Received: 12 October 2023 | Accepted: 14 October 2023

DOI: 10.1111/aas.14353

REVIEW ARTICLE

acta Anaesthesiologica Scandinavica

Peripheral nerve blocks for closed reduction of distal radius fractures—A protocol for a systematic review

Sanja Pisljagic[1] | Jens L. Temberg[1] | Mathias T. Steensbæk[1] | Sina Yousef[1] | Mathias Maagaard[2] | Lana Chafranska[1] | Kai H. W. Lange[1,3] | Christian Rothe[1] | Lars H. Lundstrøm[1,3] | Anders K. Nørskov[1,2]

[1]Department of Anaesthesiology, Copenhagen University Hospital—North Zealand, Hillerød, Denmark

[2]Centre for Anaesthesiological Research, Department of Anaesthesiology, Zealand University Hospital, Køge, Denmark

[3]Department of Clinical Medicine, University of Copenhagen, Copenhagen, Denmark

Correspondence
Sanja Pisljagic, Department of Anaesthesiology, Copenhagen University Hospital—North Zealand, Hillerød, Dyrehavevej 29, 3400 Hillerød, Denmark.
Email: sanja.spis0008@gmail.com

Funding information
Department of Anaesthesiology at Copenhagen University Hospital - North Zealand, Hillerød

Abstract

Background: Current methods of anaesthesia used for closed reduction of distal radial fractures may be insufficient for pain relief and muscle relaxation, potentially compromising reduction quality and patient satisfaction. Peripheral nerve blocks have already been implemented for surgery of wrist fractures and may provide optimal conditions for closed reduction due to complete motor and sensory blockade of the involved nerves. However, existing literature on peripheral nerve blocks for closed reduction is sparse, and no updated systematic review or meta-analysis exists.

Aims: This protocol is developed according to the PRISMA-P statement. The systematic review and meta-analysis aim to consolidate the literature regarding the effect and harm of peripheral nerve blocks compared with other anaesthesia modalities for closed reduction of distal radius fractures in adults.

Methods: The two primary outcomes are the proportion of participants needing surgery after closed reduction and pain during closed reduction. We will only include randomised clinical trials. Two review authors will each independently screen literature, extract data, and assess risk of bias with Risk of Bias 2 Tool. Meta-analysis will be carried out with Rstudio. We will also perform a Trial Sequential Analysis. The certainty of evidence will be judged using GRADE guidelines.

Discussion: We will use up-to-date methodology when conducting the systematic review outlined in this protocol. The results may guide clinicians in their decision-making regarding the use of anaesthesia for closed reduction of distal radius fractures in adults.

KEYWORDS
brachial plexus block, closed reduction, Colles' fracture, distal radius fracture, peripheral nerve block

1 | INTRODUCTION

Distal radius fractures (DRFs) are among the most common bone fractures, with an increasing incidence.[1–6] DRFs have a bimodal distribution and peak incidence in young men with high-energy trauma and in women ≥60 years with low-energy trauma due to decreased bone density.[1]

National clinical guidelines from the Danish Health Authority recommend 5 weeks of conservative treatment for stable, non-displaced DRFs. The guidelines recommend closed reduction for displaced DRFs, followed by surgery if bone alignment is unsatisfactory.[5]

Closed reduction is performed by manipulating the bone fragments to achieve anatomical position of the fractured bones. Manual or finger trap traction is used for muscle tiring.[7]

Several methods of anaesthesia are proposed for closed reduction, among them is, intravenous regional anaesthesia (IVRA)—currently recommended by The National Institute for Health and Care Excellence guidelines.[8,9] Studies have shown that the anaesthesia method influences pain perception and muscle relaxation during closed reduction, consequently affecting the quality of reduction and a number of attempts.[10–12] Current methods of anaesthesia used for closed reduction of DRFs may be insufficient regarding pain relief and muscle relaxation, potentially compromising the reduction quality and patient satisfaction.[13] Peripheral nerve blocks (PNBs) can provide complete afferent and efferent blockade, are well-established for upper extremity surgery and have a good safety profile.[14,15]

A review from 2002 concluded that the efficacy of closed reduction of DRF remains unknown due to insufficient evidence.[8] A recently published protocol for a systematic review investigated various pain relief methods for closed reduction.[16] However, the present protocol describes a systematic review that specifically examines the role of PNBs in the closed reduction of DRF, including trial sequential analysis (TSA).

