Arthroscopic Compared with Open Repairs for Recurrent Anterior Shoulder Instability. A Systematic Review and Meta-Analysis of the Literature

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A Systematic Review and Meta-Analysis of the Literature

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Background: Both arthroscopic and open surgical repairs are utilized for the management of anterior glenohumeral instability. To determine the evidence supporting the relative effectiveness of these two approaches, we conducted a rigorous and comprehensive analysis of all reports comparing arthroscopic and open repairs.

Methods: A systematic analysis of eighteen published or presented studies was performed to determine if there were significant differences between the two approaches with regard to recurrence (recurrent dislocation, subluxation, and/or apprehension and/or a reoperation for instability), return to work and/or sports, and Rowe scores. We also performed subgroup analysis to determine if the quality of the study or the arthroscopic technique influenced the results.

Results: We identified four randomized controlled trials, ten controlled clinical trials, and four other comparative studies. Results were influenced both by the quality of the study and by the arthroscopic technique. Meta-analysis revealed that, compared with open methods, arthroscopic repairs were associated with significantly higher risks of recurrent instability ($p < 0.00001$, relative risk = 2.37, 95% confidence interval = 1.66 to 3.38), recurrent dislocation ($p < 0.0001$, relative risk = 2.74, 95% confidence interval = 1.75 to 4.28), and a reoperation ($p = 0.002$, relative risk = 2.32, 95% confidence interval = 1.35 to 3.99). When considered alone, arthroscopic suture anchor techniques were associated with significantly higher risks of recurrent instability ($p = 0.01$, relative risk = 2.25, 95% confidence interval = 1.21 to 4.17) and recurrent dislocation ($p = 0.004$, relative risk = 2.57, 95% confidence interval = 1.35 to 4.92) than were open methods. Arthroscopic approaches were also less effective than open methods with regard to enabling patients to return to work and/or sports ($p = 0.03$, relative risk = 0.87, 95% confidence interval = 0.77 to 0.99). On the other hand, analysis of the randomized clinical trials indicated that arthroscopic repairs were associated with higher Rowe scores ($p = 0.002$, standardized mean difference = 0.43, 95% confidence interval = 0.16 to 0.70) than were open methods. Similarly, analysis of the arthroscopic suture anchor techniques alone showed the Rowe scores to be higher ($p = 0.04$, standardized mean difference = 0.29, 95% confidence interval = 0.01 to 0.56) than those associated with open methods.

Conclusions: The available evidence indicates that arthroscopic approaches are not as effective as open approaches in preventing recurrent instability or enabling patients to return to work. Arthroscopic approaches resulted in better function as reflected by the Rowe scores in the randomized clinical trials. The study design and the arthroscopic technique had substantial effects on the results of the analysis.

Level of Evidence: Therapeutic Level II. See Instructions to Authors for a complete description of levels of evidence.

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Surgical repairs for the treatment of anterior instability of the shoulder can be performed with use of either arthroscopic or open approaches. The classic open Bankart procedure includes incision of the subscapularis tendon and capsule to expose the anterior aspect of the labrum and capsule for secure reattachment to the glenoid rim with suture passed through bone tunnels[1]. Multiple variations have been used to expose the joint, to repair the ligaments, and to treat associated capsular laxity[2-11]. Open approaches have yielded consistently low rates of recurrent instability[12-15].

Arthroscopic techniques have also been utilized to treat anterior instability of the shoulder. Results have been reported after the use of staples[16], transglenoid sutures[17,18], bioabsorbable tacks[19], and suture anchors[20-22], with proponents describing benefits related to smaller incisions, less loss of motion, lower risk of subscapularis failure, quicker return to sports, and higher patient satisfaction.

Despite the advocates for each approach, the evidence regarding the relative effectiveness of open and arthroscopic treatment of anterior glenohumeral instability remains unclear. Two previous meta-analyses have been published[23,24], but these studies did not:

1. include all available published and unpublished series.

2. utilize both fixed-effects and random-effects models[25,26]. Fixed-effects models assume homogeneity—i.e., that every study included in the analysis evaluated the same treatment effect. Random-effects models assume that the treatment effect may have been different in each study. Heterogeneity can result from the inclusion of different populations, from the application of different surgical techniques, from differences in follow-up, or from differences in outcome measurement. If bias or heterogeneity is present, the fixed and random-effects models may lead to different conclusions.

3. employ funnel plots to discover possible publication bias[27]. Funnel plots are a graphical representation of treatment effect versus sample size. Optimally, the treatment effect should not change with sample size; funnel plots reveal asymmetry in this relationship. Asymmetry (e.g., the relative absence of small studies with negative findings) can bias conclusions away from the true treatment effect.

4. include a number-needed-to-treat analysis to demonstrate the clinical relevance of the different outcomes[28]. The number needed to treat describes the number of patients one would need to treat with one technique (e.g., open repair) rather than the alternative technique (e.g., arthroscopic repair) in order to prevent a single event (e.g., recurrent dislocation).

5. compare what has been proposed as the "gold standard" arthroscopic technique—i.e., repair with suture anchors[21,29-32]—with open approaches.

6. include an analysis of the effect of study quality on the observed treatment effect. The reliability and utility of a meta-analysis depend in large measure on the quality of the primary source studies that are evaluated. Non-randomized trials are expected to have a variety of biases compared with randomized controlled trials[33]. A meta-analysis that compares randomized with non-randomized trials offers one window into this effect.

Because of the clinical importance of the question and the incompleteness of the published systematic reviews, we performed a systematic review and rigorous meta-analysis of all published and presented literature comparing open and arthroscopic approaches to the treatment of anterior shoulder instability. In this study, we tested the hypothesis that the literature demonstrates significant differences between the effectiveness of arthroscopic treatment and that of open treatment of anterior shoulder instability, specifically with respect to (1) restoration of shoulder stability (indicated by the absence of recurrent dislocation, subluxation, or recurrent apprehension and no need for a reoperation), (2) the rate of recurrent dislocation alone, (3) the rate of reoperations for instability alone, (4) the ability of patients to return to work or sports, and (5) the Rowe scores. We further hypothesized that the quality of the study influences the results. Finally, we tested the hypothesis that the specific arthroscopic technique influences the clinical outcome.

