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Systematic Review

Efficacy of percutaneous electrolysis in upper limb tendinopathies: a systematic review

Lola Rouger[1] 0009-0000-5695-4231

Javier Guerrero Troyano[1] 0009-0009-7399-787X

[1] Centro de Fisioterapia Javier Guerrero, Plaza Serrano Berges 12, local, 50016 Zaragoza, España.

ABSTRACT

Introduction: Upper limb tendinopathies are a frequent cause of pain and disability. Percutaneous electrolysis (PE), applied under ultrasound guidance, is proposed as an invasive technique to modulate pain and support tissue repair.

Objectives: To determine the efficacy of percutaneous electrolysis in reducing pain and its effects on function, strength, joint range, pain to pressure threshold (PPT), potential ultrasound findings, and dosage.

Methods: A PRISMA review was conducted in PubMed, PEDro, Web of Science, and Cochrane. Inclusion criteria: adults with upper limb tendinopathy treated with percutaneous electrolysis, in whom at least one pain-related outcome measure was assessed. Exclusion criteria: cadaver, lower limb studies, non-inclusion of pain measurement, publications other than English, French, or Spanish. Quality was assessed using RoB2 (RCTs) and MINORS (non-randomized).

Results: We included 7 studies (RCTs, n=32-50; longitudinal study, n=36), with follow-up periods of up to 1 year. PE showed a significant reduction in pain in all studies, with more consistent results in the shoulder. Function improved significantly in most studies, with clinically relevant effects. Increases in strength, electromyographic activity, and ROM were observed, especially in the shoulder. The PPT showed heterogeneous results. Satisfaction was higher in the PE groups, and ultrasound changes showed early improvements in hypervascularization and later structural changes.

Discussion: In adults with upper limb tendinopathies, percutaneous electrolysis is a useful adjunct to exercise to achieve early analgesia, while functional gains seem to depend mainly on the exercise program. The heterogeneity of protocols and the risk of bias suggest that it should be considered adjuvant, not substitutive, and that RCTs with standardized dosimetry, a well-defined anatomical target, and 12-month follow-up ≥should be prioritized.

Conclusions: Percutaneous electrolysis can be considered an effective complement for modulating pain in upper-limb tendinopathies, but its contribution is secondary to exercise and requires validation in studies with greater methodological control.

Keywords: percutaneous intratissue electrolysis; tendinopathy; upper limb; pain; functionality.

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Correspondence: javiergt15@gmail.com

1. Introduction

Musculoskeletal conditions of the upper limb are a major cause of pain, disability, and healthcare resource utilization[1]. In the general population, approximately one in six people experiences shoulder pain, accounting for 1%-5% of primary care consultations and making it among the most frequent musculoskeletal reasons[2]. Within this picture, most cases are associated with mobility deficits, decreased strength, and limitations in daily and work activities[3-5].

The etiology of rotator cuff injuries is multifactorial, with repetitive loads and sustained postures, diabetes, hypertension, and hyperlipidemia among the possible factors[6]. Its high prevalence and chronicity entail a substantial socioeconomic burden, with an estimated cost of approximately €4,139/year per patient in Sweden, more than 80% of which is attributable to sick leave[7].

In the case of chronic tendinopathy, matrix disorganization is observed with misalignment of collagen fascicles[8], proliferation of fibroblasts[9], an increase in type III collagen in relation to type I[10], neovascularization mediated by angiogenic factors such as VEGF, and sensory innervation changes associated with mediators such as NGF[9,11].

Recent research has described alterations in inflammatory resolution mechanisms, including a reduction in pro-resolutive lipid mediators such as 15-epi LXA4 and MaR1 and an increase in the enzyme 15-PGDH, changes that contribute to persistent inflammation and make it difficult to repair and reorganize the tendon[12-14].

Percutaneous electrolysis (PE) has emerged as a minimally invasive technique aimed at stimulating tissue repair. It consists of applying a galvanic current through a solid, threadlike needle, under ultrasound guidance, directly to the degenerated tissue[15-16]. This stimulus has been described as triggering a localized electrochemical reaction that promotes a reparative inflammatory response, including type I collagen synthesis and tissue remodeling[17].

