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[Intervention Protocol]

Telerehabilitation versus conventional face-to-face land-based exercises following hip or knee arthroplasty

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ABSTRACT

Objectives

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

To assess the benefits and harms of land-based exercises provided via telerehabilitation with conventional face-to-face land-based exercises on pain, function, gait, and quality of life after hip or knee arthroplasty.

BACKGROUND

Description of the condition

Hip and knee arthroplasties are surgical replacements of the respective joints and the two of the most commonly performed orthopaedic surgeries worldwide. They are especially indicated in hip or knee end-stage osteoarthritis with severe radiological changes, persistent pain, limited range of motion, reduced function and quality of life, and failure of conservative therapy delivered for six months (Gademan 2016; Hunter 2019). The medical cost of osteoarthritis has been estimated to account for between 1% and 2.5% of high-income countries' gross domestic product, with hip and knee joint arthroplasty representing the greatest proportion of these healthcare costs (Hunter 2019). According to the American Joint Replacement Registry (AJRR) Annual Report on Hip and Knee Arthroplasty Data 2020 (AJRR 2020), between 2012 and 2019, over 1.8 million hip and knee arthroplasties were performed in the USA. The National Joint Registry (NJR) for England, Wales, Northern Ireland, and the Isle of Man 2020 documented more than 2.4 million arthroplasties between 2003 and 2019 (NJR 2020), and the Australian Orthopaedic Association National Joint Replacement Registry (AOANJRR) documented over 1.5 million arthroplasties between 1999 and 2019 (AOANJRR 2020). In these registries, the number of procedures is increasing every year.

Arthroplasties can vary based on the type of surgical procedure (primary or a revision replacement), the approach, and the type of replacement used for the hip and knee joints. The procedure is called 'primary replacement' when a new hip or knee prosthesis is implanted and called 'revision replacement' when the old device is replaced with a new one. The approach can be 'standard traditional' or 'minimally invasive'; some of the most common traditional approaches for the hip are posterolateral or posterior, direct lateral, anterolateral, direct anterior, and transtrochanteric (laterally on the hip); and medial parapatellar, subvastus, midvastus (all anterior on the knee), and lateral for the knee. The type of replacement can be total, partial, or resurfacing for the hip, and unicompartmental, bicompartamental, or tricompartmental for the knee.

Description of the intervention

The conventional face-to-face home-based and inpatient/outpatient rehabilitation have shown satisfactory effectiveness and safety, optimising physical activity postsurgery and capable of enhancing the clinical and social benefits of the surgery (Arnold 2016; Buhagiar 2017; Di Monaco 2009; Lowe 2007; Mahomed 2008). Therapeutic exercises are commonly prescribed following a hip or knee arthroplasty to maximise functional outcomes (Artz 2015). Physical therapists have been utilising therapeutic land-based exercises since the profession's conception, and they have been demonstrated to be fundamental in improving function, performance, and disability (Saunders 2007). A specialised postsurgical rehabilitation is required early and for several months after the surgery to achieve successful outcomes (MAS 2005; Sattler 2019). In this context, the growing demand for hip or knee replacement rehabilitation can increase costs and waiting lists, becoming a threat to the sustainability of healthcare services. Moreover, delivering rehabilitation becomes difficult for patients in remote areas, vulnerable groups, and ageing populations (WHO 2016). The time and costs of transportation and even the risk of falling or crashing can also be barriers to conventional outpatient

face-to-face rehabilitation. Thus, there is a need to find alternative hip or knee arthroplasty rehabilitation methods that minimise or solve these problems. In this context, telerehabilitation, which is a technology-based approach, provides a new paradigm for the delivery of rehabilitation services.

Telerehabilitation can be defined as the provision of a rehabilitation service at a distance using telecommunications technology as a delivery medium which is real-time (or synchronous), implying that all users (health professionals and patients) involved can exchange information instantaneously, through mediums such as telephones, virtual reality, or video-conferencing platforms (Theodoros 2008). This encompasses various services such as assessment, monitoring, intervention, supervision, education, consultation, and counselling (Brennan 2010). When it involves exercises, telerehabilitation has been described as a technology-based approach that provides physical activity and uses common household equipment (e.g. towels, chair, and tape measure), assessment before and after exercise, supervised exercises (e.g. aerobic-based, strength, balance, flexibility, co-ordination, or endurance activities, or combinations thereof), home exercises for days without supervised sessions, and suggestions for pain control, walking aids, and the return to activities (Antón 2016; Bini 2017; Campbell 2019; Eichler 2017; Eisermann 2004; Hardt 2018; Moffet 2015; Moffet 2017; Piqueras 2013; Russell 2003; Russell 2011; Sharareh 2014; Tousignant 2011; Wang 2018). The exercise's intensity, difficulty level, duration, and other therapeutic parameters for each study have been standardised and adjusted based on each patient's tolerance and needs (Antón 2016; Bini 2017; Campbell 2019; Eichler 2017; Eisermann 2004; Hardt 2018; Moffet 2015; Moffet 2017; Piqueras 2013; Russell 2003; Russell 2011; Sharareh 2014; Tousignant 2011; Wang 2018).