1.1 | Objectives

To evaluate the effect and harm of PNBs compared to other anaesthesia modalities for closed reduction of DRFs in adults.

2 | METHODS

This protocol is developed according to the PRISMA-P statement.[17] The final systematic review will be reported following the PRISMA guideline, and the methodology will follow the recommendations of the Cochrane Handbook for Systematic Reviews and Interventions.[18,19]

2.1 | Eligibility criteria

2.1.1 | Types of studies

We will only include randomised clinical trials investigating the use of PNBs for the closed reduction of DRFs with or without ulnar fractures. We will only include studies published in the Latin alphabet. Unpublished trials will be considered if trial data and methodological descriptions are provided in written form or through direct contact with authors. Studies and trials using quasi-randomisation will be excluded.

2.1.2 | Participants

Adults, as defined by trialists, with a DRF requiring closed reduction will be included.

2.1.3 | Types of interventions

Experimental: PNBs such as brachial plexus blocks or distal blocks of individual nerves and combinations of these techniques independent of type, volume, or concentration of local anaesthetic and level of expertise of the physician performing the procedure.

Control: All other methods of anaesthesia, including general anaesthesia, IVRA (Bier block), procedural sedation, local anaesthesia (e.g., haematoma block/local infiltration), and combinations of these.

2.2 | Information sources

2.2.1 | Electronic searches

We will search the following databases from their inception to the present:

1. Medline OVID
2. Embase Ovid
3. The Cochrane Central Register of Controlled Trials (CENTRAL)
4. Cumulative Index to Nursing and Allied Health Literature (CINAHL)
5. Web of Science Core Collection

We have no date restrictions in our search strategy. The search will be conducted within 6 months from the date the systematic review manuscript has been submitted for publication. We will search for ongoing clinical trials and unpublished studies from the following sources:

1. ClinicalTrials.gov
2. The World Health Organization International Clinical Trials Registry Platform (ICTRP)
3. The ISRCTN registry (www.isrctn.com)
4. The Australian New Zealand Clinical Trials Registry (www.anzctr.org.au)
5. EudraCT (https://eudract.ema.europa.eu/)
6. Clinical Trials Information System (CTIS)

2.2.2 | Searching other resources

We will manually search the reference lists of the included trials and any relevant systematic reviews to identify other potentially eligible trials.

2.3 | Search strategy

The search strategy was developed for MEDLINE and will be modified for the other databases. The search strategy was developed with the

help of an information specialist. The search strategy is presented in Appendix A.

2.4 | Data management

2.4.1 | Selection of studies

We will use Covidence (systematic review software) to manage the screening process.[20] Two review authors will each independently screen the titles, abstracts, and relevant full texts. In case of discrepancy, consensus will be reached through joint discussion. A third and senior review author will be involved if a consensus cannot be reached.

2.4.2 | Data extraction

Two review authors will each independently extract the following data (if available) from the included trials:

1. *Basic information*: Author, title, date of publication, and language.
2. *Methodology*: Study design, method of randomisation, intention-to-treat versus per protocol, and loss-to-follow-up.
3. *Participant characteristics*: Sample size, age, gender, type of fracture, comorbidities (such as osteoporosis), frailty score, and criteria for inclusion/exclusion.
4. *Interventions*: Type of anaesthesia, dosages, method of administration, guidance for PNBs (e.g., ultrasound [US] or nerve stimulation), setting, and method of reduction (manual/finger trap traction).
5. *Outcomes*: Primary and secondary outcomes as reported in the trials, duration of follow-up, measurement methods, and units.
6. *Sponsorship*: Funding for the trial and conflict of interests of the trial authors

Data will be extracted into a standardised extraction sheet and converted into the appropriate format in R Studio for meta-analysis.[21]

2.5 | Outcome measures

2.5.1 | Co-primary outcomes

1. The proportion of participants needing DRF surgery after closed reduction

   A patient's need for surgery will be defined by trialists.