At the molecular level, the galvanic current activates the NLRP3 inflammasome and modulates IL-1β release, processes involved in tendon repair[18]. In animal models, PD has been shown to include gene expression changes consistent with extracellular matrix regeneration and reorganization[19]. These results provide pathophysiological support for the technique.

The dosage is controversial. The available evidence suggests that low doses primarily induce analgesic effects through modulation of peripheral and central sensitivity, with no clear evidence of structural changes[20]. On the other hand, high doses are associated with a more intense inflammatory response, capable of inducing structural changes in the womb, although accompanied by greater post-procedural pain and longer recovery[21-22]. A recent systematic review on intensities concluded that the quality of the evidence is low to moderate and that methodological heterogeneity precludes firm recommendations[22]. Experimental studies suggest that biological and regenerative effects vary with the dose applied, which could explain the clinical distinction between analgesic and restorative effects[20-21].

The safety of the technique has also been evaluated. Studies using ultrasound on cadavers have confirmed the anatomical validity of upper-limb approaches, such as the distal biceps tendon and the arch of Frohse[23], demonstrating safe trajectories with respect to neurovascu-

lar structures[24]. In vitro research has shown that the application of PE does not significantly increase tissue temperature[25]. The morphology, composition, and resistance of the needles used have been evaluated, guaranteeing the safety of their clinical use[26]. On the other hand, transient autonomic responses have been observed during the application of the technique, with no serious adverse events recorded[27-28].

Globally, several systematic reviews and meta-analyses that include different locations conclude that PE is effective in reducing pain and partially improving collagen function and remodeling. A recent systematic review indicated that PE has analgesic benefits compared to placebo, although it does not seem to outweigh the analgesic effect of corticosteroid injections[29], an aspect relevant to contextualize its positioning within the conservative clinical approach to tendinopathies. However, limitations persist due to heterogeneity in protocols and variation in methodological quality[29-31].

Currently, there are no systematic reviews specifically focused on the use of PE for upper limb tendinopathies. The existing evidence is fragmented into individual primary studies that jointly address different interventions or anatomical locations. In this context, it is necessary to carry out a systematic review to evaluate the clinical efficacy of PD in upper limb tendinopathies, considering pain as the main variable, and analyzing function, strength, range of motion, ultrasound changes, as well as the influence of treatment dosage and safety profile as secondary variables.

2. Methods

This study is a systematic review prepared following the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines[32].

2.1 Eligibility Criteria

The inclusion criteria defined for this review were as follows: studies published between 2010-2025, which included PE as the main intervention or not, and which evaluated at least the pain variable. Both randomized clinical trials and non-randomized studies were accepted as long as they presented a clear methodology and enabled assessment of the intervention's impact on any region of the upper limb. This decision was made to broaden the scope of the review, as the technique is emerging and does not yet have a large number of controlled trials.

On the other hand, studies carried out on cadavers were excluded, as well as those that did not allow access to full text or that were written in languages other than French, Spanish, or English.

2.2 Data Sources and Search Strategy

The search was conducted using the PICO method, which structures the research question into four components: patients, intervention, comparison, and outcome. The keywords "Tendinopathy" and "Percutaneous electrolysis" were used with their synonyms, along with the Boolean AND and OR operators, including [MeSH] terms. The C and O of PICO were not used as restrictive filters to ensure that no items were omitted. The databases consulted were PubMed, PEDro, Web of Science, Cochrane. The complete search strategy is attached hereto.

Table 1. PICO method with keywords.

PICO METHOD		KEYWORDS
Population (1)	Adults with tendinopathy	"Tendinopathy"[MeSH] OR "tendinopathy" OR "tendinopathies" OR "tendonopathy" OR "tendonopathies" OR "tendinitis" OR "tendinitides" OR "tendonitis" OR "tendinosis" OR "tendinoses" OR "tendonosis" OR "tendonoses"
AND		
Intervention (2)	Intratissue percutaneous electrolysis intervention	"Percutaneous electrolysis" OR "Percutaneous needle electrolysis" OR "Intratissue percutaneous electrolysis" OR "EPI"
AND		
Comparison (3)	Other interventions or placebo	
AND		
Outcome (4)	Pain, function, ultrasound changes, ROM, strength, PPT	

2.3 Selection of Studies

The selection of studies was carried out in two phases. In the first, a researcher (LR) executed the search strategy and removed the duplicates using Mendeley. A second review author (JG) verified that this first phase was completed correctly.

