studies reporting disease stage, early or freezing stage, frozen stage, or mixed stage populations were most common, although stage was frequently unreported or inconsistently defined. The evidence was derived mainly from small, single centre trials and 78% of studies were conducted in Asia (appendix 3).

Transitivity and comparator characteristics

The transitivity assumption was considered broadly plausible, although it could not be fully verified for all network links. Mean age and sex distribution were reasonably comparable across many direct comparisons, whereas baseline pain, function, range of motion, and sex distribution differed appreciably for some links. Disease duration and clinical stage could not be compared reliably across direct comparisons because these variables were frequently missing, reported only as eligibility ranges, or defined inconsistently. Several links were also informed by only one or a few studies, and incomplete reporting of baseline characteristics further limited the assessment of some comparisons (appendix 4).

Usual care was used as a comparator in 60 studies and represented a heterogeneous pragmatic node encompassing multimodal, exercise based, physiotherapy based, analgesic or other conservative, and minimal or unclear care. Most studies with a usual care comparator were conducted in Asia (51/60, 85%), and background treatment was broadly balanced in 44/60 (73%) studies. Appendix 4 provides detailed geographical and temporal distributions of usual care characteristics.

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Table 1 | Characteristics of included randomised controlled trials and study populations (n=189)

Characteristics	No (%) of studies or median (interquartile range)
Sample size	60 (40-72)
Age (years)	54 (52-56)
Women (%)	62 (53-68)
Follow-up duration (weeks)	12 (6-24)
Baseline pain intensity (10 cm visual analogue scale)	7.1 (6.2-7.7)
Baseline function score (0-100)	63 (53-73)
Baseline range of motion (degrees)	
External rotation (normal range 0-90°)	33 (23-43)
Abduction (normal range 0-180°)	85 (72-98)
Flexion (normal range 0-180°)	105 (87-124)
Internal rotation (normal range 0-70°)	40 (31-48)
Year of publication	
2000 or earlier	2 (1)
2001-2005	8 (4)
2006-2010	13 (7)
2011-2015	26 (14)
2016-2020	45 (24)
2021-2025	95 (50)
Country or region	
Asia	147 (78)
Europe	18 (9)
Africa	12 (6)
North America	5 (3)
Australia	5 (3)
South America	2 (1)
Duration of pain	
<3 months	37 (20)
3-6 months	51 (27)
Not specified	101 (53)
Type of adhesive capsulitis	
Primary adhesive capsulitis	165 (87)
Secondary adhesive capsulitis	3 (2)
Not specified	21 (11)
Comorbidities	
Diabetes mellitus (>50% of patients)	13 (7)
Not specified	176 (93)
Scales used for measuring pain	
Visual analogue scale	135 (93)
Numerical pain rating scale	11 (7)
Scales used for measuring physical function	
Shoulder Pain and Disability Index	85 (57)
Constant-Murley Score	29 (20)
Other type	35 (23)
Type of range of motion assessment	
Active	20 (14)
Passive	71 (49)
Not specified	55 (37)
Reported funding	
Non-industry funding	38 (20)
Unfunded	41 (22)
No funding statement	110 (58)

Risk of bias

Risk of bias was frequently high, particularly for patient reported outcomes. Overall, 103 of 146 studies (71%) contributing pain outcomes, 108 of 149 studies (72%) contributing function outcomes, and 104 of 146 studies (71%) contributing range of motion outcomes were judged to be at high overall risk of bias. Lack of participant or assessor blinding, deviations from intended interventions, and concerns about outcome measurement were common (appendix 5).

Direct pairwise meta-analyses and agreement between direct and indirect evidence

At short term follow-up, 35 direct comparisons for pain and 38 direct comparisons for function were supported by at least two studies. Six pain comparisons and nine function comparisons were supported by at least five studies (table 2). Within these better informed comparisons, direct and indirect estimates were generally similar in direction and magnitude, and none showed statistically significant disagreement in net splitting analyses. Appendix 6 reports complete pooled direct estimates and forest plots.

Across all estimable comparisons, local inconsistency was detected in several short term networks, including comparisons of extracorporeal shockwave therapy with usual care for function, flexion, and abduction, and comparisons of intra-articular steroid and passive mobilisation with usual care for flexion. No statistically significant local inconsistency involving usual care was detected for short term pain or external rotation.

Exploratory exclusion analyses removing a small number of studies considered potential contributors attenuated some inconsistency signals. After these exclusions, the global inconsistency tests were no longer statistically significant for short term function (P=0.13) or short term flexion (P=0.15). However, the restricted networks contained less evidence and may therefore have had reduced power to detect inconsistency. The non-significant tests should not be interpreted as showing that the original networks were coherent. Some effect estimates also changed after exclusion, indicating sensitivity to the included evidence.

Because these analyses were post hoc and excluded otherwise eligible studies, the prespecified primary analyses were retained as the main results. The exclusion analyses were used only to explore potential sources of inconsistency and assess the stability of the findings rather than to establish that the original networks satisfied the consistency assumption. Where statistically significant disagreement between direct and indirect evidence was identified, the certainty of the corresponding network estimate was downgraded for incoherence in the comparison specific GRADE assessment. Appendices 7 and 8 report complete net splitting and exploratory exclusion analysis results.

Pain

A total of 146 trials involving 10 064 participants evaluated 50 interventions for pain. Figure 2 shows the network geometry for pain at short term follow-up and appendix 9 provides network plots for the remaining outcomes and follow-up periods. For each principal outcome and follow-up period, appendix 10 reports the number of contributing studies and participants for each intervention node, alongside the corresponding network estimates and certainty of evidence; direct pairwise evidence is reported separately in appendix 6. No statistically significant

Table 2 | Direct, indirect, and network estimates for short term pain and function comparisons supported by at least five studies

Outcome and comparison	No of studies	No of participants	Direct estimate	Indirect estimate	P value	Network estimate
Pain at ≤3 months						
Intra-articular steroid *v* placebo	7	305	-1.15 (-2.20 to -0.11)	-0.85 (-1.67 to -0.04)	0.66	-0.97 (-1.61 to -0.32)
Intra-articular steroid *v* usual care	5	275	-0.87 (-1.99 to 0.25)	-0.67 (-1.41 to 0.07)	0.77	-0.73 (-1.35 to -0.11)
Suprascapular nerve block *v* usual care	5	532	-0.96 (-2.08 to 0.15)	-0.38 (-1.41 to 0.64)	0.45	-0.65 (-1.40 to 0.11)
Passive mobilisation *v* usual care	14	573	-1.48 (-2.18 to -0.79)	-0.69 (-1.79 to 0.41)	0.24	-1.26 (-1.85 to -0.67)
Intra-articular steroid *v* suprascapular nerve block	5	333	-0.42 (-1.59 to 0.74)	0.16 (-0.82 to 1.13)	0.45	-0.08 (-0.83 to 0.67)
Intra-articular steroid *v* intra-articular platelet rich plasma	8	769	0.71 (-0.18 to 1.61)	0.41 (-1.29 to 2.11)	0.76	0.65 (-0.14 to 1.44)
Function at ≤3 months						
Intra-articular steroid *v* placebo	6	265	-14.79 (-22.94 to -6.65)	-13.10 (-18.50 to -7.70)	0.73	-13.61 (-18.11 to -9.11)
Intra-articular steroid *v* usual care	8	448	-6.87 (-12.53 to -1.21)	-1.34 (-6.06 to 3.37)	0.14	-3.61 (-7.23 to 0.02)
Suprascapular nerve block *v* usual care	5	532	-7.21 (-14.19 to -0.24)	-2.09 (-8.11 to 3.94)	0.28	-4.28 (-8.84 to 0.28)
Passive mobilisation *v* usual care	12	421	-8.06 (-12.76 to -3.37)	-0.10 (-8.43 to 8.22)	0.1	-6.14 (-10.23 to -2.05)
Intra-articular steroid *v* suprascapular nerve block	7	481	1.04 (-5.15 to 7.24)	0.31 (-5.80 to 6.42)	0.87	0.67 (-3.68 to 5.02)
Intra-articular steroid *v* intra-articular platelet rich plasma	6	641	8.71 (2.29 to 15.13)	0.75 (-7.95 to 9.45)	0.15	5.91 (0.74 to 11.07)
Intra-articular steroid *v* intra-articular hyaluronic acid	5	207	0.40 (-7.67 to 8.46)	-7.35 (-20.58 to 5.88)	0.33	-1.70 (-8.59 to 5.19)
Intra-articular steroid *v* hydrodilatation	5	259	2.99 (-4.77 to 10.75)	-0.83 (-8.82 to 7.16)	0.5	1.13 (-4.43 to 6.70)
Intra-articular steroid *v* intra-articular steroid and hyaluronic acid	5	377	2.19 (-5.38 to 9.76)	6.28 (-2.18 to 14.74)	0.48	4.01 (-1.63 to 9.65)