How the intervention might work

Exercise is a common, non-pharmacological, evidence-based intervention that has shown changes in biomarkers involved in neurotransmission and inflammation, suggesting anti-inflammatory (Gleeson 2011) and hypoalgesic effects (Kawi 2016), and may be able to reverse signs of neuropathic pain and increased endogenous opioid content in brainstem regions important in pain modulation (Stagg 2011). Moreover, acute exercise has a hypoalgesic effect on experimentally induced pain in healthy adults and both a hypoalgesic and hyperalgesic effect in adults with chronic pain (Naugle 2012; Wewege 2021). Despite the evidence from guidelines (AAOS 2017; Cibulka 2017; Fernandes 2013; Kolasinski 2020; McGrory 2016), and the associations with inflammatory markers and neurotransmitter-related/metabolite-detecting genes may imply a hypoalgesic effect of exercise, the effect mechanism during the rehabilitation is not understood.

Telerehabilitation has been growing rapidly as an alternative or complementary therapy via conventional face-to-face land-based therapy care services. Although there are some challenges such as the quality of service of the telecommunication network used in the community, technical difficulties with the in-home installation of equipment, and patient safety, telerehabilitation may offer a possible solution to meet the growing demands of patient care for hip or knee replacement rehabilitation. It could potentially enable the equitable delivery of health services. Regarding the advantages of telerehabilitation, the lack of access to appropriate hip or knee replacement rehabilitation may be potentially overcome by using real-time telerehabilitation. This can improve the accessibility of

rehabilitation services (and specialised professionals) in rural and remote communities. Another potential advantage is eliminating transportation time and costs for both the patient and therapist and even the risk of falling for some patients. The costs of delivering telerehabilitation after hip or knee replacement have been studied (Bini 2017; Campbell 2019; Hardt 2018; Tousignant 2015). The study conducted in Canada showed that cost savings from the healthcare system could range from 18% to 123%, depending on the travel time from the health centre to the patient's home. In general, telerehabilitation proved to be less expensive than in-home rehabilitation, especially when the distance between the health centre and the patient's residence was greater (Tousignant 2015). Moreover, telerehabilitation also showed a significant difference in the total number of visits to physical therapy (Bini 2017), and the total number of calls to the clinic (Campbell 2019), but no significant difference in the length of hospital stay (Hardt 2018). Finally, telerehabilitation can also reduce the time until the first rehabilitation session after hospital discharge, favouring assessment, monitoring, intervention, supervision, education, consultation, and counselling at a distance, and still improving their adherence to treatment and promoting a considerable number of exercise sessions compared to conventional outpatient face-to-face land-based rehabilitation (Galea 2019).

Why it is important to do this review

Despite the numerous advantages of telerehabilitation, robust evidence on their benefits after hip and knee replacements are still scarce. Since the first scientific publication on telerehabilitation in the 1990s (Burns 1998), the research on the topic has increased, supported mainly by people's emerging needs and the development of new communication and computer technologies (Peretti 2017). However, despite the growing number of telerehabilitation services worldwide, evidence on its clinical effectiveness and safety is still emerging. In this context, Cochrane has published four reviews (Bittner 2020; Cox 2021; Khan 2015; Laver 2020) and three protocols (Almeida 2020; Fandim 2021; Saragiotto 2020) on telerehabilitation, but none on orthopaedic postsurgical treatment.

Despite the potential benefits of telerehabilitation and its role in maintaining and improving the continuity of rehabilitation care and services due to its potential ability to increase service efficiency and contain costs, there are still many uncertainties about its effectiveness and safety, especially for hip or knee replacement. This information could potentially have a positive impact on consumers and stakeholders and influence the delivery of postsurgical treatment for hip or knee arthroplasty in several hospitals worldwide. We intend to conduct a Cochrane Review to inform patients, practitioners, and policy makers about the effectiveness of telerehabilitation programmes for patients following hip or knee arthroplasty.

This review will be conducted according to the Cochrane Musculoskeletal Group Editorial Board (Ghogomu 2014).

OBJECTIVES

To assess the benefits and harms of land-based exercises provided via telerehabilitation with conventional face-to-face land-based exercises on pain, function, gait, and quality of life after hip or knee arthroplasty.