In the second phase, the inclusion and exclusion criteria were applied by reviewing the titles and abstracts of the articles (LR and JG). Those that did not meet the established requirements were discarded.

2.4 Data Extraction Process

Data extraction was performed by a single researcher (LR), using a custom log sheet. First, the initial list was created in Word, and duplicates were removed. Excel was then used to classify the articles according to the type of study, the year of publication, the anatomical region, the type of intervention, and the variables analyzed. All articles were cross-checked by a second reviewer (JG). In those cases where there were discrepancies or doubts regarding eligibility, the final decision was reached through discussion and consensus among the investigators.

2.5 Study Variables

The main variable studied is pain; secondary variables include functionality, strength, EMS, ROM, degree of satisfaction, and ultrasound changes (vascularization, echogenicity, tendon thickness).

2.6 Assessment of Methodological Quality/Risk of Bias

The methodological quality of the included studies was assessed using two tools, depending on each study's design.

Randomized clinical trials used the Cochrane ROB 2 tool, which assesses risk of bias across five domains: randomization, deviations from intended interventions, incomplete outcome data, outcome measurement, and reporting bias. Each domain is answered using standardized options: Yes (Y), Probably Yes (PY), No (N), Probably No (PN), No information (NI). Then the risk of

bias in each domain is determined: Low risk, Some concerns, High risk, and finally, a global judgment of the study is generated. To facilitate this assessment, the Excel template available on Cochrane's official website was used, which allows the responses to each item to be recorded in a structured way and an overall assessment of the risk of bias to be obtained. The algorithm's automatic result was reviewed to check for discrepancies; the details are in the annex.

On the other hand, for non-randomized studies, the Methodological Index for Non-Randomized Studies (MINORS)[33] tool was applied, which assesses aspects such as objective clarity, consecutive patient inclusion, prospective data collection, adequacy of outcome measures, and quality of statistical analysis. This tool comprises 12 items: 8 items are applicable to non-comparative studies, and 4 items are additional for comparative studies. Each item is scored with: 0 (Not reported), 1 (Reported but inadequate), and 2 (Reported and adequate). The maximum total score is 16 for non-comparative studies and 24 for comparative studies. There is no automatic algorithm to determine overall quality, but higher scores are considered to reflect better methodological quality, with high methodological quality between 13 - 16 points, moderate quality between 9 - 12, and low quality below 9, which is considered for non-comparative studies. For comparative studies, a high methodological quality is between 19 - 24 points, moderate between 13 - 18 points, and low if it is less than 13.

3. Results

3.1 Selection of Studies.

Following a literature search of the Web of Science (WOS), PEDro, PubMed, and Cochrane databases, 170 records were initially identified. After eliminating 38 duplicates, the titles and abstracts of the remaining studies were evaluated against the established inclusion criteria. At this stage, 125 articles were excluded for failing to meet these criteria. There were no additional exclusions after full-text reading, resulting in 7 studies included in the review.

3.2 Characteristics of the Study.

3.2.1 Sample Characteristics

The characteristics are shown in Table 2. The included studies were conducted between 2014 and 2024, with the most recent studies conducted in 2024. Randomized clinical trials included between 32 and 50 participants each, and the only longitudinal study was of 36 patients. Most studies have focused on the shoulder[34–37], and three have examined the elbow[38–40]. The interventions lasted between 3 and 5 weeks, followed by a follow-up of up to one year. One study did not follow up[34]; two studies by Rodríguez-Huguet et al. followed up for 3 months[38,39]; two studies followed up for 6 months[35,36]; and two studies followed up for 1 year[37,40].