Estimates presented as mean differences with 95% confidence intervals (CIs). Pain outcomes expressed on a 0-10 scale and function outcomes on a 0-100 scale. Negative mean differences favour first named treatment whereas positive mean differences favour second named treatment. Numbers of studies and participants refer to direct evidence contributing to each comparison. P values obtained from net splitting analyses comparing corresponding direct and indirect estimates. Only direct comparisons supported by at least five studies are presented.

global inconsistency was detected at short term or mid-term follow-up (appendix 7).

At short term follow-up, moderate certainty evidence suggested that extracorporeal shockwave therapy reduced pain compared with usual care (eight studies, 464 participants; mean difference -1.97 on a 0-10 scale, 95% confidence interval -2.97 to -0.97). Low certainty evidence suggested a smaller benefit with passive mobilisation (24 studies, 1302 participants; -1.26, -1.85 to -0.67), although the confidence interval included effects below the prespecified one point threshold for clinical importance. Estimates for the remaining interventions were supported by low or very low certainty evidence (fig 3; appendix 10).

At mid-term follow-up, extracorporeal shockwave therapy was associated with lower pain (two studies, 162 participants; -3.02, -5.27 to -0.76), but the certainty of evidence was very low because the estimate was based entirely on sparse indirect evidence and limited information. No mid-term pain comparison was supported by high or moderate certainty evidence (appendices 10 and 11).

Shoulder function

A total of 149 trials involving 10 149 participants evaluated 52 interventions for shoulder function. Figure 4 shows the network geometry for function at short term follow-up. Global inconsistency was detected at short term follow-up (P=0.001) but not at mid-term follow-up (P=0.95). After a small number of studies considered potential contributors to local inconsistency were excluded in an exploratory analysis, the short term global inconsistency test was no longer statistically significant (P=0.13). This finding was not interpreted as evidence that coherence had been restored. The primary analysis was therefore retained, and the exclusion analysis was used only to explore potential sources of inconsistency and assess the stability of the findings (appendices 7 and 8).

At short term follow-up, low certainty evidence suggested that extracorporeal shockwave therapy may improve function compared with usual care (12 studies, 663 participants; mean difference -11.42, 95% CI -16.68 to -6.15). However, the direct and indirect estimates were importantly incoherent (P=0.001): the direct evidence suggested little or no important difference, whereas the indirect evidence suggested a clinically important benefit. After assessment of imprecision, the direct evidence was rated as very low certainty, while both the indirect evidence and the network estimate were rated as low certainty; the network estimate was therefore selected as the best estimate.

At short term follow-up, proprioceptive neuromuscular facilitation also produced a favourable network estimate (six studies, 265 participants; mean difference -12.26, 95% CI -18.99 to -5.52), but the certainty of evidence was very low. Estimates for the remaining interventions were also supported predominantly by low or very low certainty evidence (fig 5; appendix 10).

At mid-term follow-up, low certainty evidence suggested that extracorporeal shockwave therapy may improve function compared with usual care (three studies, 205 participants; mean difference -19.12, 95% CI -34.72 to -3.52). However, this estimate was based on a small number of participants and had a wide confidence interval. The remaining mid-term estimates were supported by very low certainty evidence and were therefore highly uncertain (appendices 10 and 11).

Range of motion

A total of 146 trials involving 9508 participants evaluated 56 interventions for range of motion.

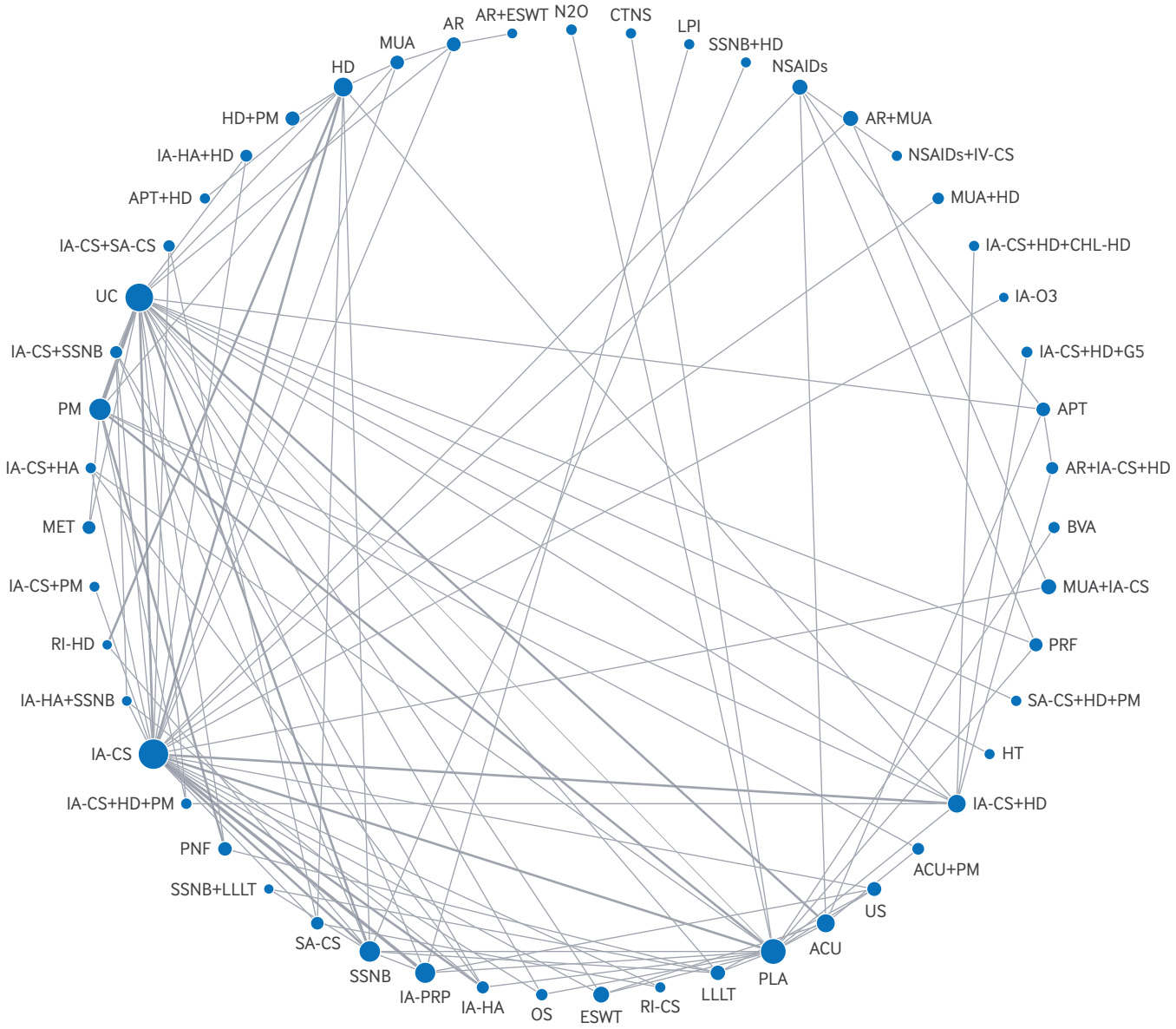
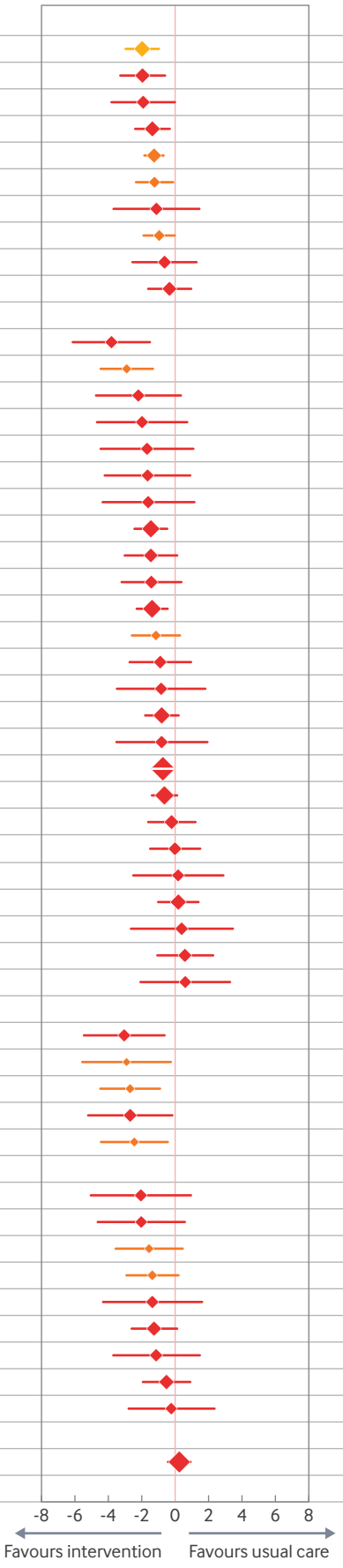