Materials and Methods

Inclusion and Exclusion Criteria

We identified articles and abstracts that met the following inclusion criteria: (1) comparison of one or more arthroscopic techniques with open techniques, (2) evaluation of patients who had predominantly anterior instability (including those with a first-time dislocation, those who had attempted rehabilitation, and those with multidirectional laxity but a clinical diagnosis of anterior instability), and (3) use of an anterior soft-tissue repair (i.e., a Bankart procedure and/or capsular shift). We included retrospective comparative studies and observational case-control trials as there is a paucity of randomized, controlled trials pertaining to this topic; inclusion of these studies, which were analyzed separately from the randomized trials, permitted evaluation of the hypothesis regarding the effect of study quality on the size of the observed effect.

Studies were excluded if there was no comparison group, if a bone-block-type of procedure was used, or if the predominant direction of the instability was posterior. We did not exclude papers that included patients with a first-time dislocation or those with glenoid defects or a Hill-Sachs lesion.

Identification of Studies and Publication Bias

A search of the Medline database on PubMed[34], for the years 1966 to November 2004, was conducted with use of five combinations of search terms: (1) "Bankart," (2) "shoulder AND instability," (3) "shoulder AND dislocation AND anterior," (4) "shoulder AND capsulorrhaphy," and (5) "shoulder AND capsular shift." Following this, a search of the Cochrane Collaboration Library[35] was performed with use of the same combination of search terms. We also performed an online search of the Arthroscopy Association of North America (AANA) annual meetings abstracts from 1998 to 2004 and the International Society of Arthroscopy, Knee Surgery and Orthopaedic Sports Medicine (ISAKOS) meeting abstracts available for 1997, 1999, and 2001. We performed a manual search of the American Academy of Orthopaedic Surgeons (AAOS) annual meeting abstracts

from 2000 to 2005 and the American Shoulder and Elbow Surgeons (ASES) annual open meeting abstracts from 1996 to 2005. Additional strategies included searching the citations of several review articles and a prominent textbook, *The Shoulder*[8]. We also contacted three subject-matter experts who were not involved in this study in order to maximize the likelihood that all relevant literature would be identified. In addition, attempts were made to contact authors of several articles used in the study to obtain additional, unpublished data.

Two orthopaedic surgeons independently reviewed the titles to identify articles that might meet our eligibility criteria. These abstracts or articles were then collected and reviewed to determine if they were appropriate for inclusion.

Data Extraction

Two coauthors (T.R.L. and A.K.F.) abstracted all relevant available information regarding the type of study, the population, the intervention, and the end points from each article. Any differences in the data that the two collected were reconciled by consensus. Demographic data included age, sex, hand dominance, number of preoperative dislocations, time to surgery, and duration of follow-up. Surgical data included the technique that was used. Outcome data included stability end points (recurrent dislocation, subluxation, and/or apprehension and/or a reoperation for recurrent instability), the Rowe score (a composite score in which up to 50 points is assigned for stability, up to 30 points is assigned for function, and up to 20 points is assigned for motion)[1], the ability to return to work and/or sports, and the range of motion.

Complications were recorded as presented in the report.

Assessment of Methodological Quality and Grouping of Studies

Each study was classified into one of three groups on the basis of the overall study quality. Grouping was based on the Levels of Evidence statement in the Instructions to Authors of *The Journal of Bone and Joint Surgery*. Level-I therapeutic studies include high-quality randomized controlled trials with ≥80% follow-up, blinding, and narrow confidence intervals. Level-II therapeutic studies include lesser-quality randomized clinical trials (no blinding, <80% follow-up, or improper randomization) and prospective comparative studies. Level-III therapeutic studies include case-control series and retrospective comparative series, and Level-IV and V studies include case series and expert opinion, respectively. In our study, the "best" group included Level-I and II randomized clinical trials. The "good" group included Level-II prospective comparative studies and Level III case-control studies (controlled clinical trials). The "fair" group included Level-III series in which the arthroscopically treated group differed from the group treated with an open technique as treatment was indicated by the pathological findings at the time of surgery or the inability to perform a secure repair with use of an arthroscopic technique (finding-dictated trials). Placing the studies into one of these three groups allowed us to perform a subgroup analysis of the influence of study quality on the effect estimate.

For subgroup analysis of the influence of the specific arthroscopic technique on the effect estimate, studies were classified according to the arthroscopic intervention that had been used, regardless of whether suture anchors or bone tunnels had been employed for the open procedures. Three groups were established according to whether the arthroscopic repair had been done with suture anchors, bioabsorbable tacks, or transglenoid sutures. Three studies[36-38] were excluded from this portion of the analysis because they could not be placed into one of these three groups. The authors of those studies had used multiple arthroscopic techniques and did not report the results of the techniques independently.

Meta-Analysis

For binary outcomes (dislocation, subluxation, apprehension, reoperation, and return to work and/or sports), the relative risk and 95% confidence interval were calculated with use of the Cochrane Collaboration's meta-analysis program, Review Manager 4.2[39]. Data were then pooled within each subgroup and across all studies with use of both fixed-effects and random-effects models[25,26]. When there was no difference between the findings derived with the two models, we reported the results from the fixed-effects model. If there was a difference, we reported the results of both models. Differences between the models occurred when there was heterogeneity of results across studies and could often be explained by variations among subgroups. A random-effects model typically resulted in a more conservative estimate, meaning that it was less likely to show a difference between treatment approaches than was a fixed-effects model. If an effect was present, significant differences should ideally be shown with use of both models. Results were presented with use of forest plots summarizing effect size estimates and 95% confidence intervals. If an end point was not reported in a study, it was excluded from the analysis.