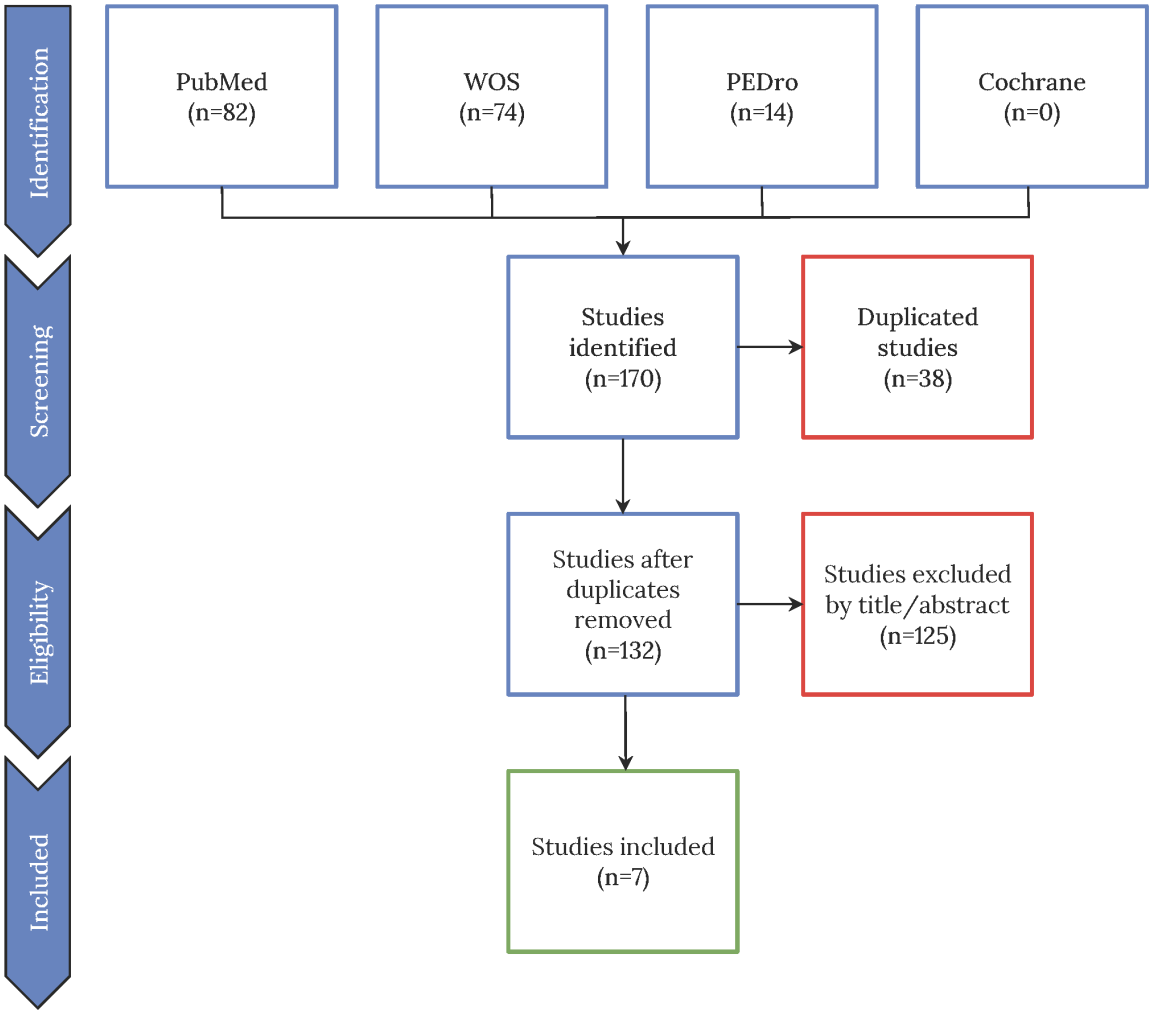

Figure 1. Flowchart on the choice of items.

The variables included in the studies were measured with different scales. The NRPS (Numeric Pain Rating Scale) was used for pain assessment[34–40], with several studies using it as the primary variable [34,35,37–40]. Function was measured using the DASH (Disabilities of the Arm, Shoulder and Hand) scales[34–36,40], SPADI (Shoulder Pain and Disabilities Index)[35,36], and PRTEE (Patient-Rated Tennis Elbow Evaluation)[39]. Quality of life was assessed using the SF-12 (Short Form-12 Health Survey)[38]. Muscle strength measurements were recorded in one study[35]. Joint mobility was assessed using range of motion (ROM)[37–39], while pain sensitivity was determined by pressure pain threshold (PPT)[36–39]. One study incorporated surface electromyography (sEMG)[38]. In addition, ultrasound structural parameters[35,40] and patient-perceived outcome measures such as satisfaction[40] and global perception of change (GROC)[36] were included. The summary and interpretation of these variables are in the Annex (Table A1).