Fig 2 | Network plot for pain (up to three months). Lines connect interventions that were directly compared in eligible studies. Node size is proportional to number of participants assigned to each intervention and edge thickness is proportional to number of studies directly comparing the connected interventions. ACU=acupuncture; APT=acupotomy; AR=arthroscopic release; BVA=bee venom acupuncture; CHL-HD=coracohumeral ligament hydrodilatation; CS=corticosteroid; CTNS=calcitonin nasal spray; ESWT=extracorporeal shockwave therapy; G5=5% glucose; HA=hyaluronic acid; HD=hydrodilatation; IA=intra-articular; IV=intravenous; LLLT=low level laser therapy; LPI=local procaine injection; MET=muscle energy technique; MUA=manipulation under anaesthesia; N2O=nitrous oxide; NSAID=non-steroidal anti-inflammatory drug; O3=ozone; OS=oral corticosteroid; PLA=placebo; PM=passive mobilisation; PNF=proprioceptive neuromuscular facilitation; PRF=pulsed radiofrequency; PRP=platelet rich plasma; RI=rotator interval; SA=subacromial; SSNB=suprascapular nerve block; UC=usual care; US=ultrasound therapy. Plus sign indicates combination of interventions. Hyphen links route, anatomical site, or treatment qualifier to corresponding intervention

Seventy one studies reported passive range of motion measurements, 20 reported active measurements, and 55 did not specify whether the measurements were active or passive. Global inconsistency was detected for flexion at short term (P=0.01) and mid-term (P=0.003) follow-up, but not for the other movement planes. After exploratory analyses excluding a small number of studies considered potential contributors, the global inconsistency tests for flexion were no longer statistically significant at short term or mid-term follow-up (P=0.15 and P=0.26, respectively). The non-significant tests in these restricted networks were therefore interpreted as stability analyses rather than evidence that the original inconsistency had been resolved. The primary analyses were therefore retained, and the exclusion analyses were used only to explore potential sources of inconsistency and assess the stability of the findings (appendices 7 and 8).

Intervention	No of participants	No of trials	Mean difference (95%CI)	Mean difference (95%CI)
Conservative				
Extracorporeal shockwave therapy	464	8		-1.97 (-2.97 to -0.97)
Muscle energy technique	192	4		-1.95 (-3.28 to -0.61)
Acupuncture and passive mobilisation	131	2		-1.92 (-3.81 to -0.02)
Proprioceptive neuromuscular facilitation	276	6		-1.37 (-2.41 to -0.33)
Passive mobilisation	1302	24		-1.26 (-1.85 to -0.67)
Low level laser therapy	357	7		-1.24 (-2.35 to -0.13)
Heat therapy	60	1		-1.12 (-3.68 to 1.44)
Acupuncture	843	9		-0.96 (-1.89 to -0.02)
Bee venom acupuncture	106	2		-0.64 (-2.56 to 1.28)
Ultrasound therapy	359	5		-0.33 (-1.63 to 0.96)
Injection or procedure				
Rotator interval steroids	77	1		-3.82 (-6.13 to -1.51)
Pulsed radiofrequency	218	3		-2.90 (-4.46 to -1.34)
Rotator interval hydrodilatation	54	1		-2.20 (-4.74 to 0.34)
Intra-articular ozone	40	1		-1.98 (-4.67 to 0.71)
NSAIDs and intravenous steroid	60	1		-1.69 (-4.46 to 1.08)
Calcitonin nasal spray	64	1		-1.66 (-4.22 to 0.89)
Nitrous oxide inhalation	69	1		-1.61 (-4.35 to 1.13)
Hydrodilatation	923	12		-1.46 (-2.45 to -0.47)
Intra-articular steroid and hyaluronic acid	120	3		-1.45 (-3.02 to 0.11)
Intra-articular and subacromial steroids	164	2		-1.43 (-3.21 to 0.36)
Intra-articular platelet rich plasma	1080	13		-1.38 (-2.30 to -0.46)
Intra-articular steroid and SSNB	184	3		-1.15 (-2.59 to 0.29)
Intra-articular hyaluronic acid and HD	152	2		-0.89 (-2.73 to 0.95)
Local procaine injection	60	1		-0.84 (-3.48 to 1.80)
Intra-articular steroid and HD	752	11		-0.81 (-1.80 to 0.19)
Intra-articular steroid, HD, and 5% glucose	100	1		-0.81 (-3.52 to 1.91)
Intra-articular steroid	3775	48		-0.73 (-1.35 to -0.11)
Suprascapular nerve block	1246	17		-0.65 (-1.40 to 0.11)
Intra-articular hyaluronic acid	248	4		-0.21 (-1.62 to 1.21)
Subacromial steroids	273	3		-0.01 (-1.51 to 1.49)
SSNB and HD	48	1		0.17 (-2.51 to 2.86)
NSAIDs	396	7		0.18 (-1.02 to 1.38)
Intra-articular steroid, HD, and CHL-HD	63	1		0.39 (-2.65 to 3.43)
Oral steroids	118	3		0.59 (-1.08 to 2.26)
Intra-articular hyaluronic acid and SSNB	80	1		0.60 (-2.07 to 3.27)
Combined				
SSNB and LLLT	55	1		-3.05 (-5.46 to -0.64)
Subacromial steroid, HD, and PM	70	1		-2.91 (-5.56 to -0.26)
Intra-articular steroid, HD, and PM	85	2		-2.70 (-4.49 to -0.91)
Intra-articular steroid and PM	51	2		-2.69 (-5.21 to -0.17)
HD and PM	301	2		-2.45 (-4.45 to -0.44)
Surgical or release				
AR and extracorporeal shockwave therapy	46	1		-2.06 (-5.05 to 0.93)
MUA and HD	121	1		-2.03 (-4.63 to 0.57)
AR, intra-articular steroid, and HD	140	2		-1.56 (-3.56 to 0.44)
Arthroscopic release	289	4		-1.37 (-2.92 to 0.18)
Acupotomy and HD	66	1		-1.36 (-4.32 to 1.60)
Manipulation under anaesthesia	297	4		-1.26 (-2.62 to 0.11)
AR and MUA	503	1		-1.13 (-3.71 to 1.45)
Acupotomy	279	4		-0.52 (-1.94 to 0.90)
MUA and intra-articular steroid	503	1		-0.23 (-2.80 to 2.34)
Control				
Placebo	2184	33		0.24 (-0.44 to 0.91)