METHODS

Criteria for considering studies for this review

Types of studies

We will include randomised controlled trials (RCTs) reported as full text, published as abstract only, and unpublished data. There will be no language or publication status restrictions.

Types of participants

We will include adults aged 18 years or older who underwent any postsurgical land-based exercises provided via telerehabilitation for any type of hip or knee replacement, alone or combined with other treatments, as long as separate data were provided for joints and treatments. We will only consider mixed interventions (combination of face-to-face and telerehabilitation) if the telerehabilitation component was at least 75% of the total intervention. We will include studies that have a subset of eligible participants if they report outcomes for eligible participants separately, or at least 80% of the study population consists of relevant participants.

Types of interventions

Any postsurgical face-to-face land-based exercises following hip or knee replacement, alone or combined with other treatments will be included. We will include studies with the following comparators after hip and knee replacement:

- land-based exercises provided by telerehabilitation versus conventional face-to-face land-based exercises;
- land-based exercises provided by telerehabilitation versus usual care (self-directed exercise, no intervention).

The land-based exercises for both telerehabilitation and conventional face-to-face can be general or specific to increase physical activity levels. Land-based exercises can be delivered alone or combined with other treatments, as long as co-interventions are equally given to all study groups.

In the case of mixed interventions (a combination of conventional land-based face-to-face and telerehabilitation), we will consider as one or the other intervention if the component is at least 80% of the total intervention. In this case, we will carry out a sensitivity analysis to assess the impact of this decision on the review's findings. Otherwise, we will only make a descriptive report of the data.

Telerehabilitation can be performed by services over telecommunication networks or the Internet, synchronous (in real-time) or asynchronous (not in real-time), telephone calls, telephone/Internet text/image messages, synchronous video-conferencing, asynchronous multimedia online, and other ways, alone or combined with other treatments, as long as separated data were provided for joints and treatments.

Types of outcome measures

Major outcomes

- Pain intensity.
- Function.
- Health-related quality of life.

- Gait performance.
- Patient-reported satisfaction or global perceived effect.
- Withdrawals due to adverse events related to rehabilitation exercises.

We will consider the outcomes indicated by the Outcome Measures in Rheumatology (OMERACT) Total Joint Replacement (TJR) Working Group (WG) core domain set for hip/knee TJR trials (Singh 2017a; Singh 2017b), present with core outcome set in the COMET Database (COMET Initiative 2021). However, we adapted this information to our review because these outcomes refer to the surgery itself and not necessarily postsurgical exercises. If there is more than one measure for an outcome, we will give preference to:

- pain intensity (in the following order of preference: pain overall, pain on walking, Western Ontario and McMaster Universities Osteoarthritis Index (WOMAC) pain subscale, pain on activities other than walking, WOMAC global scale, Lequesne Osteoarthritis Index Global Score, other algofunctional scales, patient's global assessment, physician's global assessment, other outcomes, no continuous outcome reported);
- function (in the following order of preference: global disability score, walking disability, WOMAC disability subscore, composite disability scores other than WOMAC, disability other than walking, WOMAC global scale, Lequesne Osteoarthritis Index Global Score, other algofunctional scales);
- health-related quality of life (in the following order of preference: 36-Item Short-Form Health Survey (SF-36) (mental component summary), 12-Item Short-Form Health Survey (SF-12) (mental component summary), EuroQol questionnaires (EQ-5D), Veterans RAND 36-Item Health Survey (VR-36), Veterans RAND 12-Item Health Survey (VR-12), other algofunctional scales);
- gait performance (timed up and go test (seconds), six-minute walk test (metres), 10-m walk test (seconds), 40-m fast-paced walk test (seconds), gait parameters such as speed (centimetres/second), cadence (steps/minute), and step length (centimetres), self-paced walk test (seconds), stair tests (seconds), stair climb test (seconds), five-times-sit-to-stand test (seconds), 30-second chair stand test (number of sit-to-stands), chair stand test (number), or others evaluated with validated instruments);
- patient-reported global perceived effect (in the following order of preference: Global Perceived Effect Scales, Patient Global Impression of Change, other algofunctional scales);
- withdrawals due to adverse events related to rehabilitation exercises such as fatigue, increase in pain, and joint swelling (expressed in percentages or number of events).

These major outcomes will form the basis of the GRADE assessment and be summarised in the review's abstract and other summary formats.

Minor outcomes

- Range of motion.
- Isometric lower limb muscle strength.
- Intervention adherence.
- Self-efficacy.
- Costs of delivering the intervention.