The minimally invasive intervention was PE, and it was analysed in isolation or combined with other interventions, mainly therapeutic exercise (TE) and on other occasions with manual therapy (MT), or stretching, while in others it was compared with techniques such as deep dry needling (DN), percutaneous nerve stimulation (PNS), transcutaneous electrical nerve stimulation (TENS), therapeutic ultrasound (US) or myofascial vacuum therapy (VT).

Table 2. Features of the items included

Article (Author, Year)	Design	Intervention	Duration	Population	Groups	Variables	Tendon treated	PE App
Arias-Buriá et al., 2015	*RCTs*	*PE + exercises vs. exercises alone in sub-acromial pain*	*4 weeks*	*36 patients*	*-PE + EE (n=17)* *-EE (n=19)*	*NPRS DASH*	*Supraspinatus*	*Ultrasound* *350 µA / 1.2 min / 1x week* *EPTE V01*
Miguel-Valtierra et al., 2018	*RCTs*	*PE + MT + exercises vs. MT + exercises in subacromial pain*	*5 weeks + follow-up (6 months)*	*50 patients*	*-PE + MT + EE (n=25)* *-MT + EE (n=25)*	*DASH NPRS* *SPADI* *PPT* *GROC*	*Supraspinatus*	*Ultrasound* *350 µA / 90 sec / 1x week* *EPTE V01*
Góngora-Rodríguez et al., 2024	*RCTs*	*PE + PNS + EE vs. TENS + US + EE in supraspinatus tendinopathy*	*5 weeks + follow-up (24 weeks)*	*50 patients*	*-PE + PNS + EE (n=25)* *-TENS + US + EE (n=25)*	*NPRS ultrasound strength DASH, SPADI*	*Supraspinatus*	*Ultrasound* *350 µA / 72 sec / 1x week* *EPTE*
Rodríguez-Huguet et al., 2020	*RCTs*	*PE + EE vs. DN + EE* *in supraspina-tus tendinop-athy*	*4 weeks + follow-up (1 year)*	*36 patients*	*-PE (n=18)* *-DN (n=18)*	*NPRS* *ROM* *PPT*	*Supraspinatus*	*Ultrasound* *350 µA / 1.2 min / 1x week* *EPTE*
Rodríguez-Huguet et al., 2020	*RCTs*	*PE + EE vs. DN + EE in* *lateral epicon-dyalgia*	*4 weeks + follow-up (3 months)*	*32 patients*	*-PE + EE (n=16)* *-DN + EE (n=16)*	*NPRS ROM* *PPT* *SF12*	*Tendon muscles* *epicondyle*	*Ultrasound* *350 µA / 1.2 min / 1x week* *EPTE*
Rodríguez-Huguet et al., 2024	*RCTs*	*VT+PE+EE vs. MT+* *US + EE in* *lateral epicon-dyalgia*	*4 weeks + follow-up (3 months)*	*40 patients*	*-VT+PE+EE (n=20)* *-MT+US+EE (n=20)*	*NPRS* *ROM* *PPT* *PRTEE*	*Common exten-sor tendon*	*Ultrasound* *350 µA / 80 sec / 1x week* *EPTE*
Valera-Garrido et al., 2014	*Longi-tudinal study of repeated mea-sures*	*PE + EE +* *stretches in* *chronic lateral epicondylitis*	*3 weeks + follow-up (1 year)*	*36 patients*	*- PE + EE*	*NPRS DASH* *Ultrasound* *Satisfaction*	*Extensor carpi radialis brevis*	*Ultrasound* *4-6 mA, 3 sec x3* *EPI ECC Cesmar*

RCT=randomized clinical trial/ PE= percutaneous electrolysis/ EE= exercises/ DN= dry needling/ MT= manual therapy/ PNS= peripheral nerve stimulation/ TENS= transcutaneous electrical nerve stimulation/US= therapeutic ultrasound/VT= vacuum (myofascial) therapy/NPRS= Numeric Pain Rating Scale/VAS= Visual Analog Scale/DASH= Disability of the Arm, Shoulder and Hand /SPADI= Shoulder pain and disability index /PRTEE-S= Patient-Rated Tennis Elbow Evaluation /PPT= Pressure Pain threshold/ROM= Range of motion/sEMG= surface electromyography

PE was always applied under ultrasound control. Regarding intensity, Góngora-Rodríguez et al.[35] used an intensity of 350 µA for 72 seconds, 4 studies spent more time, namely 80 seconds [34,37–39] and 90 seconds for Miguel Valtierra et al.[36]. All of them used the EPTE device. On the other hand, one study used Cesmar PPE (1st version) at higher intensity: Valera et al.[40] (4-6 mA x 3s x 3). In all studies, the experimental group received one session of PE per week.