Certainty of the evidence ◆ Moderate ◆ Low ◆ Very low

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Fig 3 | Network meta-analysis estimates of interventions compared with usual care for pain (up to three months). Estimates are presented as mean differences with 95% confidence intervals (CIs). Negative mean differences favour intervention whereas positive mean differences favour usual care. The vertical line at zero represents no difference between intervention and usual care. Interventions are grouped according to treatment category. For each intervention, number of participants represents total number randomised across all eligible arms of studies that included that intervention rather than only the number allocated to intervention arm. AR=arthroscopic release; CHL-HD=coracohumeral ligament hydrodilatation; HD=hydrodilatation; LLLT=low level laser therapy; MUA=manipulation under anaesthesia; NSAID=non-steroidal anti-inflammatory drug; PM=passive mobilisation; SSNB=suprascapular nerve block

At short term follow-up, moderate certainty evidence suggested that manipulation under anaesthesia combined with intra-articular steroid injections improved flexion compared with usual care (three studies, 336 participants; mean difference 33.29°, 95% CI 11.19° to 55.39°), while manipulation under anaesthesia alone also improved flexion (five studies, 554 participants; mean difference 26.16°, 95% CI 8.19° to 44.13°). Low certainty evidence suggested that extracorporeal shockwave therapy may improve flexion (eight studies, 365 participants; mean difference 22.42°, 95% CI 12.99° to 31.84°) and abduction (11 studies, 550 participants; mean difference 18.62°, 95% CI 8.09° to 29.14°). For both comparisons, important incoherence was identified between the direct and indirect estimates. After separate assessments of imprecision, the indirect and network estimates had the joint highest final certainty; the network estimates were therefore retained as the best estimates and were rated as low certainty. Estimates for extension, external rotation, internal rotation, and most other interventions were supported by low or very low certainty evidence (fig 6; appendix 10).

At mid-term follow-up, the evidence was limited and unstable. Low certainty evidence suggested that passive mobilisation may improve flexion compared with usual care (four studies, 320 participants; mean difference 21.80°, 95% CI 12.61° to 30.99°), whereas the remaining range of motion estimates were supported by low or very low certainty evidence (appendices 10 and 11). Internal rotation at mid-term follow-up was not analysed because the network included fewer than 10 studies.

For the local inconsistency identified in the flexion, abduction, and external rotation networks, exploratory analyses were conducted after excluding studies considered potential contributors. Sensitivity analyses restricted to passive range of motion measurements were also performed to assess the possible influence of measurement type. Although the direction of several effect estimates was generally preserved, some effect sizes and treatment rankings changed as the networks became smaller. These analyses were therefore used only to explore potential sources of inconsistency and assess the stability of the findings and were not considered to confirm the robustness of the primary results (appendices 7 and 8).

Adverse events and additional outcomes

Only 30 of the 189 trials, involving 2658 participants and 23 interventions, reported adverse event data suitable for quantitative analysis. No statistically significant global inconsistency was detected (P=0.06), but all comparative safety estimates were rated as very low certainty, and most confidence intervals were extremely wide (appendices 7, 10, and 11). Health related quality of life data were too sparse and heterogeneous to form a connected network and are summarised descriptively in appendix 12. Comparison adjusted funnel plots and exploratory meta-regression analyses did not show a consistent pattern of small study effects or effect modification, although isolated associations were observed and these analyses had limited power because many comparisons were sparse (appendices 13 and 14).

Stability varied across the principal comparisons and analytical restrictions. For short term pain, the estimated effect of extracorporeal shockwave therapy remained broadly similar under the alternative baseline to follow-up correlation assumptions, the structured usual care restriction, and the visual analogue scale only analysis, with greater variation observed after exclusion of studies at high risk of bias. For short term function, the estimated effect of extracorporeal shockwave therapy was more sensitive to analytical restrictions, particularly in the intention-to-treat only and lower risk of bias analyses; greater variation was also observed in the Shoulder Pain and Disability Index only analysis, which included fewer studies and treatment nodes and resulted in a smaller, structurally different network. For short term flexion, manipulation under anaesthesia with or without intra-articular steroid injections remained broadly similar under the alternative correlation and structured usual care analyses when estimable, although these interventions could not be estimated in some more restricted networks. Extracorporeal shockwave therapy for short term flexion and abduction showed a similar pattern, remaining broadly similar under the alternative correlation and structured usual care analyses, with greater variation observed in some more restrictive analyses. These analyses therefore indicated differential sensitivity across comparisons rather than consistent stability across all principal findings. Non-estimability in restricted networks was not interpreted as confirmation or contradiction of the primary findings (appendix 8).

Discussion

Principal findings

Three findings are most relevant to clinical practice. Firstly, moderate certainty evidence suggests that extracorporeal shockwave therapy probably

