

# The effect of preoperative interventions on postoperative outcomes following elective hand surgery: A systematic review

Hand Therapy  
2025, Vol. 30(1) 19–33  
© The Author(s) 2024



Article reuse guidelines:  
[sagepub.com/journals-permissions](https://sagepub.com/journals-permissions)  
DOI: 10.1177/17589983241301449  
[journals.sagepub.com/home/ht](https://journals.sagepub.com/home/ht)



Tahra Babiker-Moore<sup>1,2</sup> , Carol J Clark<sup>1</sup> , Emma Kavanagh<sup>1</sup> and Timothy B Crook<sup>2</sup>

## Abstract

**Background:** Hand surgery is commonly required for conditions like Dupuytren's disease, carpal tunnel syndrome, and carpometacarpal osteoarthritis. Hand experts agree that patient education and managing expectations can optimise surgical outcomes. With an aging population, and rising rates of diabetes and obesity, a significant increase in elective hand surgeries is anticipated over the next decade.

**Objective:** To assess the effectiveness of preoperative therapy interventions on improving postoperative outcomes following elective hand surgery.

**Method:** A systematic search of six databases accessed journals from January 2011 to April 2024. Included studies assessed postoperative outcomes following preoperative therapy intervention.

**Results:** Seven articles met the inclusion criteria: six randomised controlled trials (RCTs) and one retrospective cohort study. Five RCTs explored effects of opioid education on postoperative consumption, all reporting statistically significant differences in favour of the intervention, with one showing a decrease of 49.7 morphine equivalent units (95% CI: 11.9 to 87.5), representing a 34.7% decrease between intervention and control. All RCTs scored poorly for risk of bias with the exception of one which assessed the effects of a neuroscience pain education on postoperative pain, reporting a non-significant decrease of 7.7% favouring the control.

**Conclusion:** There is a lack of quality research assessing preoperative interventions and outcomes for hand surgery. There was some indication of favourable outcomes following preoperative opioid education; however, number of studies were small, the evidence quality was poor, and data were limited. Further research is required to address these gaps and identify the most effective preoperative interventions.

## Keywords

Hand surgery, preoperative therapy interventions, education

Date received: 18 May 2024; accepted: 4 October 2024

## Introduction

Hand conditions such as Dupuytren's disease, carpal tunnel syndrome, and carpometacarpal osteoarthritis are becoming increasingly prevalent, partly due to an aging population and rising chronic health disorders.<sup>1</sup> The socioeconomic impact of impaired hand function and pain due to these conditions is substantial, affecting individuals' social, personal, and mental capacities.<sup>2,3</sup>

Specialist hand surgery is often required to restore function, and the number of such surgeries is expected to rise significantly in the UK over the next decade.<sup>1</sup> Various postoperative rehabilitation interventions are available following hand surgery, including exercise, education,

massage, splinting, ultrasound, and corticosteroid injections. However, preoperative interventions are not routinely implemented, despite consensus among hand experts that

<sup>1</sup>Faculty of Health and Social Sciences, Bournemouth University, Bournemouth, UK

<sup>2</sup>South Wing E1 Orthopaedic Outpatients, Dorset County Hospital, Dorchester, UK

### Corresponding author:

Tahra Babiker-Moore, Faculty of Health and Social Sciences, Bournemouth University, Bournemouth Gateway Building, Bournemouth BH8 8GP, UK.

Email: [tbabikermoo@bournemouth.ac.uk](mailto:tbabikermoo@bournemouth.ac.uk)

addressing patients' anxiety, pain, and expectations preoperatively is crucial to achieving optimal outcomes.<sup>4,5</sup> It is suggested that this preoperative focus should involve expectation setting, and education around post-operative exercise prescription, oedema reduction, scar management, and advice on activities of daily living prior to surgery.<sup>4</sup>

In contrast, an invasive preoperative surgical intervention has demonstrated improved outcomes for Dupuytren's fasciectomy.<sup>6</sup> This intervention uses an external fixator in a preliminary operative procedure to facilitate progressive soft tissue distraction at the proximal interphalangeal joint, showing promising results in enhancing extension gains post-fasciectomy. Given the success of conservative management for even longstanding flexion contractures, this raises the question of whether similar outcomes could be achieved with non-invasive methods, such as serial casting or a static progressive thermoplastic splint applied preoperatively.<sup>7</sup>

Optimal postoperative outcomes in musculoskeletal surgeries often hinge on the interplay of reduced pain, enhanced knowledge, and increased adherence to postoperative therapy, all of which are intricately connected.<sup>8,9</sup> Education can significantly influence a patient's perception and understanding of pain, thereby shaping both their pain experience and psychological response.<sup>10</sup> Effective pain management not only alleviates discomfort but also promotes better adherence to therapy regimens. Patients experiencing lower pain levels are more likely to engage actively in rehabilitation exercises, leading to improvements in both clinical outcomes and functional recovery.<sup>11</sup> Achieving these outcomes requires implementing behaviour change strategies, facilitated by strong therapeutic alliances between therapists and patients.<sup>12</sup> Engagement in preoperative therapy lays the foundation for cultivating these alliances and prepares patients mentally and physically for postoperative recovery. This systematic review aims to assess existing evidence for the effectiveness of preoperative therapy interventions on improving postoperative outcomes following elective hand surgery.

## Method

### Search strategy

The review protocol was registered with PROSPERO (CRD42023394946) and conducted in accordance with PRISMA guidelines.<sup>13</sup> A systematic search of the literature was conducted in April 2024, identifying articles within six electronic databases: MEDLINE Complete; CINAHL Complete; Academic Search Ultimate; Scopus; Web of Science; and Cochrane Library. The reference list of included studies was also searched, while unpublished studies were not pursued. A Boolean search strategy was used to achieve a comprehensive identification of the available

literature, using terms key to the research question, along with their synonyms (supplemental table 1). Both free-text terms and subject headings were used where possible. The complete search strategy was first created in a separate log document and then copied and pasted into the relevant databases. Following removal of duplicates, articles identified through initial searches were assessed and screened for eligibility by title and abstract by the first author (TBM). The first and second authors (TBM and CC) then independently undertook a full text evaluation of remaining articles. Reasons for omission of excluded articles were recorded (Figure 1).

### Selection criteria (inclusion and exclusion)

Eligibility for study inclusion comprised of English language, peer-reviewed academic journal articles published between January 2011 and April 2024. Only articles with an adult and non-cancer patient study population were included due to the possible effects of common cancer treatments on the musculoskeletal system and the degree and rate of surgical recovery. All pilot and feasibility studies, as well as studies only reporting protocol and conference proceedings, were excluded from this review, while studies of all other designs were considered for inclusion. Full inclusion and exclusion criteria are illustrated in Table 1.