3.2.2 Intervention and Follow-up

All items included therapeutic exercise as part of the intervention. In all clinical trials focused on the shoulder, the same type of program was used, consisting of an exercise targeting the supraspinatus muscle, another on the infraspinatus muscle, and a scapular control exercise. Although it is not specified whether these exercises followed a standardized protocol, the content was homogeneous among the studies, with only the number of repetitions varying. In gener-

al, patients performed three sets of 10-12 repetitions. The number of supervised sessions also varied across studies. In the work of Arias et al.[34], a single session was carried out under the supervision of a physiotherapist, whereas in the other studies, continuous follow-up was provided by physiotherapy professionals. As for the elbow studies, similar exercise programs were also used, with three sets of 10 repetitions performed in all cases. However, the load used was adapted according to the study: Valera et al.[40] adjusted the weight individually, whereas the other two studies used a fixed load of 1 kg[38,39]. In both shoulder and elbow studies, patients were instructed to perform the exercises twice a day.

In addition to therapeutic exercise (TE), the included studies incorporated other invasive techniques, such as dry needling and percutaneous peripheral nerve stimulation (PNS)[35]. In two of the clinical trials, dry needling was compared to a group treated by percutaneous electrolysis (PE)[37,38]. In both cases, dry needling was performed once a week for four weeks using Hong's rapid entry technique, which targets specific muscles.

In the study by Góngora-Rodríguez et al.[35], the invasive PNS technique was not used as a comparator between groups but was added to PE treatment. In this study, a group that received PE + PNS + therapeutic exercise (TE) was compared with another group that received transcutaneous electrical nerve stimulation (TENS) + therapeutic ultrasound (US) + TE.

In this study, percutaneous peripheral nerve stimulation (PNS) was applied after the PE intervention at a low frequency (10 Hz) for 90 seconds to elicit a sensory or motor response[34].

The control group received TENS and therapeutic ultrasound five days a week. The TENS was applied for 20 minutes at 150 Hz with a 100 µs pulse duration, while the ultrasound was applied at 1 MHz at 1.5 W/cm^2 in continuous mode to the painful area for 5 minutes.

One study in each anatomical region included manual therapy. In the case of the shoulder, the article by Miguel Valtierra et al.[36] conducted one session per week for 5 weeks, and consisted of passive joint mobilizations targeting the glenohumeral, acromioclavicular, sternoclavicular, scapulothoracic, and associated soft tissue joints. In the case of the elbow, Rodríguez-Huguet et al.[39] performed friction maneuvers on the epicondylar muscles and forearm region for 5 minutes, then using a 1MHz US device, with a power of 1.5W/cm^2 in pulsatile mode in the painful area. An elbow study by Valera et al.[40] included a twice-daily stretching program consisting of 3 sets of 7 repetitions, lasting 45 seconds each and leaving 30 seconds of rest between repetitions.

Myofascial vacuum therapy has been used in a single study by Rodríguez-Huguet et al.[39]. The negative pressure applied was 100 mbar, with intervals of 0.6 seconds of active pressure and 0.3 seconds of pause. The VT treatment time per session was 15 minutes, and the time for 20 cm2 applicators was in the epicondylar muscles.

The summary of the co-interventions is presented in the Annex (Supplement A2).

3.3 Assessment of Methodological Quality/Risk of Bias

Most studies present low risk in the domains of randomization, missing data, outcome measurement, and reported outcome selection (Figure 2). However, some methodological concerns were identified. The study by Góngora et al. was classified as moderate risk during the randomization process due to a lack of sequence concealment and baseline differences in the DASH variable. In addition, three studies reported baseline differences in NPRS or DASH, which were adjusted for in ANCOVA.