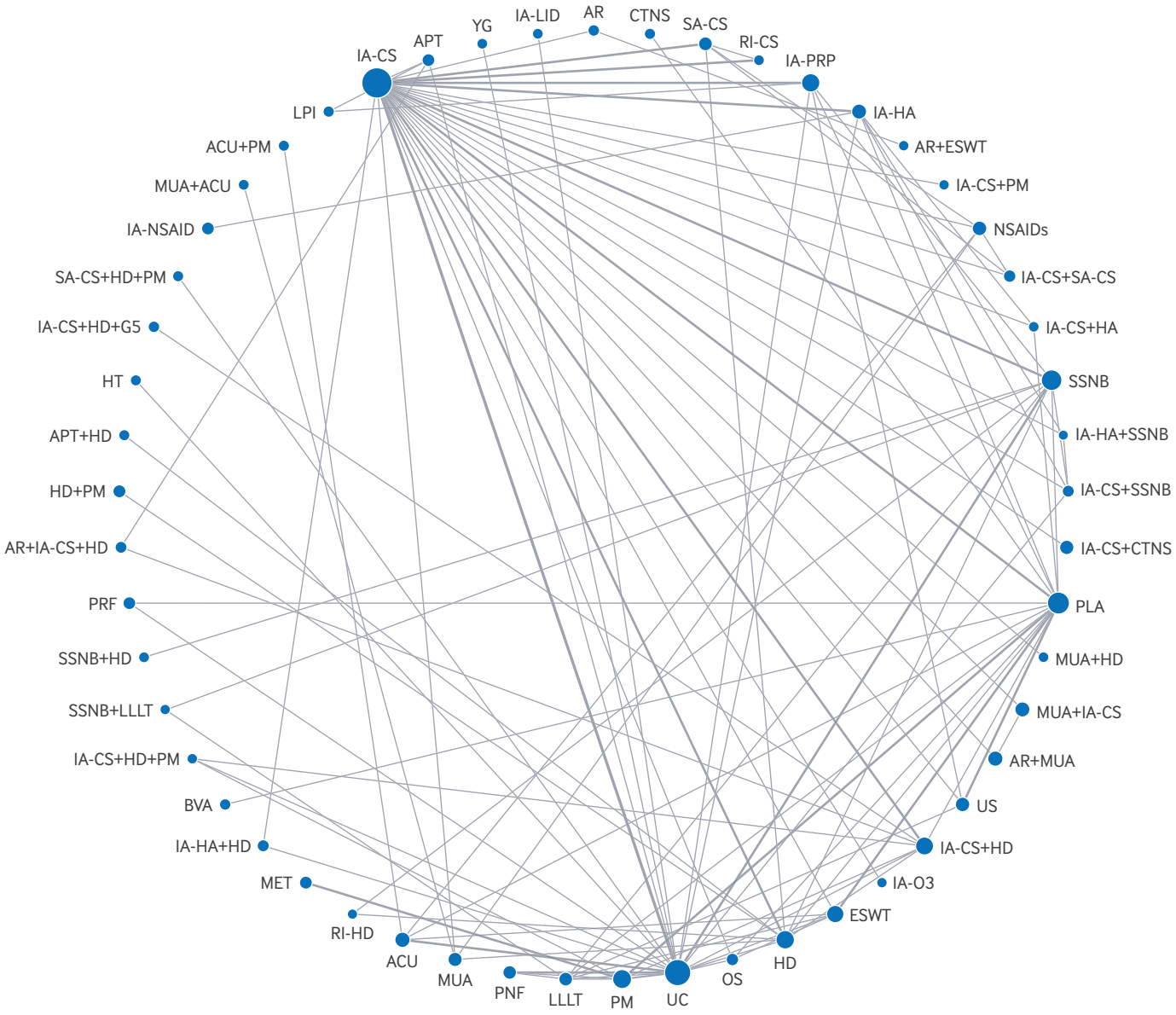


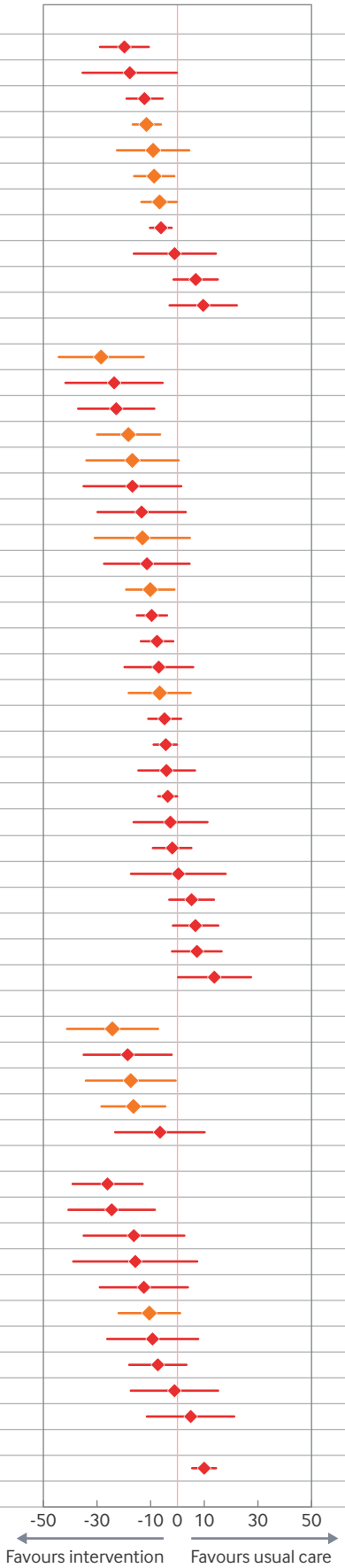
Fig 4 | Network plot for shoulder function (up to three months). Lines connect interventions that were directly compared in eligible studies. Node size is proportional to number of participants assigned to each intervention and edge thickness is proportional to number of studies directly comparing connected interventions. ACU=acupuncture; APT=acupotomy; AR=arthroscopic release; BVA=bee venom acupuncture; CS=corticosteroid; CTNS=calcitonin nasal spray; ESWT=extracorporeal shockwave therapy; G5=5% glucose; HA=hyaluronic acid; HD=hydrodilatation; IA=intra-articular; LID=lidocaine; LLLT=low level laser therapy; LPI=local procaine injection; MET=muscle energy technique; MUA=manipulation under anaesthesia; NSAID=non-steroidal anti-inflammatory drug; O3=ozone; OS=oral corticosteroid; PLA=placebo; PM=passive mobilisation; PNF=proprioceptive neuromuscular facilitation; PRF=pulsed radiofrequency; PRP=platelet rich plasma; RI=rotator interval; SA=subacromial; SSNB=suprascapular nerve block; UC=usual care; US=ultrasound therapy; YG=yoga. Plus sign indicates combination of interventions. Hyphen links route, anatomical site, or treatment qualifier to corresponding intervention and does not indicate subtraction

reduces short term pain compared with usual care, with a point estimate exceeding the prespecified threshold for clinical importance. Secondly, moderate certainty evidence suggests that manipulation under anaesthesia, with or without intra-articular steroid injections, probably improves short term flexion. Thirdly, apparent benefits of extracorporeal shockwave therapy for short term function, flexion, and abduction are less certain because these estimates were supported by low certainty evidence and showed important incoherence between direct and indirect evidence.

Beyond these findings, the evidence was substantially less informative. At mid-term follow-up, low certainty evidence suggested possible improvements in function with extracorporeal shockwave therapy and in flexion with passive mobilisation, while the apparent pain benefit of extracorporeal shockwave therapy was supported by very low certainty evidence. These

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Intervention	No of participants	No of trials	Mean difference (95%CI)	Mean difference (95%CI)
Conservative				
Muscle energy technique	192	4		-19.75 (-28.88 to -10.62)
Acupuncture and passive mobilisation	61	1		-17.74 (-35.32 to -0.16)
Proprioceptive neuromuscular facilitation	265	6		-12.26 (-18.99 to -5.52)
Extracorporeal shockwave therapy	663	12		-11.42 (-16.68 to -6.15)
Heat therapy	80	2		-9.02 (-22.51 to 4.47)
Low level laser therapy	304	6		-8.57 (-16.06 to -1.09)
Acupuncture	395	8		-6.64 (-13.47 to 0.19)
Passive mobilisation	1022	21		-6.14 (-10.23 to -2.05)
Yoga	72	1		-0.98 (-16.29 to 14.34)
Ultrasound therapy	359	5		6.82 (-1.44 to 15.09)
Bee venom acupuncture	106	2		9.60 (-2.92 to 22.13)
Injection or procedure				
Intra-articular steroid and calcitonin nasal spray	391	1		-28.38 (-44.21 to -12.54)
Rotator interval hydrodilatation	62	1		-23.63 (-41.81 to -5.45)
Rotator interval steroids	77	1		-22.83 (-37.05 to -8.61)
Pulsed radiofrequency	178	2		-18.23 (-30.00 to -6.47)
Intra-articular NSAIDs	160	1		-16.80 (-34.00 to 0.41)
Intra-articular ozone	40	1		-16.80 (-35.02 to 1.42)
Intra-articular steroid, HD, and 5% glucose	100	1		-13.31 (-29.77 to 3.14)
Intra-articular lidocaine	66	1		-13.04 (-30.78 to 4.70)
Calcitonin nasal spray	64	1		-11.39 (-27.34 to 4.56)
Intra-articular steroid and SSNB	184	3		-10.08 (-19.09 to -1.07)
Intra-articular platelet rich plasma	952	11		-9.51 (-15.14 to -3.89)
Intra-articular steroid and HD	868	12		-7.61 (-13.67 to -1.56)
Intra-articular steroid and hyaluronic acid	100	2		-6.91 (-19.71 to 5.88)
Intra-articular hyaluronic acid and HD	152	2		-6.65 (-18.15 to 4.85)
Hydrodilatation	944	13		-4.74 (-10.87 to 1.38)
Suprascapular nerve block	1367	18		-4.28 (-8.84 to 0.28)
Intra-articular and subacromial steroids	268	2		-4.02 (-14.61 to 6.57)
Intra-articular steroid	4510	51		-3.61 (-7.23 to 0.02)
Intra-articular hyaluronic acid and SSNB	80	1		-2.60 (-16.32 to 11.13)
Intra-articular hyaluronic acid	541	7		-1.91 (-9.05 to 5.24)
SSNB and HD	48	1		0.35 (-17.27 to 17.97)
Subacromial steroids	431	4		5.30 (-3.03 to 13.63)
NSAIDs	432	4		6.74 (-1.71 to 15.19)
Oral steroids	158	4		7.28 (-1.97 to 16.53)
Local procaine injection	99	2		13.75 (0.05 to 27.45)
Combined				
Subacromial steroid, HD, and PM	70	1		-24.20 (-41.18 to -7.22)
SSNB and LLLT	55	1		-18.52 (-34.95 to -2.10)
HD and PM	180	1		-17.37 (-34.12 to -0.62)
Intra-articular steroid, HD, and PM	85	2		-16.39 (-28.32 to -4.45)
Intra-articular steroid and PM	51	2		-6.53 (-23.21 to 10.16)
Surgical or release				
AR, intra-articular steroid, and HD	140	2		-26.03 (-39.12 to -12.93)
MUA and HD	56	1		-24.51 (-40.66 to -8.36)
MUA and acupuncture	72	1		-16.19 (-34.95 to 2.56)
AR and ESWT	46	1		-15.72 (-38.87 to 7.43)
Arthroscopic release	102	2		-12.51 (-28.90 to 3.88)
Acupotomy	179	3		-10.45 (-21.93 to 1.03)
Acupotomy and HD	66	1		-9.24 (-26.23 to 7.74)
Manipulation under anaesthesia	317	4		-7.31 (-18.04 to 3.42)
MUA and intra-articular steroid	503	1		-1.11 (-17.40 to 15.18)
AR and MUA	503	1		4.93 (-11.36 to 21.23)
Control interventions				
Placebo	1610	29		10.01 (5.47 to 14.54)