For continuous outcomes (Rowe scores), the standardized mean difference and 95% confidence interval were calculated with use of Review Manager 4.2[39]. Again, data were pooled within each subgroup and across all studies with use of both fixed-effects and random-effects models. Rowe scores were reported in twelve articles, but the reporting was inconsistent. In four of the articles[36,40-42], the means and standard deviations were included, and those are presented in our analysis. In five articles[43-47], the median values were presented along with the variance; the technique of Tomlinson and Beyene[48] was used to convert those data to the equivalent mean and standard deviation values for inclusion in our analysis. In three articles[30,37,49], the mean values were given without standard deviations so the data could not be included in the statistical analysis. We analyzed our data twice. First we analyzed the initial group of four studies, and then we pooled both the initial group of four studies and the adjusted group of five studies. We reported only the results of the second, pooled analysis unless there was a difference in the results of the two analyses.

The standard chi-square test for heterogeneity (Q) across study results for each outcome was performed, with calculation of the accompanying I^2 statistic[50]. The major advantage in using

this statistic instead of the more commonly used Q statistic is that it provides an estimate of the proportion of total variation in study results that is caused by heterogeneity, rather than sampling error. I^2 may be roughly interpreted as absent (0% to 25%), low (25.1% to 50%), moderate (50.1% to 75%), or high (75.1% to 100%) heterogeneity[50].

Funnel plots were constructed to examine the possibility of publication bias[27], with the relative risk result from each study on the x axis and the standard error on the y axis. The standard error reflects differences among studies of different sample sizes. Publication bias would be suggested by asymmetry in this plot (e.g., with a relative absence of publications showing small effects with smaller sample sizes)[51].

In addition, a number needed to treat was calculated for each binary outcome. The difference in relative risks and the 95% confidence interval of the difference was calculated for each study and then pooled. The inverse of this risk difference and the upper and lower confidence intervals were used to calculate the number needed to treat with a 95% confidence interval. Results can be reported as the number needed to harm or the number needed to benefit, and they indicate how many procedures need to be performed with one approach to prevent one adverse event (or conversely, to result in one good outcome) from occurring with the alternative approach[28].

We attempted to estimate differences in the range of motion, but inadequacies in the reporting of the data prevented analysis of this end point. Authors reported the absolute loss of motion[30,31,36,40,44-46,52-55], the percentage of normal motion[49], or the range of motion[38,43]. No variances, standard deviations, or ranges were presented to allow an estimate of effect differences.

Results

Literature Search (Fig. 1)

The PubMed search identified 2108 studies, and fourteen of these met our inclusion criteria. One of them was excluded as a result of duplicate publication[56], leaving thirteen articles. The search of the Cochrane Collaboration identified ninety-four studies, ten of which met our inclusion criteria. Four had not been identified by the PubMed search[46,57-59]. One of these was excluded as a result of duplicate publication[46], and another[59] was excluded because of inadequate data. Our search of the AANA and ISAKOS abstracts identified three abstracts, two of which met our inclusion criteria. Both articles were later published and were identified by the PubMed search[44,53]. The third was excluded because it included patients in whom a bone graft had been placed on the anteroinferior aspect of the glenoid[60]. Our search of the ASES meeting abstracts identified four abstracts, two of which met our inclusion criteria[40,55]; an author of one of these abstracts[40] responded to our invitation to provide more data for the analysis. This search identified two other studies[44,58], which later were published and were identified with the other search strategies. Our search of the other sources did not identify any additional studies. One other study was published during our project, and it was included[38]. This resulted in eighteen studies[31,36-38,40-45,47,49,52-55,57,58] available for analysis (see Appendix).

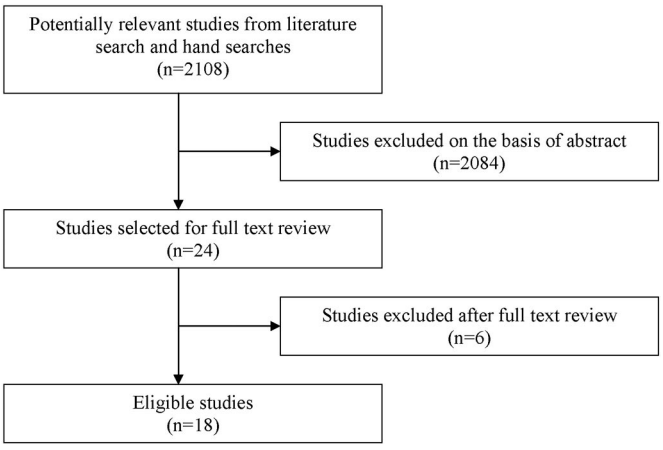

Fig. 1

Summary of the systematic review profile.

Analysis of the funnel plots was limited by the relatively sparse distribution of data points on most of the plots, which made it difficult for us to draw any firm conclusions. The plot with the most data points (recurrent instability) does not suggest a publication bias, as the points are relatively evenly distributed (see Appendix).

Grouping of Studies (see Appendix)

Of the eighteen articles that were identified, four were randomized clinical trials (two Level-I studies[40,47] and two Level-II studies)[41,54]. This was the "best" group of studies. Ten studies, all non-randomized comparative trials, were included in the "good" group (the controlled clinical trials). In these studies, the selected approach was based on patient preference[36,43,49,55], surgeon preference[58], or a retrospective analysis of a surgeon's experience[38,44,52,53,57]. The "fair" group included four studies in which the intervention was based on the pathological findings seen at the time of surgery[30,31,42] or in which an open approach was used because an arthroscopic approach had failed[37,45].

We identified six studies in which suture anchors had been used in the arthroscopic procedures[40,41,44,54,57,58], four studies in which bioabsorbable tacks had been used in the arthroscopic procedures[30,43,45,53], and five studies in which transglenoid sutures had been used in the arthroscopic procedures[42,47,49,52,53]. Bone tunnels had been utilized in the open procedures only in the studies by Green and Christensen[52] and Geiger et al.[49]. Field et al.[58] and Sisto and Cook[45] did not specify the fixation used in the open procedures. The remaining authors utilized suture anchors in the open repairs.

