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Surgical (percutaneous pinning or plate fixation) versus non-surgical interventions for distal radius fractures in adults: an individual participant data review

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Abstract

Objectives

This is a protocol for a Cochrane Review (intervention). The objectives are as follows:

1. To assess the benefits and harms of surgical and non-surgical treatments in adults with distal radius fracture.

2. To examine the interactions between individual-level participant characteristics and the interventions of distal radius fracture.

Background

Description of the condition

Distal radius fractures are one of the most common fractures, accounting for approximately half of hand and wrist fractures (Chung 2001) and 18% of all fractures in adults (Nellans 2012). There is a bimodal age distribution in distal radius fractures, with the first peak of incidence in younger individuals (18 to 25 years old) and the second peak in older adults (> 60 years old) (Nellans 2012). Distal radius fractures are associated with higher energy trauma in younger adults and lower energy trauma in older individuals, prone to fragility fractures. The overall incidence of distal radius fractures is rising, attributed to an aging population (Nellans 2012).

Description of the intervention

Interventions for distal radius fractures may be divided into surgical and non-surgical treatments. Surgical treatments for distal radius fractures include open reduction and internal fixation with dorsal or volar plates, closed reduction and percutaneous Kirschner-wire (K-wire) fixation, and external fixation constructs (Vannabouathong 2019). Open reduction and internal fixation using a volar locking plate is a common operative intervention, but also the most

invasive and costliest intervention (Mellstrand 2019). Percutaneous wiring is considered a less invasive operative intervention, and has lower direct costs, given shorter surgery times and the use of relatively inexpensive implants (Achten 2019). Non-surgical treatments using closed reduction and subsequent cast immobilisation are the cheapest for managing distal radius fractures. Despite the potential risks (i.e. infection, neurovascular injury, hardware complications, tendon rupture, chronic postoperative pain, and scarring) (McKay 2001) and high costs (Shauver 2011, Karantana 2015), the use of surgical interventions for distal radius fractures continues to grow (Mellstrand-Navarro 2014). Although clinical decision making on treatment for distal radius fractures is often influenced by surgeon preference and patient expectation, best available evidence is also considered (Mauck 2018).

How the intervention might work

Interventions for distal radius fractures aim to restore and maintain the alignment (in the coronal, sagittal and/or axial plane) of the fractured bone while union of fracture occurs. Minimally displaced or non-displaced distal radius fractures are mostly treated by closed reduction and immobilisation of the affected wrist by a cast to hold the bone fragments in position for six weeks. Fractures with significant displacement or high likelihood of non-union are treated by surgical interventions. Percutaneous K-wire fixation uses metal wires with a sharp point penetrating through the skin across the distal radius to hold the fracture in the anatomical position with subsequent cast immobilisation (Costa 2014). Open reduction and internal fixation is applied via a volar approach using an angle stable locking plate, and may be followed by application of a plaster splint, usually for approximately two weeks (CROSSFIRE Study Group 2021). While considered the most invasive intervention, plate fixation allows for the earliest onset of rehabilitation. Ideally, interventions for distal radius fractures should allow a basic level of function with limited use of the affected limb, and patients may commence physiotherapy when it is safe to do so, in order to restore joint mobility and muscle strength of the affected wrist to facilitate return to pre-injury function (McQueen 1988).

Why it is important to do this review

Prior to supporting evidence becoming available, a survey of orthopaedic surgeons showed a preference for surgical over non-surgical treatments (Ansari 2011). Since then, numerous clinical trials have compared various treatments for distal radius fractures, and systematic reviews on the published trials have been conducted to assess the effectiveness of these treatments. However, the findings of these reviews are inconsistent. For example, three systematic reviews reported that patients receiving various surgical treatments reported earlier functional improvements than non-surgical treatments (Ochen 2020, Stephens 2020, Vannabouathong 2019). A recent meta-analysis found no clinically important difference between volar locking plate fixation and closed reduction in patient-reported pain and function at timepoints up to 12 months (Lawson 2021).