We will report results in additional tables. If there is more than one measure for an outcome, we will give preference to:

- range of motion (active, using a goniometer, dynamometer, or any other valid instruments as reported in the study. If only passive range of motion is reported, we will use that);
- isometric lower limb muscle strength (hip abduction and knee flexion and extension in both joints, evaluated in the following order of preference: isokinetic dynamometry, hand-held dynamometry, manually through Medical Research Council (MRC) Scale, other valid instruments);
- intervention adherence (expressed in percentages or number of participants);
- self-efficacy (in the following order of preference: Arthritis Self-Efficacy Scale, Pain Self-Efficacy Questionnaire, Chronic Pain Self-Efficacy Scale, other algofunctional scales);
- costs of delivering the intervention.

The major and minor outcomes will not be used as a criterion for including studies in this review.

Timing of outcome assessments

The three time points considered for all outcomes will be the following: short-term (three months or less after randomisation), intermediate-term (more than three months to six months or less after randomisation), and long-term (more than six months to less than 12 months after randomisation). If there are multiple time points classified within the same category, we will use the one closest to three months for the short-term, closest to six months for the intermediate-term, and closest to 12 months for the long-term follow-up time point. The primary time point will be postintervention.

Search methods for identification of studies

Electronic searches

We will search the Cochrane Central Register of Controlled Trials (CENTRAL), MEDLINE via Ovid, Embase via Ovid, Latin American and Caribbean Health Sciences Literature (LILACS), and Physiotherapy Evidence Database (PEDro). We will also search the US National Institutes of Health Ongoing Trials Register ClinicalTrials.gov (clinicaltrials.gov/) and the World Health Organization (WHO) International Clinical Trials Registry Platform (ICTRP) portal (trialsearch.who.int) for ongoing trials. We will search all databases from their inception to the present, and we will apply no restrictions on the date, language, or publication status. We will also search MEDLINE, Embase, and LILACS for any relevant retraction statements and errata for information from included studies and report the date this was done within the review.

The complete search strategy for MEDLINE via Ovid can be found in [Appendix 1](#). It will be translated appropriately as closely as possible across the other databases.

We will rerun searches for all relevant databases within six months before the publication of the review and screen the results for potentially eligible studies. If the studies meet the eligibility criteria, we will incorporate them in the review. If not, we will report the potentially eligible studies as studies awaiting classification (or ongoing studies if they have not yet been completed).

Searching other resources

We will check reference lists of all primary studies and previous systematic reviews on the same topic.

Data collection and analysis

Selection of studies

Two review authors (DJAA and GSR) will independently screen titles and abstracts to include all potentially relevant studies we identify by the search and code them as 'retrieve' (eligible or potentially eligible/unclear) or 'do not retrieve'. We will retrieve the full-text study reports/publication. Two review authors (DJAA and GSR) will independently screen the full text, identify studies for inclusion, and identify and record at least one explicit reason for excluding the ineligible studies using Covidence ([Covidence](#)). We will resolve any disagreements through discussion, or if required, we will consult a third review author (DGC or VFMT). We will identify and exclude duplicates and collate multiple reports of the same study so that each study, rather than each report, is the unit of interest in the review. Any records identified through the search that involves members of the review team will be handled by team members who were not involved with the relevant study to avoid perceived conflicts of interest. We will record the selection process in sufficient detail to complete a PRISMA flow diagram ([Page 2021a](#)) and characteristics of excluded studies table.

Data extraction and management

We will use a structured data collection form for study characteristics and outcome data, piloted on at least one study in the review. We will use Covidence to manage the screening and data extraction processes ([Covidence](#)). Two review authors (DJAA and GSR) will independently extract study characteristics and outcome data from the included studies. We will resolve any disagreements through discussion, or if required, we will consult a third review author (DGC or VFMT). We will collect the information of the included studies in sufficient detail to populate a characteristics of included studies table. We will extract the following information.

- Methods: study design, total duration of the study, details of any 'run-in' period, number of study centres and location, study setting, withdrawals, and date of study.
- Participants: number, mean age, age range, sex, disease duration, severity of the condition, diagnostic criteria, important comorbidities baseline data, inclusion criteria, and exclusion criteria. For both joints, we will extract information about peculiarities related to the surgical procedure and the prosthesis, such as if the procedure is a primary or a revision replacement, the approach, and the type of replacement.
- Interventions: intervention, comparison, concomitant medications, and excluded medications. We will use the Consensus on Exercise Reporting Template (CERT) ([Slade 2016](#)) and the Template for Intervention Description and Replication (TIDieR) checklist and guide ([Hoffmann 2014](#)) to improve the reporting of interventions and make it easier for authors to structure accounts of their interventions, reviewers, and editors to assess the descriptions, and readers to use the information.
- Outcomes: major and minor outcomes specified and collected, and time points reported.
- Characteristics of the design of the trial as outlined in the [Assessment of risk of bias in included studies](#) section.