2. Pain during closed reduction

   Pain score measured during closed reduction as specified by trial authors, using a visual analogue scale, numeric pain rating scale (NRS), or other means as defined by trial authors.

2.5.2 | Secondary outcomes

1. Patient satisfaction

   Measured at any time point during the recovery period following closed reduction as specified by the trial authors, using the Quality of Recovery Score (QoR, QoR-15, or QoR-40) or other measures as defined by the trial authors.[22–24]

2. Proportion of participants with acceptable post-reduction radiographic fracture position

   Defined by radiographic criteria presented in the AAOS Clinical Practice Guideline: radial shortening <3 mm, dorsal tilt <10°, intraarticular displacement or step off <2 mm.[25] If these are not fully described or differ from the definitions used by the trial authors, we will adhere to the definitions presented in each study.

3. The proportion of participants with one or more serious adverse events (SAEs)
4. The proportion of participants with one or more adverse events (AEs)

   We expect heterogeneity in the reporting of SAEs and AEs. We will primarily extract AEs and SAEs per ICH-GCP definitions.[26] If trial authors do not use the ICH-GCP definition, we will extract events reported as 'serious' or 'severe'. If such definitions are not used, we will decide which reported adverse events we believe to be serious according to the ICH-GCP definition. We will estimate the number of patients with one or more SAEs in two ways:

   1. By choosing the one specific SAE with the highest proportion reported in each trial, we will address the lowest possible proportion of patients with one or more SAEs (aimed at reflecting a best-case scenario).
   2. By cumulating all reported SAEs, assuming patients only experienced one SAE (the number of patients in each group will constitute a maximum), we will address the highest possible reported proportion of patients with one or more SAEs (aimed at reflecting a worst-case scenario).

2.5.3 | Explorative outcomes

1. Wrist function at 3- and 12-months post-reduction, respectively, or at the longest follow-up

   We expect the measurement of function to be heterogeneous across studies. We will measure wrist function using patient-rated wrist evaluation (PRWE), disability of the arm, shoulder and hand questionnaire (DASH), or as defined by the study authors.

   If applicable, we will provide data for the outcome at the longest follow-up in case multiple time points are available.

2.6 | Assessment of risk of bias

Two review authors will each independently assess the risk of bias on the outcome- level following the Risk of Bias Volume 2 tool for randomised trials.[27]

We will assess the following bias domains and judge them to be of 'low', 'some concerns', or 'high' risk of bias:

1. Bias arising from the randomisation process
2. Bias due to deviations from intended interventions
3. Bias due to missing outcome data
4. Bias in measurement of the outcome
5. Bias in selection of the reported result

Each outcome will be judged as having an overall low or high risk of bias. An outcome will only be categorised to have an overall low risk of bias if all domains are judged to have a low risk of bias. An outcome will be categorised as having an overall high risk of bias if one or more domains are judged to have 'some concerns' or 'high risk' of bias. The results will be presented in a Cochrane risk of bias table.

2.7 | Data synthesis

2.7.1 | Measures of treatment effect

To account for the family-wise type 1 error rate, we will adjust the threshold for statistical significance for the co-primary outcomes using a halfway Bonferroni correction. Therefore, we will consider a *p*-value of .033 as the threshold for statistical significance.[28] The threshold for statistical significance ($p < .05$) will not be adjusted for secondary and exploratory outcomes.

For dichotomous outcomes, a risk ratio with corresponding 95% confidence intervals (CI), *p*-value, and TSA-adjusted 95% CIs will be computed. A mean difference with corresponding 95% CI (96.7% for the primary outcomes), *p*-value, and TSA-adjusted 95% CIs will be calculated for continuous outcomes.