Certainty of the evidence ◆ Moderate ◆ Low ◆ Very low

Fig 5 | Network meta-analysis estimates of interventions compared with usual care for function (up to three months). Estimates are presented as mean differences with 95% confidence intervals (CIs). Negative mean differences favour intervention whereas positive mean differences favour usual care. The vertical line at zero represents no difference between intervention and usual care. Interventions are grouped according to treatment category. For each intervention, number of participants represents total number randomised across all eligible arms of studies that included that intervention rather than only the number allocated to intervention arm. AR=arthroscopic release; ESWT=extracorporeal shockwave therapy; HD=hydrodilatation; LLLT=low level laser therapy; MUA=manipulation under anaesthesia; NSAID=non-steroidal anti-inflammatory drug; PM=passive mobilisation; SSNB=suprascapular nerve block

estimates were based on only a small number of studies and participants and should therefore be regarded as tentative rather than established comparative benefits. No principal mid-term finding was supported by high or moderate certainty evidence. Evidence for most other interventions was of low or very low certainty, and comparative safety remained highly uncertain because adverse events were incompletely and inconsistently reported. Accordingly, the available evidence does not support a definitive overall treatment hierarchy.

Stability varied across the principal comparisons and analytical restrictions. The short term pain finding for extracorporeal shockwave therapy remained broadly similar across several major sensitivity analyses but showed greater changes after exclusion of studies at high risk of bias, whereas the short term function finding was more sensitive to analytical restrictions. Findings for manipulation under anaesthesia, with or without intra-articular steroid injections, and for extracorporeal shockwave therapy on short term range of motion were broadly similar in some analyses when estimable, but some interventions became non-estimable or showed greater changes in more restricted networks. Therefore, the primary analyses remained the basis for interpretation.

Clinical interpretation

The short term findings are clinically relevant because pain, sleep disturbance, restricted movement, and limitations in daily activities are prominent during the symptomatic course of adhesive capsulitis.[50] For extracorporeal shockwave therapy, the point estimate for short term pain reduction exceeded the prespecified one point threshold for clinical importance, although the confidence interval marginally included effects below this threshold. The network estimate for short term function also exceeded the prespecified 10 point threshold, but its interpretation requires greater caution. Direct evidence suggested little or no important functional difference, whereas indirect evidence suggested a clinically important benefit. Although the network estimate was retained as the best estimate under the GRADE framework, the important incoherence resulted in low certainty. The range of motion findings also require careful interpretation. Low certainty evidence suggested that extracorporeal shockwave therapy may improve short term flexion and abduction, but the direct and indirect estimates were importantly incoherent for both outcomes. These findings should therefore not be interpreted as establishing a reliable improvement in shoulder movement, particularly because measured range of motion does not necessarily correspond to pain-free active movement, daily function, or quality of life.[6 51] Therefore, the most credible evidence for extracorporeal shockwave therapy relates to short term pain reduction; its apparent benefits for function and range of motion are less certain.

Moderate certainty estimates for manipulation under anaesthesia and manipulation under anaesthesia combined with intra-articular steroid injections related primarily to short term flexion. For the combined intervention, the entire confidence interval exceeded the prespecified 10° threshold for clinical importance. Nevertheless, these results do not establish that invasive treatment is preferable to less invasive options because evidence concerning pain, function, longer term recovery, and harms was limited or uncertain. Treatment decisions should therefore consider the overall benefit-harm profile, invasiveness, resource requirements, costs, patient preferences, and the availability of experienced clinicians rather than relying on a single range of motion outcome or treatment ranking.[52 53]

The apparent mid-term improvement in flexion with passive mobilisation was supported by low certainty evidence and was not accompanied by similarly reliable evidence of improvement in pain or function. Because the primary range of motion analyses combined passive, active, and unspecified measurements, improvement in measured flexion should not be assumed to represent an equivalent improvement in pain-free functional movement, daily function, or quality of life.[50 54] Therefore, this isolated finding should be interpreted cautiously and should not, by itself, be used to support passive mobilisation over other treatment options.

Combination nodes should be interpreted as complete treatment packages. An estimate for a regimen such as intra-articular steroid injections combined with hydrodilatation and passive mobilisation does not isolate the contribution of any individual component and cannot show additive or synergistic effects. Similarly, usual care represented a heterogeneous pragmatic comparator rather than a single standardised treatment. An intervention compared with minimal care may appear more effective than the same intervention compared with structured exercise or physiotherapy, and this variation may have contributed to clinical heterogeneity across the networks.

The applicability of the findings should also be considered in light of the geographical and language distribution of the evidence. Overall, 78% of included

trials were conducted in Asia, including 51 of 60 (85%) studies with a usual care comparator. Restriction to English language publications may have excluded relevant local language evidence, particularly for interventions that may be disproportionately represented in non-English language literature. These factors may have affected the representation of some interventions, the structure of the evidence networks, and the applicability of estimates anchored to usual care across healthcare settings with different treatment practices.

The available evidence primarily addressed outcomes within the first six months. Early improvement should not be interpreted as complete recovery, sustained treatment benefit, or evidence that an intervention alters the natural course of adhesive capsulitis, which may extend over one to two years or longer.[50 55] Therefore, the findings are most appropriately used to inform early treatment decisions rather than long term comparative effectiveness.

Strengths and limitations

This review comprises a comprehensive synthesis of randomised evidence for interventions for adhesive capsulitis. Key strengths include clinically informed treatment node construction, explicit separation of usual care from placebo and active comparators, assessment of transitivity and consistency, and comparison specific GRADE evaluation of direct, indirect, and network evidence. Prespecified clinically important effect thresholds were used to interpret the magnitude of treatment effects alongside evidence certainty rather than relying solely on statistical significance or treatment rankings. Extensive sensitivity and exploratory analyses examined the influence of key methodological assumptions and potential sources of inconsistency.

Several limitations substantially constrain interpretation. Approximately 70% of studies contributing to the principal outcomes were judged to be at high overall risk of bias. Pain and function were particularly susceptible to the lack of participant masking and subjective outcome measurement, while range of motion measurements may have been influenced by assessor masking and measurement technique. These concerns contributed to downgrading many comparisons and mean that statistically favourable estimates should not automatically be considered credible or clinically actionable.