Clinically, surgeons consider individual characteristics when determining which treatments are perceived to be beneficial. Indeed, individual characteristics (i.e. age, sex, fracture type and severity) could influence treatment outcome (Vannabouathong 2019). For example, surgical treatments might be more effective for younger adults (Ochen 2020). However, systematic reviews using aggregate data meta-analysis are inadequate to answer questions related to characteristics of patient subgroups for several reasons. First, aggregate data meta-analyses are limited in exploring potential intervention-covariate interactions (Wang 2021). Second, definitions of outcomes (i.e. dichotomous versus time-to-event outcomes) are often inconsistent across studies included in systematic reviews, and aggregate meta-analyses often group different outcomes in composite outcomes (Vannabouathong 2019).

Individual participant data meta-analysis (IPD-MA) can overcome the limitations of aggregate data meta-analysis. IPD-MA combines individual participant data from individual trials that allows consistent outcome definitions and unit of analysis (Ventresca 2020) and can avoid bias related to aggregate data meta-analyses when examining interactions between interventions and individual-level characteristics (Simmonds 2005). Further, IPD-MA can assess pre-determined subgroup characteristics with adequate power while maintaining randomisation of the intervention for individual participants (Tudur 2016). Thus, conducting IPD-MA provides the opportunity to elucidate the interactions between overall intervention effects for distal radius fractures and individual-level participant characteristics, which is critical to guide clinical decision making and target treatment at those who are most likely to benefit.

Objectives

1. To assess the benefits and harms of surgical and non-surgical treatments in adults with distal radius fracture.

2. To examine the interactions between individual-level participant characteristics and the interventions of distal radius fracture.

Methods

Criteria for considering studies for this review

Types of studies

We will include any randomised or quasi-randomised (method of allocating participants to a treatment which is not strictly random e.g. by date of birth, hospital record number, alternation) controlled clinical trials comparing surgical with non-surgical methods for treating distal radial fractures in adults. We will include studies reported as full text or published as abstract only where sufficient data are available, and unpublished data from completed studies if available.

Types of participants

We will include trials conducted in adult participants defined as older than 16 years of age, who have sustained a dorsally displaced fracture of the distal radius.

Types of interventions

Eligible trials must compare surgical with non-surgical interventions.

Surgical interventions will include open reduction and internal fixation (with any plate construct) or percutaneous K-wire fixation. Studies using any external fixation constructs will be excluded. Surgical interventions may use any form of anaesthesia.

Non-surgical interventions will include any combination of closed manipulation and immobilisation using any cast (e.g. plaster of Paris, fiberglass, or thermoplastic materials), splint or brace. Non-surgical treatments may use any form of anaesthesia (local, regional, or general), may be performed in any setting (clinic, emergency department, or operating theatre), and may be performed with the aid of real-time imaging.

Types of outcome measures

There is a current initiative to develop a core outcome set for distal radius fractures that is not yet complete (Deshmukh 2021). Therefore, we have selected the most appropriate outcomes after consideration by our expert patients and clinical authors.

Primary outcomes

Critical outcomes:

- Patient-reported pain measured at 12 months post intervention using a visual analogue scale (VAS) or numeric rating scale (NRS).
- Patient-reported function measured at 12 months post intervention using any validated, joint-specific instruments such as Patient-Rated Wrist Evaluation (PRWE) or Disability of the Arm, Shoulder and Hand (DASH) questionnaires.
- Any complication within 12 months of the intervention. These will be categorised as major (thromboembolic events, infection, symptomatic non-union or malunion, implant/splint failure, complex regional pain syndrome, nerve lesions with a persistent sensory or motor deficit, reoperation for any reason, re-intervention specifically for the indications of loss of fracture position, malunion, implant/splint failure, hardware irritation or tendon rupture), minor (tendon irritation not requiring re-intervention, carpal tunnel syndrome, finger stiffness), or death (due to the surgical procedure or from any cause).
- Quality of life measured using any validated tools such as the 12-item Short Form Health Survey (SF-12), 36-item Short Form Health Survey (SF-36) or EuroQoL 5-dimention 5-level (EQ-5D-5L) up to 24 months post intervention.