When different scales are used for the same outcome, and no relevant conversion is possible, we will calculate the standardised mean difference with the corresponding 95% CI and *p*-value. We will back-transform the result to the most used scale for easing interpretability. The transformation will only be performed on scales that are comparable/homogenous, as described in Chapter 6 of the Cochrane Handbook for Systematic Reviews of Interventions.[29]

Meta-analysis will be performed where effect measures are comparable between at least two studies and where heterogeneity measures indicate that pooling of results is appropriate. Should outcome data prove unfit for meta-analysis, we will aim to summarise effect estimates as described in Chapter 12 of the Cochrane Handbook for Systematic Reviews of Interventions.[30]

2.7.2 | Dealing with missing data

We will contact trial investigators to obtain missing trial data, methodological descriptions, information on outcome measures, and risks of bias components, where necessary, for inclusion in the meta-analysis. If not obtainable, we will calculate missing standard deviations as necessary based on methods outlined in Chapter 6 of the Cochrane Handbook for Systematic Reviews of Interventions.[29] We will not perform imputation of missing data in any primary analyses. We will use imputation for the sensitivity analysis (see below).

2.7.3 | Assessment of heterogeneity

We will primarily assess the heterogeneity across studies by visually inspecting forest plots and calculating I^2 and tau^2 statistics, and we will report results with the highest *p*-value. If unexpected heterogeneity arises, we may perform additional post hoc subgroup analyses.

2.7.4 | Assessment of reporting bias

If 10 or more studies are included, we will create funnel plots and test funnel plot asymmetry to assess small study bias, as described in the Cochrane Handbook for Systematic Reviews of Interventions.[19] As funnel plot asymmetry is not diagnostic of non-reporting bias, other possible causes of asymmetry must be considered when evaluating meta-bias(es).[31] We will identify ongoing and completed trials that have yet to report results by contacting the authors if information is not apparent on the registration platform. For continuous outcomes, we will use the regression asymmetry test, Egger's test, to assess funnel plot asymmetry statistically.[32]

For dichotomous outcomes, we will use the test proposed by either Harbord et al. if $tau^2 \leq 0.1$ or Rücker et al. if $tau^2 > 0.1$, depending on the degree of heterogeneity observed between studies.[33,34]

2.7.5 | Meta-analysis

A meta-analysis using a fixed-effects model and a random-effects model (Sidik–Jonkman tau estimator) will be performed, depending on an overall assessment of the underlying assumptions. We will employ an eight-step method to assess if boundaries of statistical and clinical significance are crossed.[28] We will use RStudio and the meta package for the meta-analysis.[21,35]

2.7.6 | Trial sequential analysis

When doing meta-analysis with sparse data and multiple testing, there is a risk of spurious statistically significant results. To overcome this problem, we will perform TSA to assess all outcomes. We will calculate the required information size in TSA based on a set alpha- and

beta-value as defined below. By conducting TSA, we will adjust the CIs to account for diversity and the accrued information size relative to the required information size.

When calculating the required information size, we will use an alpha of 0.033 for the primary outcomes, 0.05 for secondary/exploratory outcomes, and a beta of 0.10 for all outcomes. For continuous outcomes, we will use the variance and diversity as suggested by the meta-analysis. For the proportion of participants who underwent DRF surgery, we will assume a risk reduction of 20% to be clinically relevant. For pain during closed reduction (NRS 0-10), we will use a minimally clinically important difference (MCID) of 1 point.[36] For patient satisfaction, we will use an MCID of 0.9 for QoR, 6.0 for QoR-15 and 6.3 for QoR-40.[37,38] Currently, there is no adequate estimate of the MCID for the proportion of participants with acceptable radiographic fracture positions. Literature on the effect sizes of PNBs is sparse. Handoll et al. report an incidence of poor closed reduction results in 13/24 patients receiving a haematoma block and in 8/23 patients with IVRA, based on a study by Walther-Larsen et al.[8,39] This translates to a risk reduction of 19.4%. Using IVRA as a close substitute for the brachial plexus blocks and individual distal nerve blocks, we will assume a similar relative risk reduction of 20%.

To our knowledge, there is no MCID for the proportion of participants with one or more AEs and SAEs. Instead, we will assume a value of 25%.