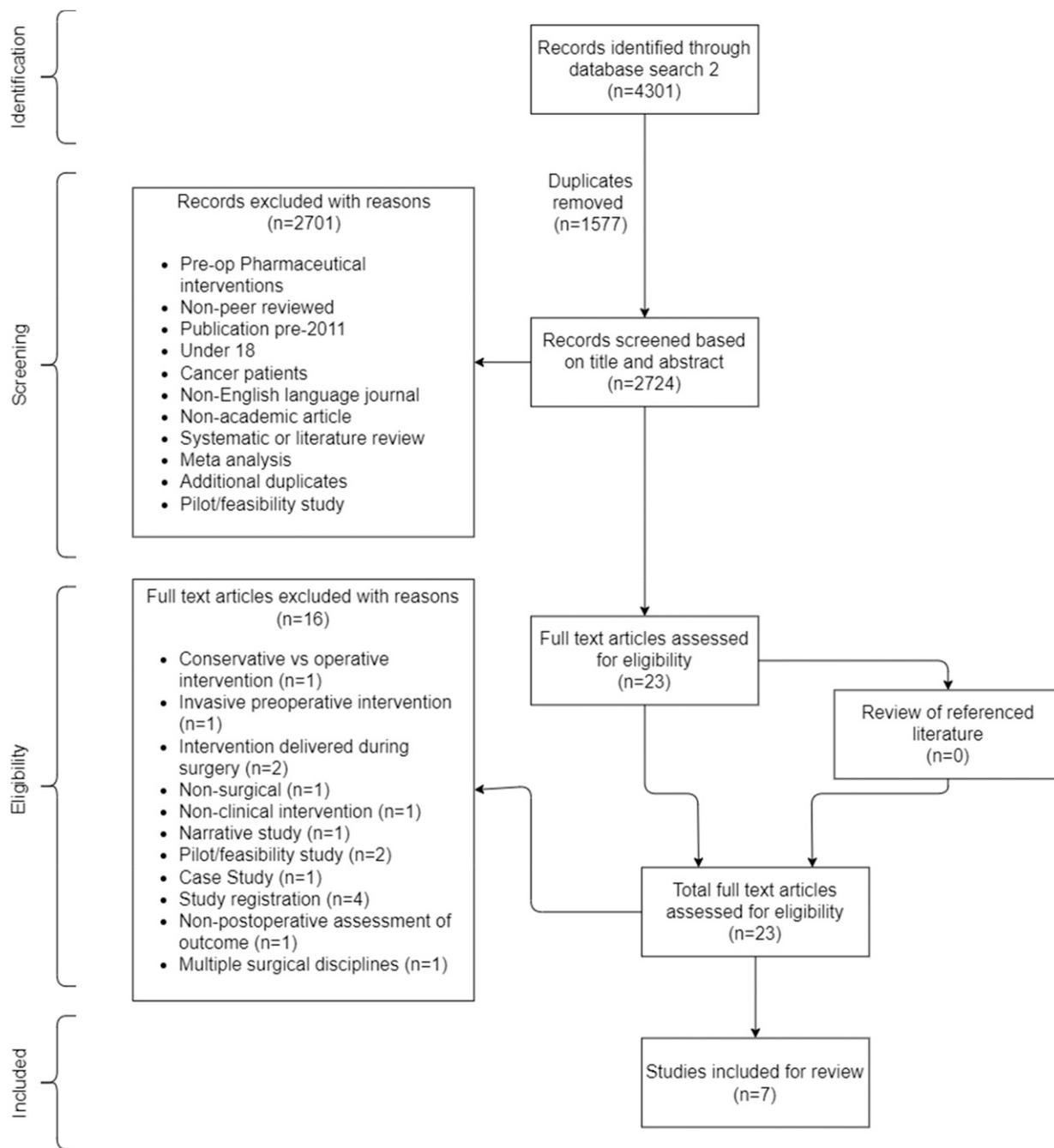
### Data collection

Study details for final included articles were recorded using the same standardised data extraction template (Table 2). Table 3 documents the study outcomes, focusing specifically on primary outcome measures that are consistently reported, or most closely related, across the included studies.

Despite five of the included studies evaluating a similar intervention, a meta-analysis was not feasible due to differences in the timing, nature, and duration of outcome assessments. The studies lacked standardisation in outcome measures, with none reporting effect sizes, and some providing insufficient data for calculating effect sizes for primary outcomes. Consequently, a narrative synthesis of findings was adopted.

### Quality appraisal

Assessment of study quality using a modified version of the Downs and Black Checklist<sup>14</sup> facilitated comparison across the two study designs included in this review. However, to mitigate against misleading review findings, and to ensure accurate inferences regarding risk of bias assessments for the included RCTs, the Cochrane Risk of Bias tool for randomised trials (RoB2) was employed.<sup>15</sup> This dual



**Figure 1.** Prisma diagram showing paper selection method.

approach ensures a comprehensive and rigorous evaluation of study quality across all included studies.

The Downs and Black checklist provides a general global score for study quality alongside individual scores for quality of reporting, internal and external validity, and power to detect effect. With a maximum score of 28, each paper was assigned the corresponding grade: excellent (24–28 points); good (19–23 points); fair (14–18 points); or poor (<14 points).<sup>16</sup> Only studies grading fair, good or excellent

were deemed robust and reliable enough to be included for review (supplemental table 2).

The RoB2 tool provides domain level and overall study judgement about risk of bias in the findings of a single outcome for RCTs (supplemental tables 3 and 4). The tool is structured into five domains through which bias might be introduced, including bias arising from the randomisation process; bias due to deviations from intended interventions; bias due to missing outcome data; bias in measurement of

**Table 1.** Inclusion and exclusion criteria.

	Inclusion criteria	Exclusion criteria
Population	<ul style="list-style-type: none"> <li>• Studies with participants over the age of 18 years.</li> <li>• Studies observing preoperative intervention prior to elective hand surgery.</li> </ul>	<ul style="list-style-type: none"> <li>• Studies observing hand trauma surgery patients.</li> <li>• Cancer patients.</li> </ul>
Intervention	<ul style="list-style-type: none"> <li>• Studies observing the preoperative effect of a therapy intervention including exercise, education, massage, splinting or psychological intervention.</li> </ul>	<ul style="list-style-type: none"> <li>• No clear methods of type of preoperative intervention.</li> <li>• Invasive preoperative interventions</li> </ul>
Outcome	<ul style="list-style-type: none"> <li>• Studies assessing physical, functional and psychological outcome measures.</li> <li>• Studies assessing patient beliefs regarding pain, pain control and satisfaction levels.</li> </ul>	<ul style="list-style-type: none"> <li>• Studies with no individualised outcome measures.</li> <li>• Studies with no postoperative evaluation of outcome.</li> </ul>
Other	<ul style="list-style-type: none"> <li>• Peer reviewed articles.</li> </ul>	<ul style="list-style-type: none"> <li>• Pilot or protocol studies.</li> <li>• Studies published in periodicals or conferences.</li> </ul>

the outcome; bias in selection of the reported result. Judgements using the tool assessed the primary study outcome under review, categorising biases as either ‘Low’ risk, ‘High’ risk, or indicating ‘Some concerns’ within each of the specified domains<sup>17</sup> (supplemental Tables 3 and 4). Assessment of quality was reviewed by the first and second authors.