Although the transitivity assumption was considered broadly plausible, it could not be confirmed. Disease duration, clinical stage, previous treatment, concomitant analgesia, and other potential effect modifiers were incompletely or inconsistently reported. Baseline severity and demographic characteristics differed appreciably for some network links, and many comparisons were informed by only one or a few studies. Global inconsistency was detected for short term function and for flexion at short term and mid-term follow-up. Local inconsistency was also identified in several pain, function, flexion, abduction, and external rotation comparisons, including comparisons of extracorporeal shockwave therapy with usual care for short term function, flexion, and abduction. Exploratory exclusion analyses involving the removal of a small number of studies resulted in non-significant global inconsistency tests for some outcomes. However, the exclusions changed the evidence contributing to some comparisons and may have also reduced the power to detect inconsistency. Therefore, these analyses were used to explore potential sources of inconsistency and assess the stability of the findings (ie, the extent to which the direction and magnitude of effect estimates were sensitive to the removal of potentially influential studies) rather than to confirm the robustness of the primary results. Moreover, statistical tests for inconsistency have limited power in sparse networks and a non-significant result does not establish coherence. Where statistically significant disagreement between direct and indirect evidence was identified for a specific comparison, this incoherence was incorporated into the comparison specific GRADE assessment and resulted in downgrading of the certainty of the corresponding network estimate.

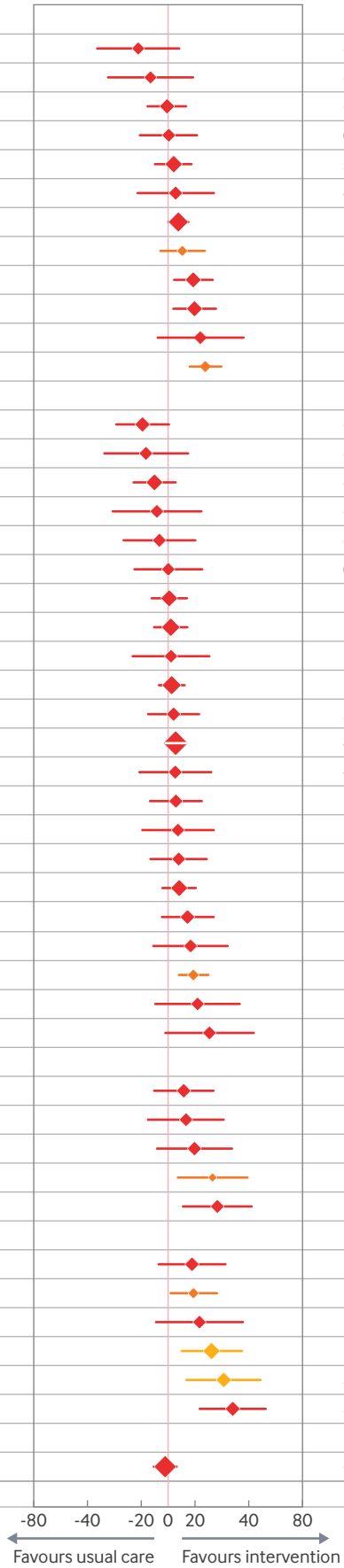
Usual care represented a heterogeneous pragmatic comparator, and 51 of the 60 studies containing a usual care arm were conducted in Asia. Although the sensitivity analysis restricted to structured and broadly balanced usual care produced broadly similar findings, variation in the content and intensity of usual care across studies cannot be excluded as a source of clinical heterogeneity. The geographical concentration of these comparisons may also limit the applicability of estimates anchored to usual care in other healthcare settings.

Linear conversion of pain and function measures to common scales facilitated pooling but did not establish conceptual equivalence between the instruments. The Constant-Murley Score combines subjective and objective domains, whereas the Shoulder Pain and Disability Index primarily captures patient reported pain and disability. Similarly, the primary range of motion analyses combined passive, active, and unspecified measurements. Although sensitivity analyses restricted to passive measurements were conducted, differences in measurement type may still have introduced clinical and methodological heterogeneity.

Standard deviations of change scores were imputed for some studies using an assumed baseline to follow-up correlation of 0.50. Sensitivity analyses using alternative correlations of 0.25 and 0.75 generally supported the direction and interpretation of the principal findings, although some estimates in sparse networks remained sensitive to this assumption.

Adverse event reporting was inadequate. The absence of reported events could not be treated as evidence that no events occurred, and the available data were insufficient to identify uncommon, serious, delayed, or treatment specific harms. The absence of statistically significant differences between interventions should therefore not be interpreted as evidence of equivalent safety.

Intervention	No of participants	No of trials	Mean difference (95%CI)	Mean difference (95%CI)
Conservative				
Thermotherapy and passive mobilisation	30	1		-17.69 (-42.18 to 6.80)
LLLT and PM	30	1		-10.39 (-35.73 to 14.95)
Ultrasound therapy	359	5		-0.53 (-11.94 to 10.88)
Bee venom acupuncture	106	2		0.48 (-16.59 to 17.55)
Low level laser therapy	335	7		3.31 (-7.64 to 14.25)
Yoga	40	1		4.80 (-18.02 to 27.62)
Passive mobilisation	988	18		6.41 (0.50 to 12.32)
Acupuncture	192	4		8.89 (-4.31 to 22.09)
Proprioceptive neuromuscular facilitation	160	4		15.25 (3.69 to 26.81)
Muscle energy technique	168	4		15.96 (3.29 to 28.63)
Heat therapy	20	1		19.50 (-6.01 to 45.01)
Extracorporeal shockwave therapy	365	8		22.42 (12.99 to 31.84)
Injection or procedure				
Oral steroids	118	3		-14.87 (-30.54 to 0.80)
SSNB and HD	48	1		-12.76 (-37.70 to 12.18)
NSAIDs	409	5		-7.78 (-20.28 to 4.73)
Intra-articular steroid and CHL-HD	63	1		-6.27 (-32.62 to 20.07)
Nitrous oxide inhalation	69	1		-4.84 (-26.22 to 16.55)
Intra-articular steroid and SSNB	80	1		0.43 (-19.77 to 20.63)
Intra-articular hyaluronic acid	351	6		1.00 (-9.49 to 11.49)
Intra-articular steroid and HD	690	10		1.83 (-8.06 to 11.71)
Intra-articular steroid, HD, and 5% glucose	100	1		2.05 (-20.83 to 24.92)
Suprascapular nerve block	1060	12		2.52 (-5.19 to 10.23)
Subacromial steroids	196	2		3.62 (-11.59 to 18.83)
Intra-articular steroid	2751	39		4.56 (-1.35 to 10.48)
Intra-articular hyaluronic acid and SSNB	80	1		4.63 (-16.81 to 26.07)
Intra-articular hyaluronic acid and HD	152	2		5.01 (-10.46 to 20.47)
Intra-articular lidocaine	66	1		6.22 (-15.11 to 27.55)
Intra-articular steroid and hyaluronic acid	100	2		6.57 (-10.16 to 23.29)
Hydrodilatation	745	9		6.88 (-3.09 to 16.85)
Intra-articular and subacromial steroids	164	2		12.02 (-3.45 to 27.48)
Pulsed radiofrequency	42	1		13.70 (-8.45 to 35.85)
Intra-articular platelet rich plasma	772	8		15.35 (6.64 to 24.07)
Intra-articular ozone	40	1		17.81 (-7.35 to 42.97)
Rotator interval hydrodilatation	54	1		24.95 (-1.34 to 51.25)
Combined				
HD and PM	301	2		9.70 (-8.00 to 27.40)
Subacromial steroid, HD, and PM	70	1		10.90 (-11.63 to 33.43)
Intra-articular steroid and PM	51	2		16.05 (-6.29 to 38.38)
Intra-articular steroid, HD, and PM	53	1		26.74 (6.00 to 47.47)
SSNB and LLLT	55	1		29.63 (9.09 to 50.17)
Surgical or release				
Acupotomy	146	2		14.50 (-5.46 to 34.46)
Arthroscopic release	289	4		15.40 (1.59 to 29.22)
AR and ESWT	46	1		18.95 (-6.98 to 44.89)
Manipulation under anaesthesia	554	5		26.16 (8.19 to 44.13)
MUA and intra-articular steroid	336	3		33.29 (11.19 to 55.39)
AR, intra-articular steroid, and HD	140	2		38.73 (19.02 to 58.43)
Control interventions				
Placebo	1319	26		- 1.54 (-8.35 to 5.28)