For wrist function, we will use an MCID for PRWE at 11.5 points and DASH at 10.8 points.[40,41]

If trial authors use a measure of treatment effect not mentioned above, we will either try to obtain the MCIDs of the relevant method of measurement or assume one based on the definition provided by the trial authors.

2.7.7 | Sensitivity analysis

We expect to perform the following sensitivity analyses to assess the robustness of the results:

1. Best/worst-case scenario
2. Worst/best-case scenario

In the best-case scenario, we will assume that participants with missing data experienced a positive outcome (i.e., they did not experience adverse events or SAEs and did not need surgery for DRF). For continuous outcomes, we will impute the mean plus or minus '2' standard deviations depending on the direction of a beneficial effect. Conversely, in the worst-case scenario, we will assume the opposite.

2.7.8 | Subgroup analysis

We expect to perform the following subgroup analyses:

1. Evaluation of high versus low risk of biased trials' influence on results
2. Evaluation of guidance methods used in PNBs (US, nerve stimulation), if appropriate
3. Evaluation of plexus brachialis block versus selective PNBs
4. Evaluation of age groups (adults ≥65 years of age vs. adults <65 years of age)

We will calculate a test of interaction and use a *p*-value of .10 as the threshold for statistical differences between subgroups.

2.8 | Meta-bias(es) and confidence in cumulative evidence

We will use principles of the Grades of Recommendation Assessment, Development, and Evaluation (GRADE) to assess the certainty of the body of evidence associated with the studies that contribute data to the outcomes. The GRADE approach will enable us to assess the level of confidence with which we can estimate the association between exposure and outcome. The approach considers:

1. Within study risk of bias (methodological quality)
2. Indirectness of evidence
3. Inconsistency of the data
4. Imprecision of effect estimates
5. Risk of publication bias

In GRADE, there are four levels of certainty of evidence:

1. Very low (the true effect is probably significantly different from the estimated effect)
2. Low (the true effect might be significantly different from the estimated effect)
3. Moderate (the true effect is probably close to the estimated effect)
4. High (the true effect is similar to the estimated effect)

The TSA will guide our judgement of imprecision, depending on whether the accrued information size is less than 50% of the required information size and no boundaries are breached—in this case, we will downgrade by two points due to imprecision. If the accrued information size exceeds 50% of the required information size and no boundaries are breached, we will downgrade by one point. If any boundaries are breached, or the required information size is equal to the accrued information size, we will not downgrade due to imprecision.

Methods and recommendations will be used according to the articles comprising the GRADE guidelines. For example, we will use communicative statements from GRADE guideline 26 to describe our GRADE assessment clearly.[42] We will use the GRADEpro Guideline Development Tool software (GRADEpro GDT).[43]

We will provide clear arguments for all decisions regarding downgrading the certainty of evidence using footnotes. Comments will be included to aid the reader's understanding of the review where necessary. Two authors will each independently judge the certainty of the

evidence, with disagreements resolved by discussion or by involving a third review author.

Our assessment will be provided in a 'Summary of Findings'-table for our primary and secondary outcomes. The table will report the number of trials and participants, absolute and relative effects, and the final assessment of the certainty of evidence for each outcome.

AUTHOR CONTRIBUTIONS

Sanja Pisljagic: Wrote the protocol draft and developed the first draft of the search strategy. **Jens L. Temberg**: Protocol development and general advice. **Mathias T. Steensbæk**: Protocol development and general advice. **Sina Yousef**: Protocol development and general advice. **Mathias Maagaard**: Protocol development, development of the final search strategy, and adaptation to other databases. **Lana Chafranska**: Protocol development and general advice. **Kai H. W. Lange**: Study conception and protocol development. **Christian Rothe**: Study conception and protocol development. **Lars H. Lundstrøm**: Study conception, protocol amendment, and approval of the final version. **Anders K. Nørskov**: Study conception, protocol amendment, and approval of the final version.

ACKNOWLEDGEMENTS

Jette Meelby—for aid in creating the search strategy and adaptation to relevant databases.