## Results

### Included studies

The search generated a total of 2724 articles of which only seven were considered appropriate for review (Figure 1). Six studies were randomised control trials (RCTs) of which five were conducted in the USA,<sup>18–22</sup> and one in Chile.<sup>24</sup> The seventh study was a retrospective observational cohort study conducted in Finland.<sup>23</sup> The six RCTs were all educational interventions, five exploring the impact of a standardised preoperative patient education programme designed to reduce opioid use after surgery,<sup>18–22</sup> and one assessing the efficacy of a single preoperative pain neuroscience education session on reducing postoperative pain, kinesiophobia and upper limb function.<sup>24</sup> The one retrospective cohort study explored the effect of conservative therapies before and after surgery on improving symptoms and patient satisfaction postoperatively.<sup>23</sup>

### Quality assessment

Quality assessment scores ranged from 14 to 25 points on the Downs and Black modified checklist (supplemental table 2). One RCT rated ‘excellent,’ scoring 25 points,<sup>24</sup> five RCTs fell within the ‘good’ classification with four scoring 19 points<sup>18,19,21,22</sup> and one scoring 22.<sup>20</sup> The final study was the only retrospective observational cohort study and classified as ‘fair’ scoring 14 points.<sup>23</sup> Despite overall quality scores of fair or above for all included studies, none

of the RCTs reported an effect size with confidence interval for primary outcomes. Three studies provided sufficient data for calculating an effect size on their primary outcome,<sup>19,21,24</sup> while the remaining three studies lacked adequate information for at least one primary outcome measure.<sup>18,20,22</sup>

The risk of bias assessment using the RoB2 tool revealed a high overall risk of bias for five of the included RCTs<sup>18–22</sup> and a low risk of bias for only one<sup>24</sup> (supplemental tables 3 and 4). The most significant contributors to the high risk of bias were found in two key areas: bias due to missing outcome data, which was evident in four of the six RCTs,<sup>19–22</sup> and bias in the measurement of outcomes, observed in three of the six studies.<sup>18,19,21</sup> The latter was primarily due to the potential influence of knowledge of the intervention received on the assessment of outcomes. Additionally, concerns regarding bias in the selection of reported results were raised in three studies, primarily due to insufficient information on a pre-specified data analysis plan.<sup>18,20,21</sup>

### Study characteristics

Table 2 lists study details for all included studies.

### Sample population

The seven studies included a total of 875 patients. However, in the study by Multanen et al.,<sup>23</sup> multiple intervention groups were assessed. When comparing only patients who received a preoperative intervention to those with no intervention, the total number of participants across all studies was 700. Five of the six RCTs evaluated the impact of a preoperative patient education programme aimed at reducing opioid use after surgery,<sup>18–22</sup> with sample sizes ranging from 40<sup>18</sup> to 191<sup>20</sup> patients. The gender distribution was relatively balanced, with females comprising 50.3% on average, and the mean age was 58.3 years. The sixth RCT

**Table 2.** Study details for the 7 included articles.

Author/ year/ location	Aims	Design	Population	Intervention	Quality assessment		
					Downs and black Score	RoB2 overall Category	RoB2 overall risk of bias
Alter and Ilyas (2017) USA	To evaluate the effect of preoperative opioid counselling on patients' pain experience and opioid consumption after carpal tunnel release (CTR).	Randomised controlled trial.	Surgery: Mini-open carpal tunnel release. Sample size: Intervention group <i>n</i> = <b>20</b> Control group <i>n</i> = <b>20</b> Mean age (years): Total study sample <i>n</i> = <b>61.5</b> Intervention group <i>n</i> = <b>61</b> Control group <i>n</i> = <b>62</b> Sex (female number (%)): Total study sample <i>n</i> = <b>25 (62.5%)</b> Intervention group <i>n</i> = <b>11 (55%)</b> Control group <i>n</i> = <b>14 (70%)</b>	Intervention group: Preoperative opioid counselling: 1-page informational form reviewed with the surgeon immediately prior to the procedure on the day of surgery. Control Group: No preoperative education intervention reported.	19	<b>Good</b>	High risk
Gilmer et al. (2024) USA	To evaluate the efficacy of a web tutorial on perceptions of pain management and usage of opioids after carpal tunnel release (CTR).	Randomised controlled trial.	Surgery: Endoscopic carpal tunnel release Sample size: Intervention group <i>n</i> = <b>25</b> Control group <i>n</i> = <b>25</b> Mean age and standard deviation (years): Intervention group <i>n</i> = <b>53.8 ± 14.1</b> Control group <i>n</i> = <b>58.2 ± 14.7</b> Sex (female number (%)): Intervention group <i>n</i> = <b>18 (72%)</b> Control group <i>n</i> = <b>15 (60%)</b> Sex (prefer not to say number (%)): Intervention group <i>n</i> = <b>1 (4%)</b> Control group <i>n</i> = <b>2 (8%)</b> Sex (nonbinary (%)): Intervention group <i>n</i> = <b>0 (0%)</b> Control group <i>n</i> = <b>1 (4%)</b>	Intervention group: Preoperative opioid education: 8-min web tutorial reviewed via an iPad in the waiting area before surgery. Control Group: No preoperative education intervention reported.	19	Good	High risk

(continued)

Table 2. (continued)

Author/ year/ location	Aims	Design	Population	Intervention	Quality assessment		
					Downs and black Score	Category	RoB2 overall risk of bias
Multanen et al. (2021) Finland	To examine the use of conservative therapies before and after carpal tunnel release (CTR).	Retrospective cohort study.	Surgery: Carpal tunnel release. Sample size: Preoperative intervention group $n = 41$ Postoperative intervention group $n = 18$ Preoperative and postoperative intervention group $n = 157$ No preoperative or postoperative intervention group $n = 43$ Mean age and standard deviation (years): Study sample $n = 62 \pm 15$ Sex (female number (%)): Study sample: $n = 172$ (66%)	Preoperative and postoperative therapy treatments included: - Splinting - Stretching - Pain medication - Massage - Acupuncture - Corticosteroid injection	14	<b>Fair</b>	N/A
Núñez-Cortés et al. (2019) Chile	To evaluate the efficacy of a multimodal intervention combining preoperative pain neuroscience education (PNE) with postoperative therapeutic exercise in surgical CTS patients	A double-blind, randomized, controlled trial.	Surgery: Carpal tunnel release. Sample size: Intervention group $n = 15$ Control group $n = 15$ Mean age and standard deviation (years): Total study sample $n = 54.2 \pm 9.2$ Intervention group $n = 58.4 \pm 8.4$ Control group $n = 53.8 \pm 8.7$ Sex (female number (%)): Total study sample $n = 20$ (66.7%) Intervention group $n = 11$ (73.3%) Control group $n = 9$ (60.0%)	Intervention group: Preoperative pain neuroscience education: a single one-on-one session one week prior to surgery. Control group: Preoperative anatomical, and pathological aspects CTS education: a single session 1 week prior to surgery.	25	<b>Excellent</b>	Low risk