Analysis of Stability, Rowe Score, and Return to Work and/or Sports

When all studies were included in the analysis, it appeared that open approaches were more reliable in restoring stability to the shoulder (see Appendix). The pooled estimate from all studies demonstrated that arthroscopic repairs were associated with a significantly higher risk of recurrent instability ($p < 0.00001$, relative risk = 2.37, 95% confidence interval = 1.66 to 3.38), recurrent dislocation alone ($p < 0.0001$, relative risk = 2.74, 95%

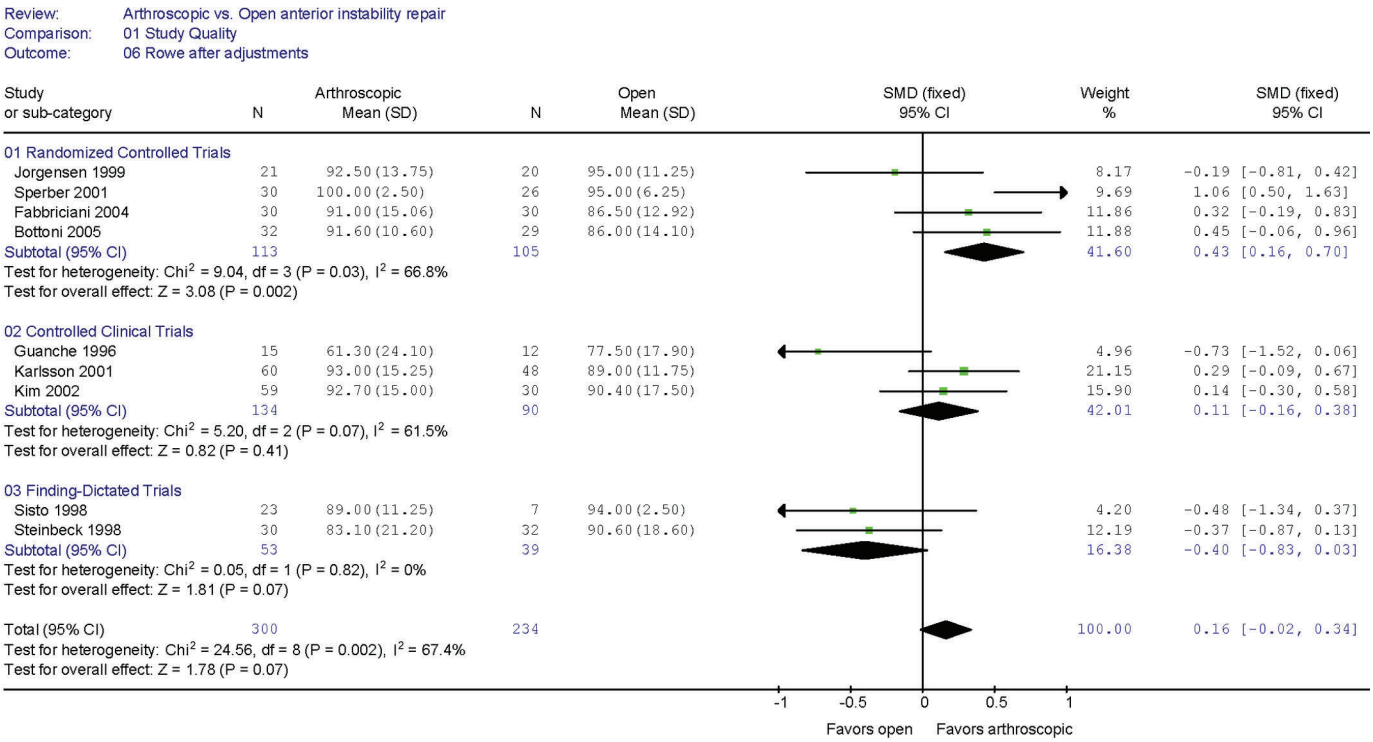

Fig. 2

Standardized mean differences (SMD) in Rowe scores as shown by subgroup analysis of study quality with use of a fixed-effects model.

confidence interval = 1.75 to 4.28), and a reoperation (p = 0.002, relative risk = 2.32, 95% confidence interval = 1.35 to 3.99). The rates of recurrent instability were 18% and 8% after arthroscopic and open approaches, respectively, whereas the rates of recurrent dislocation alone were 12% and 5%, respectively. Number-needed-to-treat analysis demonstrated that nine arthroscopic procedures would lead to one additional case of recurrent instability (95% confidence interval = 7 to 14).

Open approaches were more successful in enabling patients to return to their previous work and/or sport (p = 0.03, relative risk = 0.87, 95% confidence interval = 0.77 to 0.99).

In contrast to restoration of stability, the pooled data demonstrated no difference in Rowe scores between open and arthroscopic approaches (Fig. 2). In the Rowe scoring system,

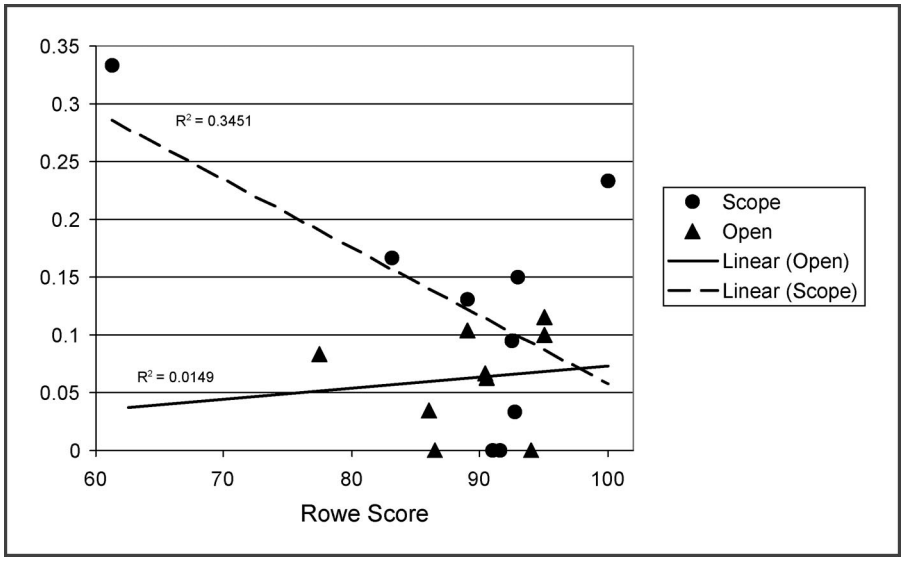

Fig. 3

Relationship of Rowe score to recurrent instability, which is an end point representing either recurrent dislocation or recurrent subluxation. Scope = arthroscopic approach.