FUNDING INFORMATION

The Department of Anaesthesiology at Copenhagen University Hospital—North Zealand, Hillerød, covers all expenses, subject to public audits. None of those involved in the review had economic gains from the study in any way, shape, or form.

CONFLICT OF INTEREST STATEMENT

The authors declare no conflicts of interest.

DATA AVAILABILITY STATEMENT

Data available on request from the authors.

ORCID

Mathias Maagaard https://orcid.org/0000-0002-9037-7295
Kai H. W. Lange https://orcid.org/0000-0003-4510-5888
Lars H. Lundstrøm https://orcid.org/0000-0002-2179-1433
Anders K. Nørskov https://orcid.org/0000-0002-6908-7832

REFERENCES

1. Porrino JA Jr, Maloney E, Scherer K, Mulcahy H, Ha AS, Allan C. Fracture of the distal radius: epidemiology and premanagement radiographic characterisation. *AJR Am J Roentgenol*. 2014;203(3):551-559.
2. Court-Brown CM, Caesar B. Epidemiology of adult fractures: a review. *Injury*. 2006;37(8):691-697.
3. Bergh C, Wennergren D, Möller M, Brisby H. Fracture incidence in adults in relation to age and gender: a study of 27,169 fractures in the Swedish fracture register in a well-defined catchment area. *PLoS One*. 2020;15(12):e0244291.
4. Sørensen S, Larsen P, Korup LR, et al. Epidemiology of distal forearm fracture: a population-based study of 5426 fractures. *Hand*. 2022. doi:10.1177/15589447221109967
5. Sundhedsstyrrelsen. National clinical guideline on the treatment of distal radial fractures. 2017 11 April 2023; Available from: https://www.sst.dk/da/udgivelser/2014/NKR-Behandling-af-haandledsnaere-brud
6. Wong PK, Hanna TN, Shuaib W, Sanders SM, Khosa F. What's in a name? Upper extremity fracture eponyms (part 1). *Int J Emerg Med*. 2015;8(1):75.
7. Søsborg-Würtz H, Corap Gellert S, Ladeby Erichsen J, Viberg B. Closed reduction of distal radius fractures: a systematic review and meta-analysis. *EFORT Open Rev*. 2018;3(4):114-120.
8. Handoll HH, Madhok R, Dodds C. Anaesthesia for treating distal radial fracture in adults. *Cochrane Database Syst Rev*. 2002;2002(3):Cd003320.
9. NICE. Fractures (non-complex): assessment and management. 2016 31 May 2023; Available from: https://www.nice.org.uk/guidance/ng38/chapter/Recommendations#management-in-the-emergency-department
10. Verma RN, Hasnain S, Sreevastava DK, Murthy TVSP. Anaesthetic management of forearm fractures using a combination of haematoma block and intravenous regional anaesthesia. *Med J Armed Forces India*. 2016;72(3):247-252.
11. Koren L, Ginesin E, Elias S, Wollstein R, Israelit S. The radiographic quality of distal radius fracture reduction using sedation versus hematoma block. *Plast Surg*. 2018;26(2):99-103.
12. McQueen M, Duckworth A. Closed re-reduction: is it an alternative. In: Hove L, Lindau T, Hølmer P, eds. *Distal Radius Fractures*. Springer; 2014:121-128.
13. Aniel-Quiroga M, Fruner G, Monge-Baeza A, García-Toledo A, Liñán-Padilla A, Jiménez I. The hematoma block is not enough as method of anesthesia in reduction of displaced distal radius fractures. *Rev Esp Cir Ortop Traumatol*. 2023;67:T371-T377.
14. Brown AR. Anaesthesia for procedures of the hand and elbow. *Best Pract Res Clin Anaesthesiol*. 2002;16(2):227-246.
15. Romanowski AJ, Andersen JH, Toquer P, et al. Risks when applying peripheral nerve blocks. *Ugeskr Laeger*. 2022;184(39):V02220126.
16. Gao JF, Yeo JW, Ho AFW, et al. Anaesthesia and analgesia in the emergency care setting for treating distal radius fractures in adults. *Cochrane Database Syst Rev*. 2022;5. doi:10.1002/14651858.CD015104
17. Moher D, Shamseer L, Clarke M, et al. Preferred reporting items for systematic review and meta-analysis protocols (PRISMA-P) 2015 statement. *Syst Rev*. 2015;4(1):1.
18. Page MJ, McKenzie JE, Bossuyt PM, et al. The PRISMA 2020 statement: an updated guideline for reporting systematic reviews. *BMJ*. 2021;372:n71.
19. Higgins JPT, Green S. *Cochrane Handbook for Systematic Reviews of Interventions*. Cochrane Book Series. 2nd ed. The Cochrane Collaboration; 2019.
20. Covidence Systematic Review Software. Veritas Health Innovation, Melbourne, Australia. Available at www.covidence.org
21. R Core Team. *R: A Language and Environment for Statistical Computing*. R Foundation for Statistical Computing; 2022 https://www.R-project.org/
22. Stark PA, Myles PS, Burke JA. Development and psychometric evaluation of a postoperative quality of recovery score: the QoR-15. *Anesthesiology*. 2013;118(6):1332-1340.
23. Myles PS, Weitkamp B, Jones K, Melick J, Hensen S. Validity and reliability of a postoperative quality of recovery score: the QoR-40. *Br J Anaesth*. 2000;84(1):11-15.
24. Myles PS, Hunt JO, Nightingale CE, et al. Development and psychometric testing of a quality of recovery score after general anesthesia and surgery in adults. *Anesth Analg*. 1999;88(1):83-90.