(continued)

**Table 2.** (continued)

Author/ year/ location	Aims	Design	Population	Intervention	Quality assessment		
					Downs and black Score	Category	RoB2 overall risk of bias
Stepan et al. (2021) USA	To create a standardized patient education program regarding postoperative pain management after hand surgery and to determine whether that education program would decrease postoperative opioid use.	Randomised controlled trial	Surgery: Cubital tunnel release, basal joint arthroplasty, palmar fasciectomy, tendon repair or transfers, nerve repair or transfer, ligament repair or reconstruction, radius and ulnar osteotomies, and arthrodesis or arthroplasty of the wrist/hand/finger  Sample size: Intervention group <i>n</i> = <b>93</b> Control group <i>n</i> = <b>98</b> Mean age in years (range): Total study sample <i>n</i> = <b>60 (19–84)</b> Intervention group <i>n</i> = <b>58 (19–82)</b> Control group <i>n</i> = <b>62.5 (22–84)</b> Sex (female number (%)): Total study sample <i>n</i> = <b>87 (45.5%)</b> Intervention group <i>n</i> = <b>41 (44.1%)</b> Control group <i>n</i> = <b>46 (46.9%)</b>	Intervention group: Preoperative opioid education: 7-min webinar and a laminated card with summary alongside routine preoperative surgical information.  Control group: Routine preoperative surgical education.	22	Good	High risk

(continued)

Table 2. (continued)

Author/ year/ location	Aims	Design	Population	Intervention	Quality assessment		
					Downs and black Score	Category	RoB2 overall risk of bias
Vincent et al. (2022) USA	To investigate the effects of preoperative opioid counselling on postoperative opioid consumption after outpatient upper extremity surgery.	Randomised controlled trial.	Surgery: Upper extremity fracture, arthroscopy, arthroplasty, arthrodesis, nerve and tendon procedures. Sample size: Intervention group $n = 62$ Control group $n = 69$ Average age (years): Total study sample $n = 50.3$ Intervention group $n = 50.9$ Control group $n = 49.8$ Sex (female number (%)): Total study sample $n = 53 (40.5\%)$ Intervention group $n = 26 (42\%)$ Control group $n = 27 (39\%)$	Intervention group: Preoperative opioid education: 5-min pre-taped video played in the preoperative area on day of surgery. Control group: No preoperative education intervention reported.	19	<b>Good</b>	High risk
Zohar-Bondar et al. (2022) USA	To create a standardized perioperative patient education program regarding postoperative pain management after hand surgery and to determine if it could reduce opioid use after hand surgery.	Randomised controlled trial	Surgery: Carpal tunnel release, de Quervain release, trigger finger release, dorsal ganglion cyst excision, wrist arthroscopy, and Dupuytren's fasciectomy. Sample size: Intervention group $n = 90$ Control group $n = 84$ Average age in years (range): Total study sample = $63 (19-90)$ Intervention group = $62.5 (19-90)$ Control group = $63.5 (29-87)$ Sex (female number (%)): Total study sample = $97 (55.7\%)$ Intervention group $n = 46 (51.1\%)$ Control group $n = 51 (60.7\%)$	Intervention group: Preoperative opioid education: 10-min opioid webinar alongside study related webinar sent to patients' prior surgery. Control group: Study related webinar sent to patients' prior surgery.	19	<b>Good</b>	High risk

examined the postoperative effects of a single preoperative pain neuroscience education on 30 patients, with a female majority of 66.7% and a mean age of 54.2 years.<sup>24</sup> The one retrospective cohort study explored conservative therapy usage before and after carpal tunnel release in 259 patients, with a female majority of 66% and a mean age of 62 years.<sup>23</sup>

Although all five RCTs evaluating a preoperative opioid education intervention assessed outcomes following a form of upper extremity surgery, inclusion criteria regarding type and location of surgery differed. One examined outcome following all elective outpatient upper extremity surgeries, including bone and soft tissue procedures of the wrist, elbow, and shoulder.<sup>21</sup> This was the only study to include shoulder surgery. Stepan et al.<sup>20</sup> assessed non-minor elective hand surgery and included procedures of the hand, wrist, and elbow, whereas Zohar-Bondar et al.<sup>22</sup> looked only at minor soft tissue procedures of the wrist and hand. The remaining two opioid studies examined outcome following carpal tunnel release only, one, a mini-open procedure<sup>18</sup> and the other, an endoscopic procedure.<sup>19</sup> The RCT examining the efficacy of a preoperative pain neuroscience education reported outcomes following carpal tunnel surgery only,<sup>24</sup> as did the 2021 retrospective cohort study examining the use of conservative therapies before and after surgery.<sup>23</sup>

### Intervention and control

The opioid educational interventions in all five opioid studies presented similar content. This encompassed education on the opioid epidemic and its ramifications, detailed information on the properties of opioids, guidelines for safe consumption practices, an overview of expected post-surgical pain levels, and a structured approach to pain management. This approach emphasised the use of non-opioid pain management strategies as the primary line of defence, underscoring the significance of mitigating opioid dependence. Four studies delivered the intervention electronically via webinar or pre-taped video,<sup>19–22</sup> with two also providing a written summary post-surgery<sup>20,22</sup> and one granting access to the webinar link for the duration of postoperative recovery<sup>19</sup> (Table 2). The fifth opioid study involved a brief review with the surgeon immediately prior to surgery.<sup>18</sup> The RCT assessing efficacy of preoperative pain neuroscience education consisted of a single 30-min, one-on-one pain neuroscience teaching session, using examples and metaphors to describe neurophysiological and biopsychosocial aspects of pain via audio and visual instruction.<sup>24</sup> Three RCTs described routine preoperative education for both intervention and control groups,<sup>20,22,24</sup> while three did not specify any preoperative intervention for controls.<sup>18,19,21</sup> The retrospective study discussed various preoperative conservative therapies like splinting and massage but lacked details on frequency or duration. The

study compared outcomes among patients receiving different therapy interventions and those receiving none.<sup>23</sup>

### Study outcomes

Outcomes were assessed with a focus on the primary measures consistently reported across the included studies, or those most closely related, to ensure comparability and relevance to this review's objectives. However, due to the varied interventions and the lack of consensus on the most appropriate outcome measures, the results were not always comparable across studies. This necessitated the segmentation of review findings, with discussion grouping outcomes as follows: postoperative opioid consumption and (where possible) postoperative pain and the proportion of patients who took opioids postoperatively for the opioid studies; postoperative pain for the pain neuroscience education study; patient satisfaction with treatment for the conservative therapy cohort study.