Secondary outcomes

- Radiographic measures including sagittal alignment (volar/palmar tilt measured in degrees), coronal alignment (radial inclination measured in degrees), axial alignment (radial shortening, or ulnar variance, measured in millimetres) and articular alignment (step-off/gap measured in millimetres), at 3 to 12 months after intervention. The value for volar tilt and radial inclination will be adjusted by subtracting the reported value from their normative values (volar tilt: 11 degrees; radial inclination: 23 degrees) (Goldfarb 2001).
- Any complication reported up to 24 months post intervention.
- Patient-reported function at 3-, 6-, 18-, or 24-months post intervention measured using questionnaires such as PRWE or DASH.
- Patient-reported wrist pain on a VAS or NRS up to five years post intervention.
- Patient-reported treatment satisfaction or success on a dichotomous outcome (yes/no), numerical rating scale or Likert scale.
- Patient-reported bother with appearance on a Likert scale at 12-month post intervention.
- Wrist range of motion including flexion, extension, ulnar and radial deviation measured in degrees at 12-month post intervention.
- Grip strength measured by a hand-held dynamometer (or any similar method) reported in kilograms at 12-months post intervention.

Search methods for identification of studies

Electronic searches

To identify the eligible studies, we will search the following electronic databases for published studies:

- Cochrane Bone, Joint, and Muscle Trauma Group Specialised Register
- Cochrane Central Register of Controlled Trials (CENTRAL)
- Medline Ovid (from 1946 onwards)
- Embase Ovid (from 1980 onwards)
- Cumulative Index to Nursing and Allied Health Literature (CINAHL, 1982 onwards)

Additionally, the following clinical trial registries will be searched for completed unpublished studies:

- ClinicalTrials.gov
- EU Clinical Trials Register
- ANZ Clinical Trial Registry
- WHO International Clinical Trial Registry Platform

The subject specific to MEDLINE search will be combined with the sensitivity-maximising version of the Cochrane Highly Sensitive Search Strategy for identifying randomised trials (Lefebvre 2019). The syntax of the electronic search strategy is available in the Appendix 1. There will be no restriction on the publication period or language. While the review is in progress, citation searching for forward citation of recent studies and citation alerts (i.e., on Google Scholar) on included studies will be used to identify new studies.

Searching other resources

The authors will check the reference list of the eligible studies and published reviews. We will search for the relevant reviews on the Database of Abstracts of Reviews of Effects (DARE) and the database of Health Technology Assessment, and any errata or retraction from the eligible trials on PubMed (www.pubmed.ncbi.nlm.nih.gov) and report the date of the search. We will also search conference proceedings of American Academy of Orthopaedic Surgeons and search for grey literature on Proquest Dissertations and Theses, www.opengrey.eu.

Data collection and analysis

Selection of studies

We will use EndNote (EndNote X9) reference software to store, organise and manage all the search results. After removing any duplicates in the search results, two authors will independently screen titles and abstracts of all studies identified by the search strategy. Full texts of the potentially eligible studies will be retrieved. Two authors will independently assess the full text articles to identify eligible studies for inclusion. Any disagreement between authors will be resolved through discussion and a third author will be consulted if consensus is unable to be achieved. Excluded studies and the reasons for exclusion will be documented. This selection process will be presented in a PRISMA flow diagram, and the characteristics of excluded studies will be summarised.

Data extraction and management

A custom data collection form will be used to extract published aggregate data for study characteristics and outcome data. This data collection form will be piloted on two included studies of the review. Individual participant data (IPD) will be requested from authors of all eligible trials. We will request deidentified data for all participants randomised in the trial and the most complete and updated follow-up data, regardless of the duration of follow-up in the

publication. The trial authors will be contacted up to three times via email, after which IPD from the studies will be considered irretrievable. While the trial authors will be provided with clear instructions on data variables requested, the process of data transfer and preferred data format for each variable, we will also accept data in the format most convenient for the trial authors and reformat the data when necessary. The requested variables from the eligible trials include:

- ***Study characteristics.*** First author, publication status/year of publication, years and places in which the study was conducted, study period and setting, study design (eligibility criteria, randomisation, follow-up period), funding source.
- ***Participant characteristics.*** Age at randomisation, sex, type of fracture, hand dominance, co-morbidities, occupation or employment status, previous glucocorticoid treatment, smoker.
- ***Allocated and received intervention(s).***
- ***Operative intervention characteristics.*** Surgical technique (i.e., plating or wiring), type of implant (i.e., size of K-wires, or type of locking plate), type and duration of postsurgical immobilisation.
- ***Non-operative intervention characteristics.*** Details of closed reduction including type of anaesthetic (i.e., local, regional or general anaesthetic), location of procedure (i.e., emergency department, operating room, or elsewhere), type of splint/cast, duration of immobilisation.
- ***Other co-intervention details.*** Time between injury and receiving intervention, rehabilitation including type (i.e., outpatient physiotherapy, home exercise program), duration/number/frequency of treatments.
- ***Outcome data.*** Primary and secondary outcome measures (as described above) at all timepoints, or the changes in primary and secondary outcome measures between baseline and follow-up timepoints.

All data will be entered in a dedicated database with password protection. One reviewer will transfer the data from the data collection form into the RevMan Web. IPD will be checked for internal consistency and consistency with published reports and for any missing items. We will use standard checks to identify missing data, assess data validity and consistency. The amount of missing data will be assessed and verified. Patterns of treatment allocation and balance of baseline characteristics between treatment groups will be examined to assess randomisation integrity. Follow-up of participants will be checked to ensure that it is balanced by treatment group and is up-to-date. Any queries will be resolved, and the final database will be verified by author of each included trial. Information about the included trials such as randomisation method will be cross-checked with published trial reports, trial protocols and data collection forms. A second reviewer will spot-check the data against the trial reports for accuracy. If IPD cannot be retrieved, aggregate data from published results will be extracted and used in meta-analysis for studies. This will be performed by two independent reviewers and any discrepancy will be resolved through discussion. If IPD are not sought from any eligible study, the reason for this will be stated for each study and summarised.

Assessment of risk of bias in included studies

to Two review authors will independently assess all included studies using the Cochrane ‘Risk of bias’ tool version 1 (ROB 1) (Higgins 2011). Disagreement between authors will be resolved through discussion and a third author will be consulted if consensus is not achieved. We will assess the risk of bias on the following domains:

- random sequence generation

- allocation concealment
- blinding of participants and personnel
- blinding of outcome assessment
- incomplete outcome data
- selective reporting
- other potential sources of bias

We will rate the adequacy of each domain as “high risk of bias”, “low risk of bias” or “unclear”. and we will present a summary figure of the the risk of bias assessment. We will use the results of this assessment when assessing the certainty of the evidence for surgical and non-surgical interventions for distal radius fractures using the GRADE approach.

Measures of treatment effect

Continuous variables such as patient-reported pain and functional outcomes will be analysed as mean differences with 95% confidence intervals (CIs) or standardised mean differences (SMD) with harmonised direction of effect and 95% CIs where continuous variables were assessed using different instruments. The SMD for the combined effect estimate will be re-expressed on the scale of one of the outcome measures used in the included studies (Schünemann 2021). Dichotomous variables (i.e., dichotomous patient-reported safety outcomes) will be analysed as Risk Ratios (RRs) with 95% CIs. It is possible for some outcomes to be reported as survival outcomes, such as time to readmission, or time to complications or revision surgery. These time-to-event outcomes will be analysed as hazard ratios (HRs) with 95% CIs.

Unit of analysis issues

When a trial has multiple timepoints, the categories described in the outcome assessment section will be used in IPD-MA. If studies report multiple intervention arms in a single trial, we will only extract data from the relevant intervention arms. Cross-over trials are not expected for this question and due to the inherent delay of a second-line treatment will not be included in the pooled analysis. The usual considerations for cluster designs in standard meta-analyses are not directly relevant here as the intra-class correlations will be handled within the proposed multi-level model.