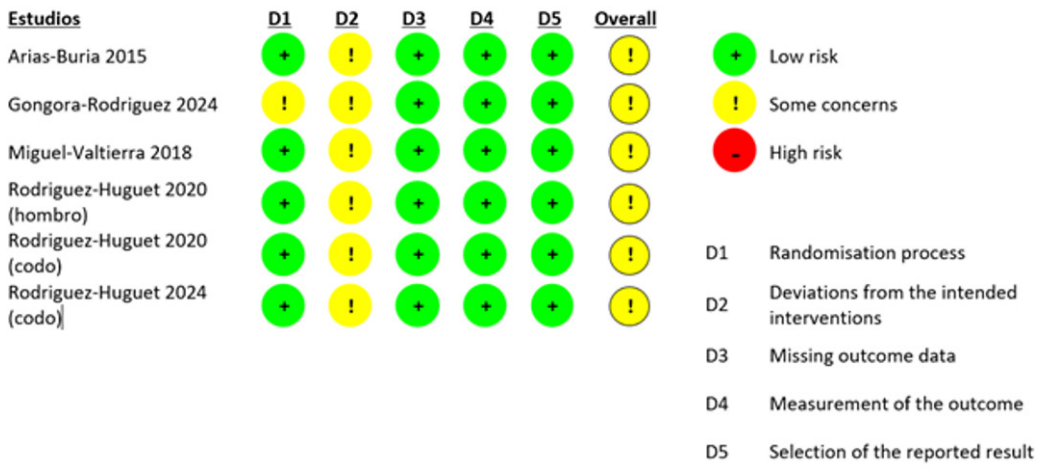

Figure 2. Assessing risk of bias with Rob2

Note. Rob legend: "+"=Low risk; "!" = Moderate risk (Some concerns); "-"=High risk. D1 = randomization process; D2 = deviations from planned interventions; D3=missing data; D4=Outcome Measurement; D5=selection of the reported outcome

Regarding blinding, all studies were single-blind, which led to the classification of this domain as moderate risk. In the domains of missing data and outcome measurement, the risk was judged to be low. The losses were minimal and justified, and although the NPRS is a self-reported measure sensitive to knowledge of the treatment received, it is considered the standard in this type of study. Regarding the selection of the reported outcome, it was assumed that the investigators followed established protocols; however, if the available information was considered insufficient, a moderate risk classification could be justified.

Overall, no study was at low risk across domains. All showed at least some methodological concern. On the other hand, the non-randomized study by Valera et al. was evaluated using the MINORS tool, yielding a score of 13/16, indicating good methodological quality. It met the main criteria, applied validated clinical and ultrasound measures, and had a blinded evaluator. However, it had two limitations: a 11% loss to follow-up, exceeding the recommended threshold, and the absence of a sample size calculation, which limits interpretation of statistical power. Despite this, the design was suitable for a non-comparative study. The full results of RoB 2 and MINORS are available in the annexes.

3.4 Summary of Results

Table 3. Synthesis of results by study.