a maximum of 50 points is assigned to stability; 20 points, to motion; and 30 points, to function. Because half of the Rowe score is determined by stability, we were interested in the relationship between the Rowe score and recurrent instability (Fig. 3). After arthroscopic repairs, higher rates of recurrent instability were associated with lower Rowe scores, as might have been predicted. Interestingly, this relationship was not observed after open repairs, suggesting that the variance in Rowe scores for patients treated with open repair was related to factors other than recurrent instability, such as motion and function.

Complications (see Appendix)

No subscapularis failures were identified in association with either open or arthroscopic techniques. Stiffness, unexplained pain, and loose hardware were seen in both groups. There were dysesthesias in both groups, with no mention of anesthesia technique (interscalene block and/or general). We did not find sufficient data for a meaningful comparison of complication rates between the two approaches.

Influence of Study Design on Effect Size

Study quality had important influences on the results (Fig. 4). No differences in any of the stability end points were found between the treatment groups in the randomized controlled trials or in the finding-dictated studies (see Appendix). In the controlled clinical trials, the groups were seen to differ significantly with regard to recurrent instability ($p < 0.00001$, relative risk = 3.02, 95% confidence interval = 1.88 to 4.86), recurrent dislocation alone ($p < 0.0001$, relative risk = 3.28, 95% confidence in-

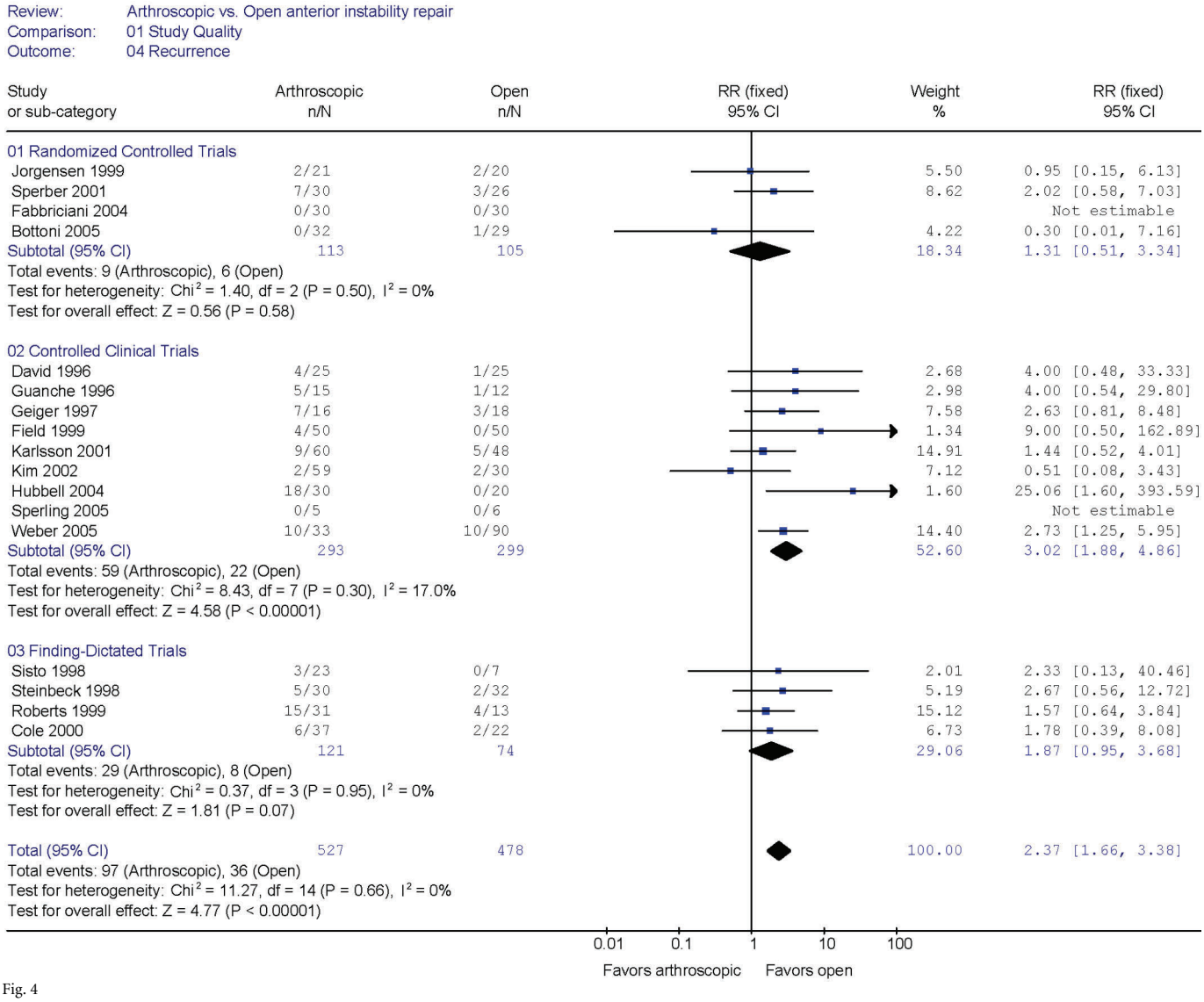

Fig. 4

Relative risk (RR) of recurrent instability as shown by subgroup analysis of study quality with use of a fixed-effects model. n/N = number with recurrent stability/number in study.