- Notes: funding for the trial and notable declarations of interest of trial authors.

We will extract the number of events and number of participants per treatment group for dichotomous outcomes, and means, standard deviations, and the number of participants per treatment group for continuous outcomes. If some statistics are not reported, we will use Review Manager 5 to input them (e.g. the calculation of standard deviations using confidence intervals (CI) or P values) ([Review Manager 2020](#)).

Two review authors (DJAA and NCJ) will extract the data from graphs or figures, if necessary, using WebPlotDigitizer software ([WebPlotDigitizer 2020](#)). We will resolve any disagreements by consensus or by involving a third review author (DGC or VFMT). In the characteristics of included studies table, we will note if outcome data were not reported in a usable way and when data were transformed or estimated from graphs or figures.

One review author (NCJ) will transfer data into Review Manager 5 ([Review Manager 2020](#)). To check for potential problems such as typographical errors in studies' reports, accuracy of data collection and manipulation, and data entry into Review Manager 5, two review authors (DJAA and GSR) will independently check if data are entered correctly by comparing the systematic review data with the study reports. We will resolve disagreements by consensus. No study data will be extracted or analysed by review members directly involved with the included studies.

We will select data to extract (including the cases of multiple outcome reporting) based on the following decision rules:

- preference to change scores if both change and endpoint values are available;
- result corresponding to a full intention-to-treat (ITT) analysis;
- result corresponding to an analysis (sometimes described as a 'modified intention-to-treat' (mITT) analysis) that adheres to ITT principles except that participants with missing outcome data are excluded;
- result corresponding to an 'as treated' or naïve 'per-protocol' analysis, or an analysis from which eligible trial participants were excluded.

Assessment of risk of bias in included studies

Two review authors (DJAA and VTC) will independently assess the risk of bias for each study using the Revised Cochrane risk of bias tool for randomised trials (RoB 2) ([Sterne 2019](#)), as outlined in Chapter 8 of the *Cochrane Handbook for Systematic Reviews of Interventions* ([Higgins 2021a](#)). We will resolve disagreements through discussion or, if required, by involving a third review author (DGC or VFMT). To improve the reliability of assessments, we will pilot the RoB 2 tool on a sample of three to six articles ([Sterne 2019](#)).

The RoB 2 tool is structured into five domains through which bias might be introduced into the result ([Sterne 2019](#)). The five domains for individually randomised trials are:

- bias arising from the randomisation process;
- bias due to deviations from intended interventions;
- bias due to missing outcome data;
- bias in the measurement of the outcome;
- bias in the selection of the reported result.

The possibility of identifying cluster-randomised trials is small. However, if we find cluster-randomised trials, we will include an additional domain ("1b. bias arising from identification or recruitment of individual participants within clusters") (Sterne 2019). For cross-over RCTs, we will only include data from the first phase, precluding the need to use the cross-over RCT version of RoB 2. We will use the standard version of RoB 2 (Sterne 2019).

For signalling questions within each domain for each outcome, we will provide one of the five possible answers in the RoB 2 tool (Sterne 2019) ('Yes', 'Probably yes', 'No', 'Probably no', and 'No information'). We will address the conflict of interests in the included trials and reflect on the possible impact on differences in study design, risk of bias in trial results, and risk of bias in synthesis results. We will report all concerns about this in the characteristics of included studies table. After this step, we will judge each domain according to the algorithm result as 'Low risk of bias', 'Some concerns', or 'High risk of bias' for each outcome. We will justify the judgements of risk of bias (high, low, and some concerns) and provide this information in the risk of bias table (as 'Support for judgement'). The overall risk-of-bias judgement for each outcome will be the least favourable assessment across the domains. We will consider blinding separately for different key outcomes if necessary. In addition, we will consider the impact of missing data by key outcomes. We will note this in the risk of bias table, where information on the risk of bias relates to unpublished data or correspondence with a trialist.

We will assess the risk of bias of all major outcomes at the postintervention time point for each study. The effect of principal interest will be the assignment to the interventions at baseline (regardless of whether the interventions are received during follow-up, known as the 'ITT effect'), except for adherence outcome (the 'per-protocol effect'). When considering treatment effects, we will consider the risk of bias for the studies that contribute to that outcome.

When the effect of interest is that of assignment to intervention, we will select the trial result included in the meta-analysis and assessed for risk of bias according to the following order of preference:

- the result corresponding to a full ITT analysis;
- the result corresponding to an analysis (sometimes described as a mITT analysis) that adheres to ITT principles except that participants with missing outcome data are excluded;
- a result corresponding to an 'as treated' or naïve 'per-protocol' analysis, or an analysis from which eligible trial participants were excluded.