Four studies assessing postoperative opioid consumption reported the number of pills consumed,<sup>18,20–22</sup> with three additionally providing either morphine equivalent units (MEU),<sup>21</sup> oral morphine equivalents (OME),<sup>20</sup> or morphine milligram equivalents (MME).<sup>22</sup> Of these four studies, two also reported on the proportion of patients using opioid postoperatively,<sup>18,20</sup> while the one remaining opioid study focused solely on this proportion without providing details on the number of pills consumed.<sup>19</sup> Outcome assessments varied between the first and fifteenth postoperative day. All five opioid studies indicated a statistically significant reduction in postoperative opioid consumption favouring the intervention. Two studies found no significant difference in pain levels between groups over days 0–3 or 0–5, as measured by a visual analogue scale (VAS).<sup>18,21</sup> Another study reported no significant pain difference between groups but used different time points for measurement.<sup>20</sup> Two studies didn't report postoperative pain scores,<sup>19,22</sup> but one of these constructed a linear regression model showing that pain at week one predicted greater opioid use.<sup>22</sup> None of the five opioid studies provided the point estimate of intervention effect. However one did report the number of pills consumed postoperatively as mean and standard deviation,<sup>21</sup> while another three reported on the proportion consuming pills postoperatively but over different time periods.<sup>18–20</sup> This allowed the effect size to be calculated on study specific outcomes. However an overall effect size for these studies could not be calculated due to differences in outcome measure or a difference in the time at which the outcome was assessed (Table 4). The one study that allowed the point estimate of intervention effect for the number of pills consumed at 2 weeks postoperatively (the difference in outcome between intervention and control groups) was calculated to be  $-49.7$  (95% CI:  $-87.5$  to  $-11.9$ ) morphine equivalent units (MEU), amounting to a 34.7% decrease in opioid consumption relative to the control group (Table 4).

**Table 3.** Study outcomes for the 7 included articles.

Author (year)	Number of opioids consumed		Number of patient consuming opioids	Pain level ( <sup>a</sup> VAS)	Patient satisfaction <sup>b</sup>
	Pills	MEU/OME/MME			
Alter and Ilyas (2017)	<b>Day 0–3:</b> Intervention group $n = 1.40$ vs control group $n = 4.20$ ( $p < .05$ ). (Variable = mean)	Not reported	<b>Day 0-3:</b> Intervention group $n = 8$ (40) vs control group $n = 16$ (80). (Variable = number (%))	<b>Day 3:</b> Intervention group = $1.90$ vs control group = $1.80$ ( $p \leq .82$ ). (Variable = mean)	Not reported
Gilmer et al. (2024)	Not reported	Not reported	<b>Day 1:</b> Intervention group $n = 3$ (12) vs control group $n = 7$ (28) ( $p = .182$ ). <b>Day 2–6:</b> Intervention group $n = 2$ (8) vs control group $n = 7$ (28) ( $p = .043$ ). <b>Day 7–14:</b> Intervention group $n = 0$ (0) vs control group $n = 5$ (20) ( $p = .018$ ). (Variable = number (%))	Not reported	Not reported
Multanen et al. (2021).	Not reported	Not reported	Not reported	Not reported	Preoperative intervention only group = 3 vs postoperative intervention only group = 2.3 vs preoperative and postoperative intervention group = 2 vs no preoperative or postoperative intervention group = 2 ( $p = .03$ ). (Variable = mean)
Núñez-Cortés et al. (2019).	Not reported	Not reported	Not reported	<b>Week 4:</b> Intervention group = $42.8 \pm 21.1$ (31.7–53.8) vs control group = $39.5 \pm 21.2$ (27.9–50.9) ( $p = .553$ ). (Variable = mean $\pm$ standard deviation)	Not reported
Stepan et al. (2021).	<b>Day 0–15:</b> Intervention group = 2 (0–56) vs control group = 5 (0–49) ( $p < .001$ ). (Variable = median (range))	<b>Day 0-14:</b> <b>(OME)</b> Intervention group = 7.8 (0–405) vs control group = 40.0 (0–323) ( $p < .001$ ). (Variable = median (range))	<b>Day 0-14:</b> Intervention group $n =$ $54$ (58.1) vs control group $n = 74$ (75.5) ( $p = .01$ ). (Variable = number (%))	<b>Week 1:</b> Intervention group = $3.3$ (0–9.3) vs control group = $3.6$ (0–9.1) ( $p < .27$ ). (Variable = median (range))	Not reported

(continued)

**Table 3.** (continued)

Author (year)	Number of opioids consumed		Number of patient consuming opioids	Pain level ( <sup>a</sup> VAS)	Patient satisfaction <sup>b</sup>
	Pills	MEU/OME/MME			
Vincent et al. (2022).	<b>Day 0–14:</b> Intervention group = 11.77 ± 10.2 vs control group = 17.35 ± 12.6 ( <i>p</i> < .007). (Variable = mean ± standard deviation)	<b>Day 0–14:</b> <b>(MEU)</b> Intervention group = 93.5 ± 97.6 vs control group = 143.2 ± 123.0 ( <i>p</i> < .013). (Variable = mean ± standard deviation)	Not reported	<b>Day 3:</b> Intervention group = <b>4.73</b> vs control group = <b>4.80</b> ( <i>p</i> < .881). (Variable = mean)	Not reported
Zohar- Bondar et al. (2022).	<b>Day 0–15:</b> Intervention group <i>n</i> = <b>0</b> (0–13) vs control group <i>n</i> = <b>0.5</b> (0–40) ( <i>p</i> < .05) (Variable = median (range))	<b>Day 0–15:</b> <b>(MME)</b> Intervention group <i>n</i> = <b>0</b> (0–14) vs control group <i>n</i> = <b>0.1</b> (0–60) ( <i>p</i> < .05) (Variable = median (range))	Not reported	<b>Week 1:</b> Regression model summary for pain (with opioids consumed as the dependent variable): beta = <b>0.93</b> , 95% confidence interval = 0.56–1.3, <i>p</i> = < .05.	Not reported

MEU: morphine equivalent units; OME: oral morphine equivalents; MME: morphine milligram equivalents.

<sup>a</sup>Pain intensity was measured on a visual analog scale (VAS; 0–10).

<sup>b</sup>Satisfaction measured with a five-point response scale from “no improvement” to “eliminated symptoms completely” (scores 0–4).

The single study on pain neuroscience education found no significant differences in perceived pain at 4 weeks postoperatively, with between group difference calculated on the VAS as 3.3 (95% CI: −11.8 to 18.4), equating to a non-statistically significant difference of 7.7% in pain score favouring the control. Additionally, there was also no significant difference in pain catastrophising, kinesiophobia, or upper extremity function at weeks four and 12 postoperatively.<sup>24</sup>

Multanen et al.<sup>23</sup> examined the use of conservative therapy before and after surgery on levels of patient satisfaction measured with a five-point response scale to the question “Did the surgery improve the condition of your hand or wrist?”. Results reported higher treatment satisfaction in patients receiving only preoperative conservative therapies compared to other groups.