Dealing with missing data

Data can be missing for some participants in one or more trials, or for all participants in one or more trials (i.e. variables were not measured, outcomes were missing) (Sutton 1998). All missing data will be assessed for amount and type of missingness (i.e. missing completely at random, missing at random, missing not at random). If the data are found to be missing at random or completely at random, multiple imputation will be applied using Multiple Imputation by Chained Equations (MICE), subject to data check results. Multiple imputation using chained equations will be performed for the main analysis for all outcomes. The potential effects of inclusion of imputed data will be examined by conducting sensitivity analysis using complete case data (see Sensitivity analysis). When data are missing for some participants in one or more trials, two data imputations will be used: a) missing data will be imputed for each study separately, in which case a two-stage analysis will be used (Burgess 2013); b) Monte-Carlo Markov Simulation (MCMS) modelling will be used to impute missing data based on distribution

observed in the published/available data. Results observed using both imputation methods will be reported (if relevant) (Quartagno 2016). A summary of missing data and a detailed methodology of dealing with missing data will also be reported.

Assessment of heterogeneity

Heterogeneity in each analysis will be assessed using the I^2 statistic and the P value from the Chi^2 test, and between study heterogeneity t^2 (Higgins 2003). All analyses will be performed using either Stata 16 (Stata v16) or newly released Stata 17. Both have the capacity to perform one- and two-stage IPD-MA (including calculation of confidence intervals and I^2 statistic), MCMS and other complex analyses. Heterogeneity will be considered substantial when the P value is < 0.1. To quantify the heterogeneity, the following ranges of I^2 statistic will be used to guide the interpretation:

- 0-40%: might not be important;
- 30-60%: may represent moderate heterogeneity;
- 50-90%: may represent substantial heterogeneity;
- 75-100%: considerable heterogeneity (Deek 2022).

When substantial heterogeneity is present, a subgroup analysis and meta-regression will be performed to investigate the potential sources or causes of heterogeneity. Any statistical heterogeneity will be considered when interpreting the results of IPD-MA. When assessing the quality of evidence in GRADE “Summary of findings” tables, 50% will be used as the cut-off point for downgrading due to high heterogeneity. We will also calculate 95% CIs for I^2 values (Higgins 2002).

Assessment of reporting biases

We will compare the original study protocol with the published study results including randomisation balance (overall and during the recruitment process), selective reporting of outcomes, blinding (when planned), planned vs executed analyses (e.g. planned intention-to-treat approach but reporting of findings per protocol) to assess reporting biases.

A funnel plot will be created and examined to explore selective outcome reporting / publication bias when IPD are received and pooled from more than 10 trials, for each outcome. Egger’s test will be used to assess the statistical significance of the reporting bias and a P value < 0.05 is considered statistically significant reporting bias (Egger 1997).

Data synthesis

Quality control

Individual participant data supplied from the included studies will be verified and harmonised. When incoming data are defined or collected differently across studies, they will be recoded into a common format to allow data aggregation. We will contact the trial authors to clarify any data inconsistency and request missing data. To ensure submitted data are accurate, valid and internally consistent, the quality of incoming data will be assessed in the following ways (Tierney 2021):

- Screening the distribution of baseline patient characteristics, number of participants and outcome results of the received individual participant data for inconsistencies against the study publications. If studies report only per protocol results without a comparison of included and excluded participants, we will compare baseline characteristics between included and excluded participants

- Screening the trial data for obvious duplicates or omissions.
- Identifying extreme outliers to check the plausibility of values for each variable received from eligible trials using boxplots. Extreme outliers are defined as data points located outside the whiskers of the boxplots (three times the interquartile range above the upper quartile or below the lower quartile).

IPD meta-analysis

A one-stage approach will be used to perform meta-analysis on IPD received from all eligible trials simultaneously using a hierarchical regression model. However, if convergence issues are raised, or the number of studies with IP level data is limited a two-stage approach may be used, and this will be explicitly stated in the final report. All analyses will be performed by an experienced biostatistician using Stata (Stata v16).