Assessment of bias in conducting the systematic review

We will conduct the review according to this published protocol and report any deviations in the 'Differences between protocol and review' section of the systematic review.

Measures of treatment effect

We will analyse dichotomous data as risk ratios or Peto odds ratios when an outcome is a rare event (approximately less than 10%) and use 95% CIs. We will analyse continuous data as mean difference (MD) when studies use the same scale or standardised mean difference (SMD) when studies use different scale (e.g. health-related quality of life) to measure an outcome and 95% CIs. We

will enter data presented as a scale with a consistent direction of effect across studies. SMDs will be back-translated to a typical scale (e.g. 0 to 10 for pain intensity) by multiplying the SMD by a typical among-person standard deviation (e.g. the standard deviation of the control group at baseline from the most representative trial), as outlined in Chapter 15 of the *Cochrane Handbook for Systematic Reviews of Intervention* (Schünemann 2021a).

For dichotomous outcomes, we will calculate the absolute percent change from the difference in the risks between the intervention and control groups using GRADEpro GDT (GRADEpro GDT), and expressed as percentages. We will calculate the number needed to treat for an additional beneficial outcome (NNTB) or the number needed to treat for an additional harmful outcome (NNTH) from the control group event rate and the relative risk using the Visual Rx NNT calculator (Cates 2008). We will calculate NNTB or NNTH for outcomes showing a clinically significant benefit/harm.

Rather than using minimal clinically important differences (MCIDs), we will judge the magnitude of the effect based on between-mean group differences for continuous measures based upon a similar approach used in the American College of Physicians 2017 guidelines for low back pain (Chou 2017). For pain measured on a 0- to 10-point scale, a difference of 0.5 to 1 points is considered slight to small, a difference of more than 1 to 2 points is considered moderate, and a difference of more than 2 points is considered large. Similarly, for function and health-related quality of life measured on a 0- to 100-point scale, a difference of 5 to 10 points is considered slight/small, a difference of more than 10 to 20 points is considered moderate, and a difference of more than 20 points is considered large.

In the 'Effects of interventions' results section and the 'What happens' column of the summary of findings table, we will provide the absolute percent change, the relative percent change from baseline (dichotomous outcomes), and the NNTB or NNTH (the NNTB or NNTH will be provided only if the outcome shows a difference between groups) for dichotomous outcomes.

Unit of analysis issues

The unit of analysis will be preferably the participant, as in the unilateral surgery cases, and in the case of bilateral surgery if the results were not reported for each side separately. In other cases, the unit of analysis will be the knee or hip. If multiple trial groups are reported in a single trial, we will include only the relevant groups. If two comparisons are combined in the same meta-analysis, we will halve the control group to avoid double-counting (e.g. two groups of different interventions via telerehabilitation and one waiting-list control). We will clarify in the 'Characteristics of included studies' table that more than two intervention groups were present in the study.

If we identify studies that included both joints (hip and knee) in the analysis of cluster-randomised trials, we will multiply the standard error of the effect estimate (from an analysis ignoring clustering) by the square root of the design effect (inflated variances), according to the method described in Chapter 23 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2021b). The meta-analysis using the inflated variances will be performed using the generic inverse variance method.

Dealing with missing data

We will contact study authors up to two times when additional information such as key study characteristics or numerical outcome data is required. If no reply is received within four weeks, we will consider the data unobtainable. If this is not possible, and the missing data are thought to introduce serious bias, we will explore the impact of including such studies in the overall assessment of results by a sensitivity analysis. We will clearly describe any assumptions and imputations to handle missing data, and sensitivity analyses will explore the effect of imputation.

We will use two imputation methods (in case the authors do not provide the missing data) for all major and minor outcomes: 'best-worst-case' scenario (assuming that all missing data in the intervention group are related to beneficial outcomes and all missing data in the control group are related to harmful outcomes) and 'worst-best-case' scenario, assuming that all missing data in the intervention group are related to harmful outcomes and all missing data in the control group are related to beneficial outcomes. For continuous outcomes, a 'beneficial outcome' will be the group mean plus two standard deviations of the group mean, and a 'harmful outcome' will be the group mean minus two standard deviations of the group mean. We will consider for both cases all randomised participants in the denominator (Jakobsen 2014). For dichotomous outcomes (e.g. number of withdrawals due to adverse events), we will calculate the withdrawal rate using the number of participants randomised in the group as the denominator. For continuous outcomes (e.g. mean change in pain intensity score), we will calculate the MD or SMD based on the number of participants analysed at that time point. If the number of participants analysed is not presented for each time point, we will use the number of randomised participants in each group at baseline. If possible, missing standard deviations will be computed from other statistics such as standard errors, CIs, or P values. If standard deviations cannot be calculated, we will impute them (e.g. from other studies in the meta-analysis). We will follow the recommendations of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2021c).