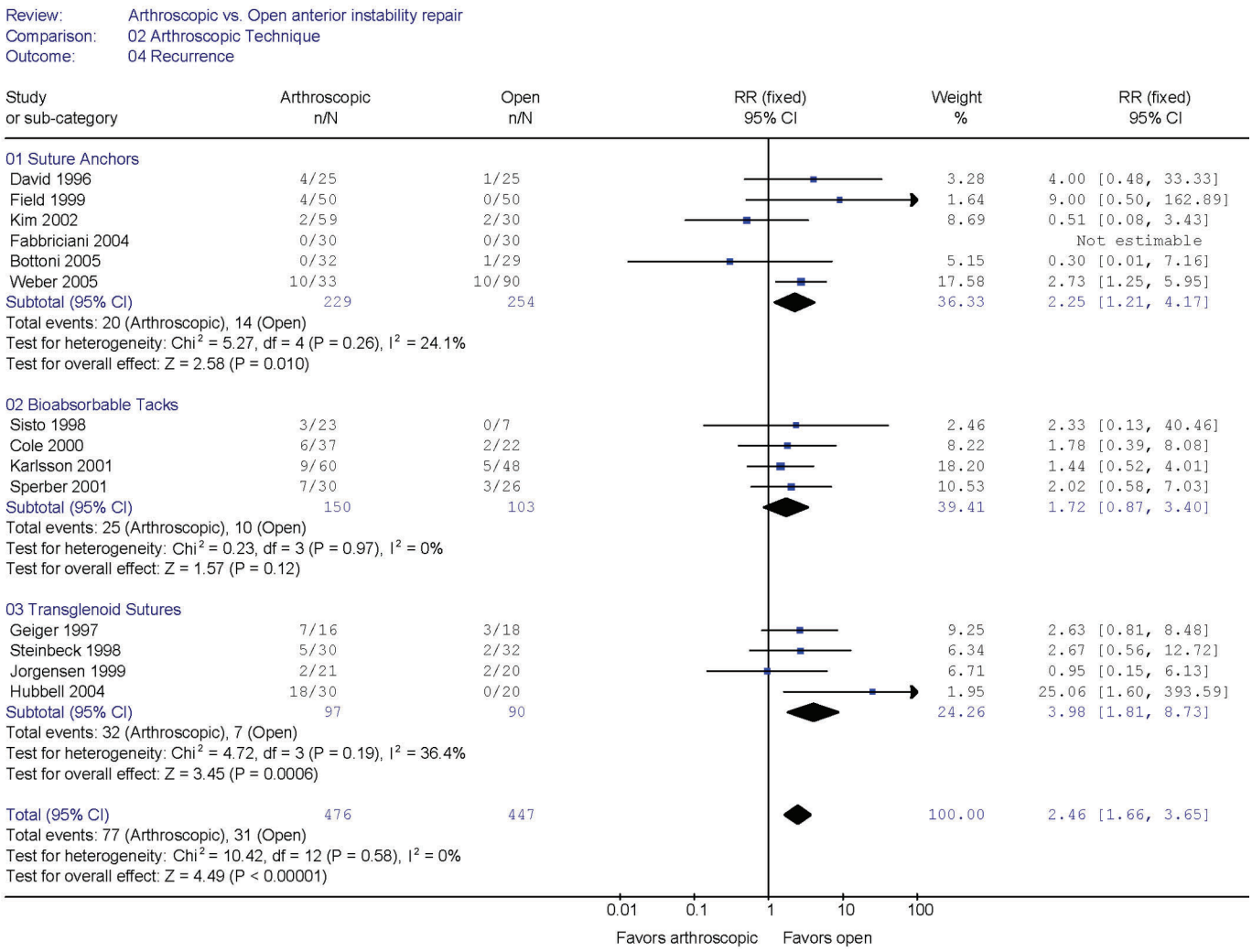

Fig. 5

Relative risk (RR) of recurrent instability as shown by subgroup analysis of arthroscopic technique with use of a fixed-effects model. n/N = number with recurrent stability/number in study.

terval = 1.86 to 5.77), and the need for a reoperation (p = 0.002, relative risk = 2.86, 95% confidence interval = 1.49 to 5.47).

Study quality was also found to have an effect on the detection of differences in Rowe score (Fig. 2). While Rowe scores were found to be no different between the open and arthroscopic treatment groups when we used a fixed-effects model after pooling all of the studies, analysis of only the randomized clinical trials showed better Rowe scores in the arthroscopic group (p = 0.002, standardized mean difference = 0.43, 95% confidence interval = 0.16 to 0.70). Heterogeneity was found to be moderate in this group of studies (I^2 = 66.8%). Thus, differences in this group of studies could be due to differences in study design, patient demographics, surgical technique, or other systematic features. No differences in Rowe scores were seen in any of the subgroups or with the pooled data when a random-effects model was used, indicating that the fixed-effects results are suggestive and that additional studies are needed. Despite higher Rowe scores in the arthroscopic group in the randomized clinical trials, no difference was seen in terms of return to work and/or sports with the numbers available.

Influence of Arthroscopic Technique on Effect Size

Subgroup analysis of specific arthroscopic techniques again showed that open techniques more reliably provided stability (Fig. 5). When the analysis was confined to arthroscopic suture anchor techniques, significantly more recurrent instability (p = 0.01, relative risk = 2.25, 95% confidence interval = 1.21 to 4.17) and recurrent dislocation alone (p = 0.004, relative risk = 2.57, 95% confidence interval = 1.35 to 4.92) were found in the arthroscopic group (see Appendix). When the analysis was confined to arthroscopic transglenoid suture techniques, stability was again seen to be more reliably provided by open techniques, which were associated with lower rates of recurrent instability (p = 0.0006, relative risk = 3.98, 95% confidence interval = 1.81 to 8.73), recurrent dislocation