## Discussion

This systematic review aimed to investigate the efficacy of preoperative therapy interventions on postoperative outcomes following elective hand surgery. While evidence suggests positive outcomes from such interventions in lower limb surgery,<sup>25,26</sup> research in the context of hand surgery is limited. Assuming results of lower limb research can be applied to the upper limb is problematic as the upper and lower limb differ in

function, anatomy and neurology. Despite the anticipated rise in elective hand surgeries, 2025 is only predicted to see numbers reach 145,000 jointly in the UK.<sup>27</sup> This relatively small number of procedures presents challenges for conducting robust research, including high costs associated with multi-centre trials. This review identified only seven studies meeting the inclusion criteria and as a result highlights the need for further research to explore the effects of preoperative interventions on the outcomes of hand surgery specifically.

The Downs and Black Checklist revealed variations among the included studies. Three of the six RCTs lacked blinding of study subjects, increasing risk of ascertainment bias. Notably, none of the included studies adequately reported on sample representativeness, potentially impacting the generalisability of findings.

While randomisation minimises the risk of bias, two of the included studies lacked adequate concealment of randomised group assignment, potentially introducing selection bias.<sup>18,21</sup> Inadequate reporting quality was evident across all studies, with some failing to provide essential data on outcome measures and effect size. Reporting of effect size is important to establish not only the direction of the effect but also its size.<sup>28</sup> Quantifying the difference in outcomes between the intervention and control groups and placing emphasis on the size of that difference also provides

a measurable means of establishing clinical significance. While statistical significance is important for interpreting research, understanding clinical significance is crucial to establishing outcomes that are meaningful to patients.<sup>29</sup> However, this component of the reporting subscale was only marked down on the Downs and Black quality assessment tool on the three studies that failed to provide enough data for effect size to be calculated on all their primary outcomes (Table 4). Furthermore, insufficient study power compromised the ability to draw accurate conclusions from the sample data, as evidenced by relatively small sample sizes and wide confidence intervals. Downs and Black summary score indicated good or excellent study quality for the six RCTs and fair quality for the cohort study. However, these relatively high scores, are misleading. A targeted assessment using the RoB2 tool for the primary outcome judged only one RCT to be at low risk of bias. Relying solely on global assessment of study quality risks masking critical biases across the studies and can lead to inaccurate conclusions about the credibility of study outcomes.<sup>15</sup> These limitations underscore the need for improved methodological rigour in future research on preoperative interventions in hand surgery.

Preoperative opioid education was examined in five of the included studies, all conducted in the United States.<sup>18–22</sup> This focus is unsurprising given the magnitude of the ongoing opioid epidemic in the country, which originated from excessive prescription and misuse of pain medication.<sup>30</sup> Upper extremity surgeons in the United States commonly prescribe opioids for postoperative pain management, potentially in unnecessary quantities.<sup>31</sup> Despite efforts to curb access to prescription drugs following the introduction of the Centres for Disease Control and Prevention's 2016 opioid prescribing guidelines, the issue persists.<sup>32</sup> While the UK is not facing the same opioid crisis, the effectiveness of educational interventions remains pertinent. If preoperative education can effectively reduce opioid consumption, similar intervention principles may be applied to other educational initiatives.

Despite three of the opioid studies evaluating very similar interventions assessed in terms of the same outcome measure, quantitative synthesis was not possible because outcomes were assessed over different time periods following surgery.

The opioid study that provided sufficient data for calculating the effect size for the number of pills consumed postoperatively, demonstrated an intervention effect showing over a 34% reduction in opioid consumption compared to the control group (Table 4). Consensus-based recommendations for initiating opioid tapering has been recommended as five to 10 percent of MEU every one to 4 weeks.<sup>33</sup> This difference exceeds consensus-based recommendations for opioid tapering, indicating clinical significance. However, due to the lack of data from other

studies, the importance and relevance of effect size remain uncertain.

The only RCT in this review that did not report any intervention effect for any outcome, assessed a preoperative pain neuroscience education intervention. A point estimate of intervention efficacy was calculated from the data provided (Table 4). Núñez-Cortés et al.<sup>24</sup> concluded no intervention effect based on finding no statistically significant difference between groups. However, the lack of statistically significant difference could be attributed to insufficient study power, as indicated by the wide confidence intervals of all reported outcome measures. As a result, and despite rating this study highest among all included studies and the only one to receive a low risk of bias assessment, meaningful conclusions cannot be drawn from the results obtained.

Levels of patient satisfaction were the sole outcome reported for participants receiving only a preoperative intervention in the cohort study investigating the impact of conservative therapies on postoperative outcomes.<sup>23</sup> While this study reported higher satisfaction with treatment in the preoperative intervention group, it is crucial to note that this was an observational study. Without randomisation, any observed differences between groups cannot be conclusively attributed as causal effects of the intervention.

The studies in this review were unable to consistently demonstrate robust and statistically significant effects of preoperative interventions on postoperative outcomes across all evaluated measures and populations. Despite significant variations in study design, quality, and risk of bias among the included studies, a consistent finding across this review emerges: insufficient evidence. This gap may explain why the National Institute of Health and Care Excellence (NICE) currently lacks preoperative clinical guideline recommendations for elective hand surgery, unlike its clear recommendations for preoperative rehabilitation in hip and knee arthroplasty.<sup>26,34</sup> Similarly, there is a dearth of guidance for shoulder replacements, with NICE instead calling for further research in this area.<sup>34</sup> This highlights the underexplored nature of preoperative interventions in upper limb surgery. By contributing to the existing body of evidence, this review emphasises the need for rigorous research to assess the effectiveness of preoperative interventions in elective hand surgery.

Considering the clinical implications of this study, several key recommendations emerge. Firstly, the lack of robust evidence emphasises the critical need for clinicians to approach preoperative care in hand surgery cautiously, acknowledging the absence of clear guidelines or strong evidence-based recommendations. Patient-centred care is crucial, with a focus on shared decision-making and comprehensive patient education to optimise postoperative outcomes.