Assessment of heterogeneity

To determine whether a meta-analysis is appropriate, we will assess clinical and methodological diversity in terms of participants, interventions, outcomes, and study characteristics for the included studies using data from the data extraction tables. We will assess statistical heterogeneity by visual inspection of the forest plot to assess for apparent differences in results between the studies and using the I^2 and χ^2 statistical tests.

The interpretation of an I^2 statistic will be as recommended in Chapter 10 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Deeks 2021): 0% to 40% might 'not be important'; 30% to 60% may represent 'moderate' heterogeneity; 50% to 90% may represent 'substantial' heterogeneity; 75% to 100% represents 'considerable' heterogeneity. We will keep in mind that the importance of the I^2 statistic depends on the magnitude and direction of effects and the strength of evidence for heterogeneity (e.g. P value from the χ^2 test, or a CI for the I^2 statistic: uncertainty in the value of the I^2 statistic is substantial when the number of studies is small).

We will interpret the χ^2 test at a P value of 0.10 or less to indicate evidence of statistical heterogeneity. If we identify substantial

heterogeneity, we will report it and investigate possible causes by following Chapter 10 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Deeks 2021) recommendations.

Assessment of reporting biases

We will create and examine a funnel plot to explore possible small-study biases. In interpreting funnel plots, we will examine the different possible reasons for the plot asymmetry as outlined in Chapter 13 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Page 2021b), and correlate this to the review results. If we can pool more than 10 trials, we will undertake formal statistical tests to investigate funnel plot asymmetry and follow the recommendations in Chapter 13 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Page 2021b). Furthermore, we will check trial protocols against published reports. For studies published after 1 July 2005, we will screen the WHO ICTRP portal (trialsearch.who.int) for the a priori trial protocol. We will evaluate whether selective reporting of outcomes is present.

Data synthesis

We will undertake meta-analyses only if this is meaningful (e.g. if participants, interventions, comparisons, and outcomes are judged to be sufficiently similar to ensure a clinically meaningful answer). We will use a random-effects model and perform a sensitivity analysis with the fixed-effect model. If meta-analysis is not possible, we will use alternative synthesis methods, such as the summary of effect estimates (e.g. median, interquartile range with box-and-whisker plots) or the combination of P values if there is no, or minimal, information reported on the direction of effect as outlined in Chapter 12 of the *Cochrane Handbook for Systematic Reviews of Interventions* (McKenzie 2021). We will present data for the hip and knee replacements separately as two primary stratified analyses and not as subgroups.

Subgroup analysis and investigation of heterogeneity

We plan to carry out the following subgroup analyses (if the information is available) on pain, function, and self-efficacy:

- type of replacement (primary or revision) for hip and knee;
- age group (less than 65 years or 65 years or older) to explore differences in the use of telerehabilitation in older people as hip and knee replacements are more common in the older population;
- telerehabilitation modality (e.g. telephone/Internet text/image messages, synchronous video-conferencing, asynchronous multimedia online, and other methods).

We will use the formal test for subgroup interactions in Review Manager 5 (Review Manager 2020), and will use caution in the interpretation of subgroup analyses as advised in Section 10 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Deeks 2021). We will compare the magnitude of the effects between the subgroups by means of assessing the overlap of the CIs of the summary estimate. Non-overlap of the CIs indicates statistical significance.

Sensitivity analysis

To investigate the robustness of the treatment effect for pain intensity, function, and patient-reported satisfaction or global perceived effect for the postintervention time point for each joint

(hip and knee), we plan to exclude studies with a high overall risk of bias in the sensitivity analyses.

Interpreting results and reaching conclusions

We will follow the guidelines in Chapter 15 of the *Cochrane Handbook for Systematic Reviews of Interventions* to interpret the synthesis results and communicate the conclusions of the review effectively (Schünemann 2021a). We will be aware of distinguishing a lack of evidence of effect from a lack of effect. We will base our conclusions on findings from the quantitative or narrative synthesis of included studies for this review. We will consider any statistical heterogeneity when interpreting results, particularly when there is variation in the direction of effect. Moreover, we will avoid making recommendations about healthcare decisions, and our implications for research will suggest priorities for future research and outline the remaining uncertainties in the area.

Summary of findings and assessment of the certainty of the evidence

We will create summary of findings tables using the following outcomes: pain intensity, function, health-related quality of life, gait performance, patient-reported satisfaction or global perceived effect, and withdrawals due to adverse events related to rehabilitation exercises at postintervention.