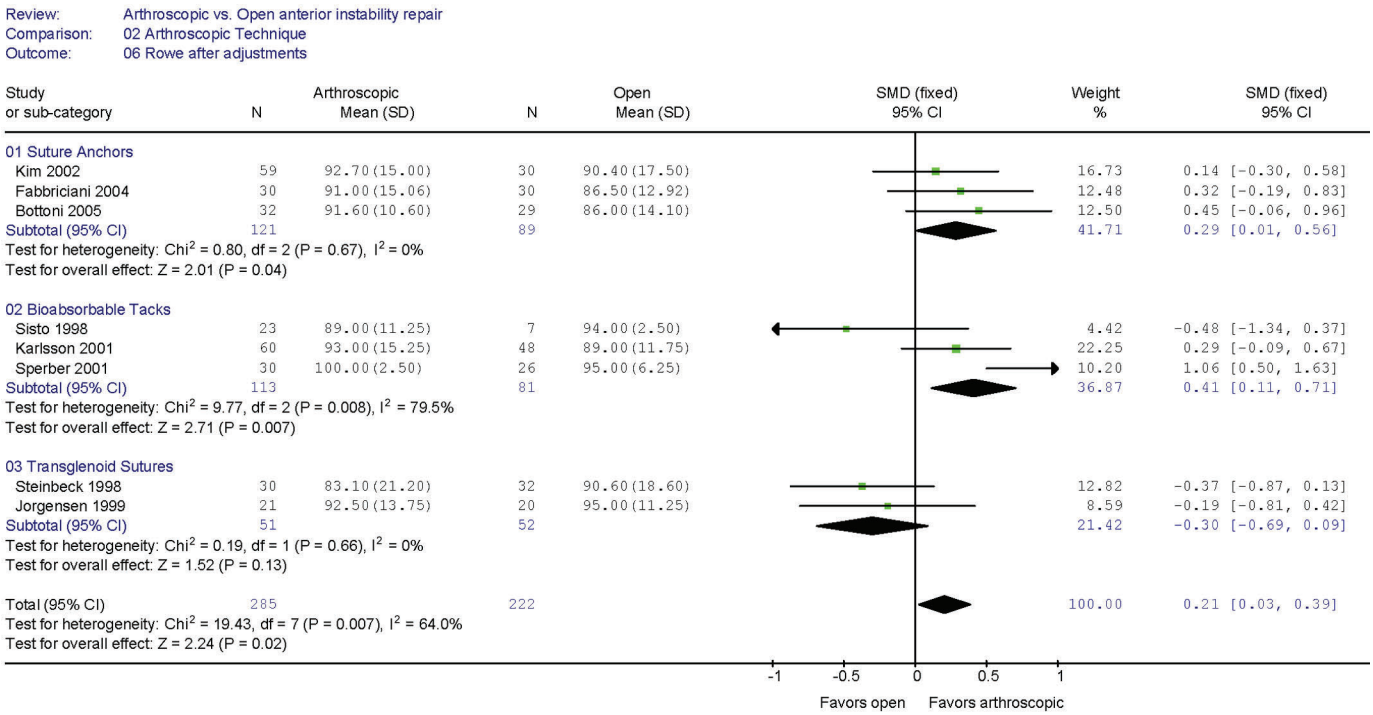

Fig. 6

Standardized weighted mean differences (SMD) in Rowe scores as shown by subgroup analysis of arthroscopic technique with use of a fixed-effects model.

alone (p = 0.01, relative risk = 4.2, 95% confidence interval = 1.34 to 13.16), recurrent subluxation alone (p = 0.03, relative risk = 2.95, 95% confidence interval = 1.12 to 7.73), and a reoperation (p = 0.007, relative risk = 9.81, 95% confidence interval = 1.86 to 51.58). Heterogeneity was low for the end point of recurrent instability (I^2 = 36.4%) and moderate for subluxation (I^2 = 60.2%) in the transglenoid suture group, and this perhaps contributed to differences seen in this subgroup for these end points. Bioabsorbable tacks seemed to be the most reliable arthroscopic technique for restoring stability, with no differences from open techniques with regard to any of the stability end points. The number-needed-to-treat analysis suggested that seventeen arthroscopic procedures with a suture anchor technique would lead to one additional case of recurrent instability (95% confidence interval = 9 to 100).

The subgroup analysis was confounded by several differences between the fixed-effects and random-effects models. The only significant differences seen with random-effects modeling were higher rates of recurrent dislocation and reoperations in the transglenoid suture group compared with those in the open-treatment group. While these findings were in agreement with the results with the fixed-effects model, none of the other significant differences that were seen in the fixed-effects models were shown by the random-effects models. This indicates that the differences shown by the fixed-effects model are suggestive but additional studies are needed.

In contrast to the results for stability, Rowe scores after arthroscopic procedures involving suture anchors or bioabsorbable tacks were better than those following open techniques (p = 0.04, standardized mean difference = 0.29, 95% confidence interval = 0.01 to 0.56 for the suture anchor group and p = 0.007, standardized mean difference = 0.41, 95% confidence interval = 0.11 to 0.71 for the bioabsorbable tack group) (Fig. 6). There was high heterogeneity in the bioabsorbable tack group (I^2 = 79.5%). In addition, when a random-effects model was used, no difference was found between the bioabsorbable-tack group and the open-treatment group. Because no reports on arthroscopic suture anchors included data regarding return to work or sports, the analysis of the effect of the technique was not performed for this end point.

Discussion

Although recurrent instability is one of the most common shoulder problems being treated with arthroscopic or open surgical approaches, there have been few rigorous trials comparing these methods. The goal of a meticulous systematic review of evidence is to combine the results of all available comparative studies; however, it is critical to recognize that these analyses are intrinsically limited by several factors: (1) each surgeon applies each technique somewhat differently, (2) the needs of individual patients may prompt modifications of the methods within a given surgeon's practice, and (3) surgeons are unlikely to be equally competent with two different techniques. The goal of the present study was to apply the best current methodology to determine whether there is clear evidence in the literature supporting the superi-

ority of either open or arthroscopic approaches.

Our analysis indicates that open approaches are more reliable for restoring stability. Pooled data demonstrated significantly lower risks of recurrent instability, dislocation alone, and a reoperation after open procedures. In addition, arthroscopic techniques involving use of suture anchors[40,41,44,55,57,58] were shown to be significantly inferior to open techniques with respect to the resulting rate of recurrent instability.