25. AAOS. *Management of Distal Radius Fractures*. 2020 26 June 2023; Available from: https://www.aaos.org/globalassets/quality-and-practice-resources/distal-radius/drfcpg.pdf
26. ICH. *Guideline for Good Clinical Practice*. 2016 26 June 2023; Available from: https://www.ema.europa.eu/en/documents/scientific-guideline/ich-guideline-good-clinical-practice-e6r2-step-5_en.pdf
27. Sterne JAC, Savović J, Page MJ, et al. RoB 2: a revised tool for assessing risk of bias in randomised trials. *BMJ*. 2019;366:l4898.
28. Jakobsen JC, Wetterslev J, Winkel P, Lange T, Gluud C. Thresholds for statistical and clinical significance in systematic reviews with meta-analytic methods. *BMC Med Res Methodol*. 2014;14(1):120.
29. Higgins J, Li T, Deeks J. Chapter 6: Choosing effect measures and computing estimates of effect. In: Higgins JPT, Thomas J, Chandler J, et al., eds. *Cochrane Handbook for Systematic Reviews of Interventions*, Version 6.3 (updated February 2022). Cochrane; 2022.
30. McKenzie J, Brennan S. Chapter 12: Synthesising and presenting findings using other methods. In: Higgins JPT, Thomas J, Chandler J, et al., eds. *Cochrane Handbook for Systematic Reviews of Interventions*, Version 6.3 (updated February 2022). Cochrane; 2022.
31. Sterne JA, Sutton AJ, Ioannidis JPA, et al. Recommendations for examining and interpreting funnel plot asymmetry in meta-analyses of randomised controlled trials. *BMJ*. 2011;343:d4002.
32. Egger M, Smith GD, Schneider M, Minder C. Bias in meta-analysis detected by a simple, graphical test. *BMJ*. 1997;315(7109):629-634.
33. Harbord RM, Egger M, Sterne JA. A modified test for small-study effects in meta-analyses of controlled trials with binary endpoints. *Stat Med*. 2006;25(20):3443-3457.
34. Rücker G, Schwarzer G, Carpenter J. Arcsine test for publication bias in meta-analyses with binary outcomes. *Stat Med*. 2008;27(5):746-763.
35. Balduzzi S, Rücker G, Schwarzer G. How to perform a meta-analysis with R: a practical tutorial. *Evid Based Ment Health*. 2019;22:153-160. doi:10.1136/ebmental-2019-300117
36. Kendrick DB, Strout TD. The minimum clinically significant difference in patient-assigned numeric scores for pain. *Am J Emerg Med*. 2005;23(7):828-832.
37. Myles PS, Myles DB, Galagher W, Chew C, MacDonald N, Dennis A. Minimal clinically important difference for three quality of recovery scales. *Anesthesiology*. 2016;125(1):39-45.
38. Myles PS, Myles DB. An updated minimal clinically important difference for the QoR-15 scale. *Anesthesiology*. 2021;135(5):934-935.
39. Walther-Larsen S, Christophersen D, Fauner M, Varmarken JE. Intravenous regional analgesia compared to infiltration analgesia in the reduction of distal forearm fractures. *Ugeskr Laeger*. 1988;150(32):1930-1932.
40. Franchignoni F, Vercelli S, Giordano A, Sartorio F, Bravini E, Ferriero G. Minimal clinically important difference of the disabilities of the arm, shoulder and hand outcome measure (DASH) and its shortened version (QuickDASH). *J Orthop Sports Phys Ther*. 2014;44(1):30-39.
41. Walenkamp MM, de Muinck Keizer R-J, Goslings JC, et al. The minimum clinically important difference of the patient-rated wrist evaluation score for patients with distal radius fractures. *Clin Orthop Relat Res*. 2015;473(10):3235-3241.
42. Santesso N, Glenton C, Dahm P, et al. GRADE guidelines 26: informative statements to communicate the findings of systematic reviews of interventions. *J Clin Epidemiol*. 2020;119:126-135.
43. GRADEpro GDT. *GRADEpro Guideline Development Tool [Software]*. McMaster University and Evidence Prime; 2022 Available from gradepro.org