We will use the following comparisons in the summary of findings tables:

- land-based exercises provided by telerehabilitation versus conventional face-to-face for hip replacement;
- land-based exercises provided by telerehabilitation versus conventional face-to-face for knee replacement;
- land-based exercises provided by telerehabilitation versus self-directed exercises for hip replacement;
- land-based exercises provided by telerehabilitation versus self-directed exercises for knee replacement.

We will create summary of findings tables as outlined in Chapter 14 of the *Cochrane Handbook for Systematic Reviews of Interventions*

(Schünemann 2021b) using GRADEpro GDT (GRADEpro GDT). We will use version 3 of the GRADEpro view to display our summary of findings tables. We will justify all decisions to downgrade the certainty of the evidence for each outcome using footnotes and make comments to aid the reader's understanding of the review, if necessary.

Two review authors (DJAA and VTC) will independently assess the certainty of the evidence. We will use the five GRADE considerations (study limitations, consistency of effect, imprecision, indirectness, and publication bias) to assess the certainty of a body of evidence as it relates to the studies which contribute data to the meta-analyses for the prespecified outcomes and to report the certainty of evidence as high, moderate, low, or very low. The overall risk of bias judgement will be used in the GRADE assessments. We will use methods and recommendations described in Chapter 14 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Schünemann 2021b).

We will provide the NNTB or NNTH (only for outcomes that show a significant difference between treatment groups) and absolute percent change for dichotomous outcomes in the 'What happens' column of the summary of findings tables as described in the [Measures of treatment effect](#) section.

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APPENDICES

Appendix 1. MEDLINE via Ovid search strategy

1. Arthroplasty/
2. Joint Prosthesis/
3. "Prostheses and Implants"/
4. Knee/
5. exp Knee Joint/
6. Hip/
7. exp hip joint/
8. or/1-3
9. or/4-7
10. 8 and 9
11. Arthroplasty, Replacement, Knee/
12. Knee Prosthesis/
13. (tka or tkr).tw.
14. (knee\$ adj (replace\$ or arthroplast\$ or prosthe\$ or endoprosthe\$ or implant\$)).tw.
15. Arthroplasty, Replacement, Hip/
16. Hip Prosthesis/
17. (tha or thr).tw.
18. (hip\$ adj (replace\$ or arthroplast\$ or prosthe\$ or endoprosthe\$ or implant\$)).tw.
19. or/10-18
20. telemedicine.mp. or exp Telemedicine/
21. telehealth.mp.
22. telerehabilitation.mp.
23. telecare.mp.
24. distance rehabilitation.mp.
25. electronic health.mp.
26. ehealth.mp.
27. home-based.mp.
28. self-management.mp.

29. mobile health.mp.
30. mhealth.mp.
31. mobile medicine.mp.
32. phone.mp.
33. telephone.mp. or exp Telephone/
34. smartphone.mp.
35. remotely delivered.mp.
36. mobile.mp.
37. apps.mp.
38. application program*.mp.
39. application software*.mp.
40. mobile application*.mp.
41. SMS.mp.
42. text messaging.mp. or exp Text Messaging/
43. texting.mp.
44. exp Internet/ or internet.mp.
45. online.mp.
46. internet-based.mp.
47. web-based.mp.
48. video conferenc*.mp.
49. tablet device.mp.
50. iPad.mp.
51. iPhone.mp.
52. distance.mp.
53. whatsapp\$.tw. OR wechat\$.tw. OR tweet\$.tw. OR Gchat.tw. OR facetime\$.tw. OR (linkedin or linked in).tw. OR vimeo.tw. OR YouTube.tw. OR wiki\$.tw. OR pinterest.tw. OR instagram.tw. OR Twitter.tw. OR Facebook.tw.
54. or/20-53
55. randomized controlled trial.pt.
56. controlled clinical trial.pt.
57. randomized.ab.
58. placebo.ab.
59. drug therapy.fs.
60. randomly.ab.
61. trial.ab.
62. groups.ab.

63. or/55-62

64. (animals not (humans and animals)).sh.

65. 63 not 64

66. 54 and 65 and 19

CONTRIBUTIONS OF AUTHORS

Conception and design: DJAA, DGC, and VFMT.

Drafting of the protocol: DJAA, VTC, NCJ, and DGC.

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Final approval of the protocol: DJAA, GSR, VTC, NCJ, MBCF, DGC, and VFMT.

DECLARATIONS OF INTEREST

DJAA: none.

GSR: none.

VTC: none.

NCJ: none.

MBCF: none.

DGC: none.

VFMT: none.

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Internal sources

- None, Other

None

External sources

- None, Other

None