In contrast, we found evidence that arthroscopic approaches resulted in better Rowe scores. This was the case for both arthroscopic procedures done with suture anchors and those done with bioabsorbable tacks. As half of the Rowe score is determined by stability, differences were likely due to higher scores for function and motion (which account for the other half of the score) after arthroscopic repair. Although arthroscopic approaches resulted in better Rowe scores, they were not as good as open approaches in enabling patients to return to work or sports.

As an additional part of our analysis, we tried to determine if study quality influenced the results. Given the relative lack of randomized controlled trials in the orthopaedic literature[61,62], this is a critically important question. We found that the results differed among the randomized clinical trials, the controlled clinical trials, and the finding-dictated studies. The controlled clinical trials (and the pooled effect estimate) showed significant differences between the open and arthroscopic approaches with regard to the end points of recurrent instability, dislocation alone, and the need for a reoperation, whereas these differences were not found in the randomized clinical trials or the finding-dictated trials. This observation indicates that the pooled-effect estimates were influenced heavily by the controlled clinical trials, where heterogeneity may have been a factor. With respect to Rowe scores, significant differences between the open and arthroscopic approaches were found in the randomized clinical trials but not in the controlled clinical trials, the finding-dictated studies, or the pooled estimate. Although no differences with regard to return to work or sports after the two approaches were seen in any subgroup, the pooled estimate demonstrated a difference. All of these results indicate that the quality of the study strongly influenced the results, suggesting the need for care in interpreting results from systematic reviews of studies of different or uncertain quality.

We also sought to determine if the arthroscopic technique influenced the results. Proponents of the arthroscopic approach claim that newer techniques involving use of suture anchors yield outcomes approaching those with open techniques[21,29-32]. However, our subgroup analysis did not support this contention. Our analysis of the six trials in which this technique had been used revealed that significantly more recurrent instability and recurrent dislocation alone were seen in the arthroscopic groups. In contrast, the Rowe scores seen following use of this arthroscopic technique were better than those observed after open techniques. Although the data did not allow a direct comparison between arthroscopic techniques, bioabsorbable tacks seemed to perform better than the other two arthroscopic techniques, as there were no differences in the stability end points between the bioabsorbable-tack and open techniques. The transglenoid sutures performed poorly compared with open techniques. Poor results have been described in many previous studies of the transglenoid suture technique[18,63-66].

Two previous meta-analyses also demonstrated that instability is more likely to recur following arthroscopic repairs[23,24]. In a systematic review of six studies, Freedman et al. found that the odds of recurrent dislocation were 2.3 times greater after an arthroscopic technique[23], and Mohtadi et al. found that the odds were 2.0 times greater in a review of eleven studies[24]. Unlike us, Freedman et al. found better Rowe scores after the open approach, although none of the series that they included were treated with an arthroscopic suture anchor technique. Mohtadi et al. found odds of 2.9 in favor of open techniques with regard to the end point of the patient returning to work, an observation that is similar to ours. Several high-quality studies, some of which have included an arthroscopic suture anchor technique, have become available since these prior meta-analyses were performed. We included eighteen studies in our systematic review, including four randomized clinical trials (involving a total of 218 patients) and six reports presenting the results of an arthroscopic suture anchor technique (483 patients). This allowed us to examine the influence of both study design and arthroscopic technique. We found that heterogeneity possibly affected some of our conclusions, as discussed. It did not seem that publication bias was present. Utilization of fixed-effects and random-effects models was enlightening, as there was disagreement between the models with regard to several end points and subgroups in the analysis of arthroscopic technique, indicating heterogeneity in the studies.

The results of this study must be considered in light of certain limitations. First, many surgeons will not attempt an arthroscopic repair in patients who have a large osseous defect (≥25% of the glenoid or ≥21% of the glenoid length[30,67,68]), in athletes who play contact sports[3,69], or in patients with multiple recurrences[69]. Inclusion of such patients in previous series may have contributed to the inferior results seen with the arthroscopic approach. We did not perform a subgroup analysis of these factors, as the data were not presented in a way that allowed this to be done, so unfortunately we were unable to clarify this issue. Second, analysis of shoulder function was limited by the data available; ideally such data would include not only a scoring system (such as the Rowe score) and return-to-activity data but also analyzable data on range of motion and subscapularis function. We attempted to evaluate all of these factors, but we were limited by sparse data. Statistical analysis would be facilitated by consistent reporting of mean values with standard deviations for range of motion in degrees of forward elevation, external rotation at the side, and external rotation in abduction. Little information was available on the function and integrity of the subscapularis after surgery, one of the primary concerns with open surgical approaches. Use of the Rowe score to evaluate shoulder function is another limitation as that score is compromised by the weight placed on

stability. We found that higher recurrence rates after arthroscopic techniques seemed to be related to lower Rowe scores. Low Rowe scores following open repairs were not associated with recurrence, suggesting that points were lost as a result of diminished range of motion or function rather than because of instability. We included the Rowe score in this analysis because it was commonly included in the literature that we analyzed. The results discussed here demonstrate the shortcomings of any scale that attempts to combine points for attributes as disparate as stability and range of motion in the same score. Such an attempt requires arbitrary weighting of one attribute in relation to another. It is obviously preferable to use an evaluation system that enables stability, range of motion, function, and comfort to be assessed separately without making an *a priori* assignment of relative value.

In conclusion, the available evidence indicates that recurrence rates are higher after use of arthroscopic techniques, even those involving suture anchors. While return to work and/or sports was better after open repairs, Rowe scores were better following arthroscopic repairs.

Appendix

eA Tables showing the demographics and the characteristics (including reported complications) of the included trials, results of selected subgroup analyses, and a funnel plot for recurrent instability are available with the electronic versions of this article, on our web site at jbjs.org (go to the article citation and click on "Supplementary Material") and on our quarterly CD-ROM (call our subscription department, at 781-449-9780, to order the CD-ROM). ■

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