How to cite this article: Pisljagic S, Temberg JL, Steensbæk MT, et al. Peripheral nerve blocks for closed reduction of distal radius fractures—A protocol for a systematic review. *Acta Anaesthesiol Scand*. 2023;1-7. doi:10.1111/aas.14353

APPENDIX A

A.1 | SEARCH STRATEGY OVID MEDLINE(R) ALL < 1946 TO JUNE 28, 2023>

1 (suprascapular approach or (suprascapular adj2 block*) or cubital approach or (cubital adj2 block*) or ulnar approach or (ulnar adj2 block*) or median approach or (median adj2 block*) or radial approach or (radial adj2 block*) or axillary approach* or (axillary adj2 block*) or retroclavicular approach or (retroclavicular adj2 block*) or infraclavicular approach or (infraclavicular adj2 block*) or supraclavicular approach or (supraclavicular adj2 block*) or interscalene approach or (interscalene adj2 block*) or (brachial plexus adj2 block*) or (peripheral adj3 block*) or (nerve adj2 block*) or regional analgesia* or regional an $estesia or "regional anesthetics").ab,ti. 23,281

2 exp nerve block/ 25943

3 exp anesthesia, local/ 18,326

4 1 or 2 or 3 54,768

5 (remanipulation or fracture manipulation or "initial reduction" or reduction fracture or orthopedic reduction or nonsurgical or non-operative or reposition* or finger trap or manual therapy or "manual traction" or conservative management or conservative therapy or conservative treatment or "closed reduction" or forearm fracture or forearm injury or forearm instability or wrist instability or barton fracture or smith fracture or colles fracture or upper extremity trauma or upper extremity injur* or upper extremity fracture or wrist trauma or wrist injury or distal radial fracture or ulna fracture or radial fracture).ab,ti. 117,038

6 exp orthopedic surgery/ 359,355

7 5 or 6 460,888

8 randomised controlled trial.pt. 595,381

9 controlled clinical trial.pt. 95,347

10 randomi?ed.ab. 726,066

11 drug therapy.fs. 2,602,414

12 randomly.ab. 411,256

13 trial.ab. 653,774

14 groups.ab. 2,535,633

15 placebo.ab. 239,378

16 8 or 9 or 10 or 11 or 12 or 13 or 14 or 15 5,711,846

17 exp animals/not humans.sh. 5,133,797

18 16 not 17 4,987,039

19 4 and 7 and 18 **2125**