For the one-stage approach, a multilevel mixed-effect model will be used, accounting for random intercept and random treatment effects and clustering of participants by study (included as random intercept by study), to generate an overall summary of the intervention effect (Simmonds 2005, Abo-Zaid 2013). Separate adjustment terms and separate residual variance terms (for continuous outcomes) for each trial will also be included (Burke 2017). Continuous variables will be checked for normality and transformed if applicable. Continuous outcomes (i.e. level of pain and functioning) will be analysed using multilevel generalised linear mixed models and the results will be reported as mean differences with 95% CIs and associated P values whereas dichotomous outcomes (i.e. complications, categorical radiographic findings) will be analysed using multilevel logistic regression models and the results will be reported as RRs with 95% CIs and associated P values (Lin 2020, Burke 2017).

The treatment comparisons of interest in this review are to measure the effects of surgical versus non-surgical interventions on the primary and secondary outcomes. All non-surgical interventions will be considered together. To determine how participant-level covariates (treatment effect modifiers) modify treatment effect, the models will also be used to evaluate the presence of individual-level interaction (Debray 2015) by specifying an interaction term between intervention and individual-level covariates in the model while accounting for clustering of participants within trials. Treatment-covariate interactions will be separated into within- and between-trial interactions to avoid ecological bias (Burke 2017). As interaction terms are heterogeneous between trials, we will use the recommended approach that assumes random-effects distributions for the interaction effects (Simmonds 2007).

As IPD might not be available for all relevant studies, published aggregate data will be included to avoid availability bias or reviewer selection bias (Ahmed 2012) and to increase statistical power for detecting treatment effects or treatment-covariate interactions (Donegan 2013, Jansen 2012). In this instance, we will combine IPD and published aggregate data from the relevant trials in meta-analysis.

Subgroup analysis and investigation of heterogeneity

To investigate whether patient-related and treatment characteristics impact outcome, we plan to conduct subgroup analyses using the following subgroups. The primary meta-analysis will include all these subgroups as covariates to explore the variation in effects by study- or participant-level characteristics via estimating the interactions between effects and covariates. A secondary descriptive analysis for these subgroups will also be performed. These subgroups include:

- sex: male vs female

- type of fracture: AO/OTA classification 23 type A vs type C (Jayakumar 2017)
- prior to randomisation, acceptable radiographic reduction achieved after initial closed reduction (yes/no)
- type of intervention: volar locking plate fixation or K-wiring vs. non-operative intervention
- type of anaesthetic: local vs. regional vs. general anaesthetic
- location of procedure (emergency department vs. theatre)
- patient treatment preference: surgery vs non-surgery vs no preference
- fracture of the dominant hand (yes/no)
- employment status: full-time, part-time, retired, unemployed
- occupation type: low, intermediate, high occupational activity category (Steeves 2015)
- smoking: never/past smoking vs. current smoker;
- diagnosed osteoporosis reported by participants (yes/no)
- diabetes mellitus (yes/no)
- previous glucocorticoid treatment (yes/no)
- rehabilitation following immobilisation: no rehabilitation vs. home based exercise only vs. outpatient physiotherapy (with or without home-based exercise)

The primary outcome measures will be used in these analyses:

- patient-reported pain measured at 12-month follow-up
- patient-reported function measured at 12-month follow-up
- major complications (as defined above under “Types of Outcomes”) within 12 months of the intervention
- minor complications (as defined above under “Types of Outcomes”) within 12 months of the intervention

To examine any differences in treatment effect between subgroups, the formal Q test will be used to test for subgroup interactions (Ronellenfitsch 2021). A significant ($P < 0.05$) interaction between the treatment factor and subgrouping factor indicates the presence of subgroup difference (Liu 2019).

Sensitivity analysis

Pre-planned sensitivity analysis will be carried out to assess the validity and the robustness of the results on the primary outcomes. First, the impacts of including studies at high risk of bias (RoB) will be assessed by running the analysis with those studies excluded. Studies with one or more Cochrane RoB domains at high risk of bias will be considered to be at high risk of bias (Khan 2016). Second, when multiple studies do not provide IPD, we will combine their aggregate data with the IPD to assess the robustness of including or excluding these studies. In addition, we will compare participant characteristics and type of fracture in aggregate data and IPD studies. This approach will help to identify heterogeneity and any bias between these studies to ensure robustness of the meta-analysis and provide preliminary data for further analysis. Third, we will examine whether the inclusion of imputed data alters the final estimates by repeating the meta-analysis using the complete case data.