**Table 4.** Outcome data table – effect size.

Study	Intervention group			Control group			Effect size	
	n	Outcome	CI for %	n	Outcome	CI for %	Difference in % consuming pills	CI for %
Alter and Ilyas (2017)	20	<b>No. consuming pills day 0–3 (%)</b> 8 (40.0)	19.1 to 63.9	20	<b>No. consuming pills day 0–3 (%)</b> 16 (80.0)	56.3 to 94.3	–40.0	–12.3 to –67.7
Gilmer et al. (2024)	25	<b>No. consuming pills day 7–14 (%)</b> 0 (0.0)	0.0 to 13.7	25	<b>No. consuming pills day 7–14 (%)</b> 5 (20.0)	6.8 to 40.7	–20.0	–4.3 to –35.7
Stepan et al. (2021)	93	<b>No. consuming pills day 0–14 (%)</b> 54 (58.1)	47.3 to 68.2	98	<b>No. consuming pills day 0–14 (%)</b> 24 (75.5)	65.7 to 83.6	–17.4	–4.3 to –30.6

Study	Intervention group				Control group				Effect size	
	n	Outcome	SD	CI	n	Outcome	SD	CI	Difference in mean # pills consumed	CI
Vincent et al. (2022)	62	<b>Mean no. pills consumed day 0–14</b>	10.2	9.3 to 14.3	69	<b>Mean no. pills consumed day 0–14</b>	12.6	14.4 to 20.4	–5.6	–9.5 to –1.7
		<b>Mean MEU consumed day 0–14</b>				<b>Mean MEU consumed day 0–14</b>				
		11.8				17.4				
		93.5	97.6	69.2 to 117.8		143.2	123.0	114.2 to 172.2	–49.7	–87.5 to –11.9
Núñez-Cortés et al. (2019)	15	<b>Mean VAS at 4 weeks post-op</b>	21.1	31.7 to 53.8	15	<b>Mean VAS 4 weeks post-op</b>	21.2	27.9 to 50.9	3.3	–11.8 to 18.4

MEU: morphine equivalent units; VAS: visual analog scale (0–100 mm); CI: 95% confidence interval; SD: standard deviation; SE: standard error.

Moreover, this review underscores the necessity for increased emphasis on conducting high-quality research in this domain. Clinicians should prioritise participation in well-designed studies that rigorously evaluate the efficacy and safety of various preoperative strategies. By supporting such research endeavours, clinicians can contribute to bridging existing knowledge gaps and advancing towards evidence-based practices.

**Limitations**

This study is not without limitations. The exclusion of non-English journal articles presents the possibility of selection bias. Although all included studies were classified as either fair, good or excellent on assessment of study quality, all bar

one study was judged to be at least at some risks to bias. Reasons for bias include failure to blind participants, absence of intervention assignment concealment and lack of provision of a sample size calculation. The absence of a reported effect size estimate, along with a precision of that estimate, and the resultant reliance on the *p* value for statistical analysis means the effect of the intervention and its relevance cannot be judged. The potential for publication bias should also be considered, as studies with negative or inconclusive results may be underreported. Finally, the lack of standardised protocols and outcome measures across studies makes it challenging to establish consistent and reliable conclusions. Varying levels of detail in reporting further complicate the assessment of the interventions’ true impact.

These limitations collectively reduce the quality of the assessed evidence, affecting the ability to draw reliable conclusions. Therefore, study findings should be interpreted with caution, and there is a clear need for more rigorous, well-designed studies in this area to provide stronger evidence and more definitive recommendations.

## Conclusion

The aim of this systematic review was to explore the effect of preoperative interventions on postoperative recovery following elective hand surgery. Using a GRADE approach to evidence rating<sup>35</sup> the results compiled for this review indicate very low-quality evidence, suggesting very little confidence in any effect size demonstrated. The small number of available studies highlights the need for further research.

Future researchers are advised to carefully consider the clinical significance of effect sizes prior to data collection and analysis, facilitating clearer interpretation of results. Additionally, establishing consensus on appropriate outcome measures, follow-up intervals, and protocols before evaluating combinations of preoperative interventions following specified surgical procedures is crucial. This presents a significant opportunity for future research to generate high-quality evidence. While this study consolidates existing evidence and highlights gaps in knowledge, clinicians should interpret these findings as a call to action to drive research forward and refine clinical practices in preoperative interventions for elective hand surgery.

## Declaration of conflicting interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

## Funding

The author(s) received no financial support for the research, authorship, and/or publication of this article.

## ORCID iDs

Tahra Babiker-Moore  <https://orcid.org/0009-0009-6195-7918>

Carol J Clark  <https://orcid.org/0000-0002-9296-7141>

## Supplemental Material

Supplemental material for this article is available online.

## References

1. Bebbington E and Furniss D. Linear regression analysis of Hospital Episode Statistics predicts a large increase in demand for elective hand surgery in England. *J Plast Reconstr Aesthet Surg* 2015; 68: 243–251. DOI: [10.1016/j.bjps.2014.10.011](https://doi.org/10.1016/j.bjps.2014.10.011).
2. Dritsaki M, Rivero-Arias O, Gray A, et al. What do we know about managing Dupuytren's disease cost-effectively? *BMC Musculoskelet Disord* 2018; 19(1): 34.
3. Auroux M, Merle B, Fontanges E, et al. The disability associated with hand osteoarthritis is substantial in a cohort of post-menopausal women: the QUALYOR study. *Osteoarthritis Cartilage* 2022; 30: 1526–1535. DOI: [10.1016/j.joca.2022.07.010](https://doi.org/10.1016/j.joca.2022.07.010).
4. Huisstede BM, Fridén J, Coert JH, et al. Carpal tunnel syndrome: hand surgeons, hand therapists, and physical medicine and rehabilitation physicians agree on a multidisciplinary treatment guideline: results from the European handguide study. *Arch Phys Med Rehabil* 2014; 95: 2253–2263. DOI: [10.1016/j.apmr.2014.06.022](https://doi.org/10.1016/j.apmr.2014.06.022).
5. Cochrane SK, Calfee RP, Stonner MM, et al. The relationship between depression, anxiety, and pain interference with therapy referral and utilization among patients with hand conditions. *J Hand Ther* 2022; 35: 24–31. DOI: [10.1016/j.jht.2020.10.006](https://doi.org/10.1016/j.jht.2020.10.006).
6. Craft RO, Smith AA, Coakley B, et al. Preliminary soft-tissue distraction versus checkrein ligament release after fasciectomy in the treatment of Dupuytren proximal interphalangeal joint contractures. *Plast Reconstr Surg* 2011; 128: 1107–1113.
7. Tocco S, Pedrazzini A and Pogliacomi F. Clinical approach in PIPJ flexion contracture conservative treatment: our experience. *Acta Biomed* 2016; 87(Suppl 1): 101–111.
8. Longo UG, De Salvatore S, Rosati C, et al. The impact of preoperative education on knee and hip replacement: a systematic review. *Osteology* 2023; 3: 94–112. DOI: [10.3390/osteology3030010](https://doi.org/10.3390/osteology3030010).
9. Zheng Y, Huang Z, Dai L, et al. The effect of preoperative rehabilitation training on the early recovery of joint function after artificial total knee arthroplasty and its effect evaluation. *J Healthc Eng* 2022; 2022: 3860991. DOI: [10.1155/2022/3860991](https://doi.org/10.1155/2022/3860991).
10. Moseley GL and Butler DS. Fifteen years of explaining pain: the past, present, and future. *J Pain* 2015; 16(9): 807–813.
11. Mallick-Searle TA-OX, Sharma K, Toal P, et al. Pain and function in chronic musculoskeletal pain-treating the whole person. *J Multidiscip Healthc* 2021; 14: 335–347.
12. Michie S, van Stralen MM and West R. The behaviour change wheel: a new method for characterising and designing behaviour change interventions. *Implement Sci* 2011; 6: 42–53. DOI: [10.1186/1748-5908-6-42](https://doi.org/10.1186/1748-5908-6-42).
13. Page MJ, McKenzie JE, Bossuyt PM, et al. The PRISMA 2020 statement: an updated guideline for reporting systematic reviews. *BMJ* 2021; 372: n71. DOI: [10.1136/bmj.n71](https://doi.org/10.1136/bmj.n71).
14. Downs SH and Black N. The feasibility of creating a checklist for the assessment of the methodological quality both of randomised and non-randomised studies of health care interventions. *J Epidemiol Community Health* 1998; 52: 377–384. DOI: [10.1136/jech.52.6.377](https://doi.org/10.1136/jech.52.6.377).
15. Büttner FA-O, Winters MA-O, Delahunt EA-O, et al. Identifying the 'incredible'! Part 2: spot the difference - a rigorous