Certainty of the evidence ◆ Moderate ◆ Low ◆ Very low

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Fig 6 | Network meta-analysis estimates of interventions compared with usual care for flexion (up to three months). Estimates are presented as mean differences with 95% confidence intervals (CIs). Negative mean differences favour usual care whereas positive mean differences favour intervention. The vertical line at zero represents no difference between intervention and usual care. Interventions are grouped according to treatment category. For each intervention, number of participants represents total number randomised across all eligible arms of studies that included that intervention rather than only the number allocated to intervention arm. AR=arthroscopic release; CHL-HD=coracohumeral ligament hydrodilatation; ESWT=extracorporeal shockwave therapy; HD=hydrodilatation; LLLT=low level laser therapy; NSAID=non-steroidal anti-inflammatory drug; PM=passive mobilisation; SSNB=suprascapular nerve block

Restricting eligibility to English language publications may have introduced language, selection, and geographical bias. Relevant local language trials may have been missed, particularly for interventions such as acupuncture, acupotomy, and bee venom acupuncture. This limitation should be considered together with the geographical concentration of the evidence, particularly the concentration of usual care comparator studies in Asia, and may have affected the completeness and structure of the networks and the applicability of the findings across healthcare settings.

Finally, the evidence was derived predominantly from populations with primary adhesive capsulitis, small single centre trials, and specialised clinical settings. Applicability to patients with secondary adhesive capsulitis, diabetes, rheumatoid arthritis, previous shoulder surgery, later stage disease, and healthcare systems underrepresented in the available evidence remains uncertain.

Relation to previous evidence

Seven previous network meta-analyses[10-16] evaluated narrower subsets of interventions and included between 17 and 92 randomised trials, whereas the present review included 189 trials across conservative, injection based, procedural, combination, and surgical treatments (appendix 15). Previous reviews often based their conclusions primarily on statistical significance or treatment ranking probabilities. By incorporating prespecified thresholds for clinically important effects and comparison specific assessments of evidence certainty, the present analysis provides a more cautious interpretation of apparently favourable treatment rankings.

The findings are broadly consistent with previous suggestions of short term benefit from extracorporeal shockwave therapy and possible benefits from platelet rich plasma or selected rehabilitation strategies. However, the present assessment indicates that the certainty supporting many of these interventions is lower than their ranking alone might imply. In particular, the apparent functional and range of motion benefits of extracorporeal shockwave therapy were limited by important incoherence between direct and indirect evidence.

Implications for practice and research

Extracorporeal shockwave therapy is supported most clearly for short term pain reduction. Moderate certainty evidence suggests that it probably reduces short term pain compared with usual care. Apparent benefits for short term function, flexion, and abduction are less certain because these estimates were supported by low certainty evidence and important incoherence between direct and indirect evidence.

Manipulation under anaesthesia, with or without intra-articular steroid injections, probably improves short term flexion. However, an isolated improvement in range of motion should not, by itself, be interpreted as sufficient evidence to favour invasive treatment over less invasive options or as showing a favourable overall benefit-harm profile. Evidence for patient reported pain and function, complications and other harms, longer term recovery, and comparative safety remains limited or uncertain.

This uncertainty is particularly important because adverse event reporting was incomplete and inconsistent. The absence of reported adverse events should not be interpreted as evidence of absence of harm, particularly when comparing non-invasive interventions with manipulation under anaesthesia or surgical procedures. Favourable efficacy estimates for an individual outcome therefore cannot establish a favourable overall benefit-harm profile.

For most other interventions, the evidence remains too uncertain to support a definitive treatment hierarchy. Clinical decisions should therefore be outcome specific and based on the certainty of evidence across all clinically important outcomes, while also considering treatment invasiveness, costs and treatment burden, local expertise, the patient's clinical circumstances and preferences, and uncertainty about comparative safety and longer term benefits. Shared decision making is particularly important when potential improvements in individual outcomes must be weighed against uncertain harms or longer term effects.

Future trials should use rigorous randomisation and allocation concealment, blinded outcome assessment where feasible, prespecified protocols and analysis plans, standardised intervention descriptions, and a core set of patient reported and objective outcomes. Studies should report disease stage and duration using consistent definitions, distinguish active from passive and pain-free range of motion, and systematically collect serious and non-serious adverse events by treatment group.

Adequately powered head-to-head trials with follow-up beyond 12 months, and preferably 18-24 months where feasible, are needed to determine whether early improvements are sustained and whether interventions alter the longer term recovery trajectory. Trials directly comparing promising interventions with

clearly defined usual care programmes are particularly important for reducing reliance on indirect evidence and resolving the incoherence identified in the current networks.

Conclusion

This network meta-analysis does not support a definitive hierarchy of treatments for adhesive capsulitis. Moderate certainty evidence suggests that extracorporeal shockwave therapy probably reduces short term pain and that manipulation under anaesthesia, with or without intra-articular steroid injections, probably improves short term flexion compared with usual care. Apparent benefits of extracorporeal shockwave therapy for short term function, flexion, and abduction are less certain because these findings were supported by low certainty evidence and important incoherence between direct and indirect estimates.

For most other interventions and mid-term outcomes, the evidence was of low or very low certainty. Comparative safety also remains highly uncertain because adverse events were incompletely and inconsistently reported; therefore, the absence of reported adverse events should not be interpreted as evidence that treatment is free from harm, particularly for invasive interventions such as manipulation under anaesthesia or surgery.

These findings primarily inform treatment decisions within the first six months and do not establish longer term comparative effectiveness or overall benefit-harm profiles. The evidence was derived predominantly from patients with primary adhesive capsulitis enrolled in small, often single centre trials, which limits applicability to secondary adhesive capsulitis, important comorbidities, and other clinical settings. Treatment decisions should therefore remain outcome specific and should also consider invasiveness, costs, patient preferences, clinical expertise, and uncertainty about longer term benefits and harms.

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Contributors: JM and PL evenly shared their contributions to this work. SG, JM, NSG, SBSK, SSZ, DLR, MS, SK, SSM, and ME conceived and designed the study.SG, JM, NSG, and SBSK discussed and drafted the study protocol. JM, PL, SC, and XL screened and selected the articles. JM, PL, SC, XL, YW, HY, and SW extracted the data and assessed the risk of bias of included trials. JM, PL, and XL analysed the data. JM, PL, and ZX rated and revised the GRADE certainty of evidence. SG, SSZ, CH, JM, and PL interpreted the results. SG and JM drafted and critically revised the manuscript. All authors contributed to revising the manuscript, had full access to all the data, and took final responsibility for the decision to submit for publication. SG, JM, NSG, SBSK, SSZ, DLR, MS, SK, SSM, and ME supervised the study. SG is the guarantor. The corresponding author attests that all listed authors meet authorship criteria and that no others meeting the criteria have been omitted.

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Ethical approval: Because the literature included in the meta-analysis got ethics approval, no additional ethics approval was required.

Data sharing: The data supporting the findings of this study were extracted from published studies and are available in the supplementary materials. Additional analytic datasets and code are available from the corresponding author upon reasonable request.

Transparency: The manuscript's guarantor (SG) affirms that the manuscript is an honest, accurate and transparent account of the study being reported; that no important aspects of the study have been omitted; and that any discrepancies from the study as planned and registered have been explained.

Dissemination to participants and related patient and public communities: Our goal is to communicate the outcomes of this research to a broad range of audiences to maximise its influence on clinical care and public health. Within academic and clinical circles, findings will be delivered at national and international conferences through oral and poster formats, and will also be integrated into teaching sessions for students in medicine and allied health disciplines. Additionally, we intend to present our results to pertinent policy makers and committees responsible for clinical guidelines, with the aim of shaping future practice. Beyond that, we will actively circulate our findings through a variety of social media and short video channels, including TikTok and WeChat.

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