Summary of findings and assessment of the certainty of the evidence

We will present 'Summary of findings' tables to summarise the key findings of this review using the methods described in the Cochrane Handbook for Systematic Reviews of Interventions (Higgins 2021). The quality of the evidence based on the included studies that contribute data to the meta-analyses for each outcome will be classified as high, moderate, low or very low, using the GRADE domains (risk of bias, consistency of effect, imprecision, indirectness and publication bias) and GRADEpro GDT software (GRADEpro GDT). Justification of the decisions to downgrade the quality of evidence will be provided. We will provide comments on whether additional outcome information that was not included in the meta-analyses and whether it supports or negates the results from the meta-analyses. A “Summary of findings” table will be presented to report the results of the comparisons between surgical and non-surgical interventions on patient-reported pain at 12 months post intervention, patient-reported function at 12 months post intervention, complication within 12 months of intervention, quality of life up to 24 months post intervention, patient-reported treatment satisfaction or success, patient-reported bother with appearance at 12 months after intervention and radiographic measures at three to 12 months after intervention. The intervention effects and the corresponding CIs for all outcomes and GRADE certainty of evidence, will be presented in a single table.

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Editorial and peer-reviewer contributions

Cochrane BJMT supported the authors in the development of this review. Xavier Griffin is a member of the Cochrane BJMT editorial base, but was not involved in the editorial process or decision-making for this review.

The following people conducted the editorial process for this article:

- Sign-off Editor (final editorial decision): Toby Lasserson, Deputy Editor in Chief, Cochrane
- Managing Editor (selected peer reviewers, collated peer-reviewer comments, provided editorial guidance to authors, edited the article): Joanne Elliott, Managing Editor, Cochrane Bone, Joint and Muscle Trauma Group
- Information Specilaist (developed search strategy, advised on search methods): Maria Clarke, Information Specialist, Cochrane Bone, Joint and Muscle Trauma Group
- Methodological Editor (advised on methodology and review content): Kerry Dwan, Statistical Editor, Cochrane
- Copy Editor (copy-editing and production): TBC
- Peer-reviewer (provided comments and recommended an editorial decision): Matthew Costa (clinical reviewer)

Contributions of authors

Conceiving the protocol: SA, XG, IH, AG, WJC
Designing the protocol: All authors
Co-ordinating the protocol: WJC, SA
Designing search strategies: WJC
Developing statistical plan: AG
Writing the protocol: WJC, SA
Securing funding for the protocol: SA

Declarations of interest

SA: none
XG: has ongoing expert consultancy with several companies. XG is fully funded by the NIHR. The views expressed are the author's own and are not necessarily those of the NIHR, NHS or the Department of Health and Social Care. XG was not involved in the editorial process.
IH: none
HA: none
LH: none
AG: none
WJC: none

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Internal sources

- No sources of support provided

External sources

- AOA Research Foundation , Australia

Appendices

Appendix 1. Search strategy for MEDLINE Ovid

1 Radius Fractures/
2 Colles' Fracture/
3 Wrist Injuries/
4 (((distal adj3 (radius or radial)) or colles or smith*2 or barton or wrist) adj3 fracture*).ti,ab.
5 or/1-4
6 Surgical Procedures, Operative/
7 Fracture Fixation/
8 Orthopedic Procedures/
9 Orthopedics/
10 (surg* or operat* or orthop*).ti,ab.
11 (pin* or nail* or screw* or plat* or rod* or wir* or fix* or ORIF or ExFix).ti,ab.
12 or/6-11
13 randomized controlled trial.pt.
14 controlled clinical trial.pt.

15 randomi?ed.ab.
16 placebo.ab.
17 drug therapy.fs.
18 randomly.ab.
19 trial.ab.
20 groups.ab.
21 or/13-20
22 exp animals/ not humans.sh.
23 21 not 22
24 5 and 12 and 23

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