- risk of bias assessment can alter the main findings of a systematic review. *Br J Sports Med* 2020; 54(13): 801–808.
16. Nascimento DC, Petriz B, Oliveira SC, et al. Effects of blood flow restriction exercise on hemostasis: a systematic review of randomized and non-randomized trials. *Int J Gen Med* 2019; 12: 91–100. DOI: [10.2147/IJGM.S194883](https://doi.org/10.2147/IJGM.S194883).
  17. Higgins J, Thomas J, Chandler J, et al. *Cochrane Handbook for Systematic Reviews of Interventions*. London, UK: Cochrane Collaboration, 2023. Available at: <https://training.cochrane.org/handbook/current> (accessed 20 June 2024).
  18. Alter TH and Ilyas AM. A prospective randomized study analyzing preoperative opioid counseling in pain management after carpal tunnel release surgery. *J Hand Surg Am* 2017; 42: 810–815. DOI: [10.1016/j.jhsa.2017.07.003](https://doi.org/10.1016/j.jhsa.2017.07.003).
  19. Gilmer G, Cluts L, Gibbons JL, et al. A randomized-controlled trial evaluating the impact of a web tutorial on perceptions and usage of opioids post-carpal tunnel release surgery. *J Hand Surg Glob Online* 2024; 6: 514–518. DOI: [10.1016/j.jhsg.2024.03.010](https://doi.org/10.1016/j.jhsg.2024.03.010).
  20. Stepan JG, Sacks HA, Verret CI, et al. Standardized perioperative patient education decreases opioid use after hand surgery: a randomized controlled trial. *Plast Reconstr Surg* 2021; 147: 409–418.
  21. Vincent S, Paskey T, Critchlow E, et al. Prospective randomized study examining preoperative opioid counseling on postoperative opioid consumption after upper extremity surgery. *Hand (New York, NY)* 2022; 17: 200–205. DOI: [10.1177/1558944720919936](https://doi.org/10.1177/1558944720919936).
  22. Zohar-Bondar A, Stepan JG, Chapman T, et al. The effect of standardized perioperative patient education on opioid use after minor soft tissue procedures distal to the wrist. *J Hand Surg Am* 2022; 47: 580.e1–580.e9. DOI: [10.1016/j.jhsa.2021.06.009](https://doi.org/10.1016/j.jhsa.2021.06.009).
  23. Multanen J, Uimonen MM, Repo JP, et al. Use of conservative therapy before and after surgery for carpal tunnel syndrome. *BMC Musculoskelet Disord* 2021; 22: 484. DOI: [10.1186/s12891-021-04378-3](https://doi.org/10.1186/s12891-021-04378-3).
  24. Núñez-Cortés R, Espinoza-Ordóñez C, Pommer PP, et al. A single preoperative pain neuroscience education: is it an effective strategy for patients with carpal tunnel syndrome? *Med Hypotheses* 2019; 126: 46–50. DOI: [10.1016/j.mehy.2019.03.013](https://doi.org/10.1016/j.mehy.2019.03.013).
  25. Bandholm T, Wainwright TW and Kehlet H. Rehabilitation strategies for optimisation of functional recovery after major joint replacement. *J Exp Orthop* 2018; 5: 44. DOI: [10.1186/s40634-018-0156-2](https://doi.org/10.1186/s40634-018-0156-2).
  26. Wainwright TW, Gill M, McDonald DA, et al. Consensus statement for perioperative care in total hip replacement and total knee replacement surgery: enhanced recovery after surgery (ERAS®) society recommendations. *Acta Orthop* 2020; 91: 3–19. DOI: [10.1080/17453674.2019.1683790](https://doi.org/10.1080/17453674.2019.1683790).
  27. The British Society for Surgery of the Hand. *Hand surgery in the UK: report of a working party*. London, UK: The British Society for Surgery of the Hand, 2018.
  28. Halsey LG, Curran-Everett D, Vowler SL, et al. The fickle P value generates irreproducible results. *Nat Methods* 2015; 12: 179–185. DOI: [10.1038/nmeth.3288](https://doi.org/10.1038/nmeth.3288).
  29. Fleischmann M and Vaughan B. Commentary: statistical significance and clinical significance - a call to consider patient reported outcome measures, effect size, confidence interval and minimal clinically important difference (MCID). *J Bodyw Mov Ther* 2019; 23: 690–694.
  30. Guy JGP, Kun Z, Bohm MK, et al. Vital signs: changes in opioid prescribing in the United States, 2006–2015. *MMWR: Morbidity & Mortality Weekly Report* 2017; 66: 697–704. DOI: [10.15585/mmwr.mm6626a4](https://doi.org/10.15585/mmwr.mm6626a4).
  31. Rodgers J, Cunningham K, Fau-Fitzgerald K, et al. Opioid consumption following outpatient upper extremity surgery. *J Hand Surg Am* 2012; 37(4): 645–650.
  32. Centers for Disease Control and Prevention. *Clinical practice guideline for prescribing opioids for pain — United States*. Atlanta, GA: Centers for Disease Control and Prevention, 2022. Available at: <https://www.cdc.gov/mmwr/volumes/71/rr/rr7103a1.htm> (accessed 03 March 2024).
  33. Sihota A, Smith BK, Ahmed SA, et al. Consensus-based recommendations for titrating cannabinoids and tapering opioids for chronic pain control. *Int J Clin Pract* 2021; 75: e13871. DOI: [10.1111/ijcp.13871](https://doi.org/10.1111/ijcp.13871).
  34. National Institute for Health and Care Excellence. *Joint replacement (primary): hip, knee and shoulder (NICE guideline, no. 157)*. London, UK: National Institute for Health and Care Excellence. Available at: <https://www.ncbi.nlm.nih.gov/books/NBK561385/> (2020, accessed 24 June 2024).
  35. Furlan AD, Malmivaara A, Chou R, et al. 2015 updated method guideline for systematic reviews in the Cochrane back and neck group. *Spine* 2015; 40: 1660–1673. DOI: [10.1097/BRS.0000000000001061](https://doi.org/10.1097/BRS.0000000000001061).