Author and year	Design / Theme	Interventions	Variables	Measurements/ Tracking	Results
Arias-Buria et al. 2015	RCTs Shoulder	Gr1: **PE** + **EE** Gr2: **EE** **PE:** EPTE 350 µA/80s/ 1x week/ x4 weeks **EE:** 3x10 rep/ 2x day/ x4 weeks (supra+ infra+ scapular control)	NPRS (average, worst, lower) DASH	2 weeks Post tto (1mes)	-**NPRS** (mean and worse): superior improvements with PE + EE/mean (F=10.447; p=0.003/ worse (F=12.269; p<0.001) / SDM>2.01 post and follow-up, for worse pain SDM = 3.20 -**DASH:** significant interaction p=0.008/ large effect sizes SDM>2.52 in both follow-ups, in post significant difference, but <MCID -Short-term clinical effects, the authors ask for more long-term data
Valtierra et al. 2018	RCTs Shoulder	Gr1: **PE** + **MT** + **EE** Gr2: **MT** + **EE** **PE:** EPTE 350 µA / 90 s / 1x week / x5 weeks **EE:** 3x12 rep /2x day/ x5 weeks (supra+ infra+ scapular control) **MT:** passive mob 1x week/ x5 weeks	NPRS (mean, worst, lowest) DASH SPADI PPT GROC	Post treatment 3 months 6 months	-**NPRS:** all p<0.001, moderate effect size post tto, large to 3-6 months -**DASH:** no differences between groups p=0.051 -**SPADI:** p<0.001, SDM>1.06, large effect size on all traces -**PPT**: no differences -**GROC**: major clinical improvement with PE
Góngora-Rodríguez et al. 2024	RCTs Shoulder	Gr1: **PE** + **PNS** + **EE** Gr2: **TENS** + **US** +EE **PE:** EPTE 350 µA/ 72 s/ 1x week/ x4 weeks **EE:** 3x10 rep / 2x day / x4 weeks (supra+ infra+ scapular control) **PNS**: EPTE 10 Hz/ 90s/ 1x week/ x4 week **TENS:** 100µs/ 150Hz/ 20min/ 5x week/ x4 weeks **US**: 1MHz/ 1.5W/cm2/ 5min/ 5x week/ x4 weeks	NPRS DASH SPADI STRENGTH EMS Ultrasound	Post tto 3 months 6 months	-**NPRS, DASH, SPADI, STRENGTH**, **EMS, ultrasound,** significant improvements at post, 12, and 24 weeks -**Ultrasound**: hypo echogenicity/hypervascularization reduction and improvement of tendon thickness in favor of the PE + PNS group. Post-treatment, only significant hypervascularization

Author and year	Design / Theme	Interventions	Variables	Measurements/ Tracking	Results
Rodríguez-Huguet et al. 2020	RCTs Shoulder	Gr1: **PE** + **EE** Gr2: **HP** + **EE** **PE:** EPTE 350 µA/80s/ 1x week/ x4 weeks **DN**: trapeze, Hong technique 1x week/ x4 weeks **EE:** 3x10 rep / 2x day / x4 weeks (supra+ infra+ scapular control)	NPRS ROM PPT	Post treatment 1 month 1 year	-**NPRS:** significant difference in favor of PE, moderate effect size, maintained at one year p=0.002 -**ROM**: significant difference for F, E, RE with moderate effect size for F, small for E and RE, no differences for ABD and RI -**PPT**: significant difference p=0.002, large effect size for proximal/media PPTs
Rodríguez-Huguet et al. 2020	RCTs Elbow	Gr1: **PE** + **EE** Gr2: **HP** + **EE** **PE:** EPTE 350 µA/ 80s/ 1x week/ x4 weeks **DN**: supinator and ECRL, 1x week/ x4 weeks **EE:** 3x10 rep / 2x day / x4 weeks (1kg F/E doll)	NPRS ROM PPT MCSF-SF12 PCS-SF12	Post tto 1 month 3 months	-**NPRS:** significant differences between groups from post to end of follow-up p<0.001 -**ROM**: significant difference in all measurements for F, no differences for E, supination, pronation -**SF-12**: no relevant changes -Time-Sustained Effects for **NPRS** and **ROM**
Rodríguez-Huguet et al. 2024	RCTs Elbow	Gr1: **PE** + **VT** + **EE** Gr2: **MT** + **US** + **EE** **PE:** EPTE 350 µA/ 80 s/ 1x week/ x4 weeks **VT:** 100mbar/ 0.6sON-0.6sOFF/ 15min, header 20cm² **MT:** frictions/ 5min **US**: 1MHz/ 1.5 W/cm²/ 5min/ pulsed mode/ 5x weeks/ x2 weeks **EE:** 3x10 rep / 2x day / x4 weeks (1kg F/E doll)	NPRS ROM PPT PRTEE-S	Post tto 1 month 3 months	-**NPRS:** significant difference p<0.001/ ES=0.408 -**ROMs**: significant difference from 1 month for supination, not significant for F, E, pronation -**PPT**: Not significant -**PRTEE-S**: pain p=0.001, ES=0.377/ specific function p=0.004, ES=0.306/ total p=0.001, ES=0.355 -PE+VT group advantage maintained at 1 and 3 months for pain, ROM, PPT, and function
Valera-Garrido et al. 2014	Study of repeated measures Elbow	**PE** + **EE** + **Stretches** **PE:** EPI 4-6 mA, 3s x3/ 1x week/ x4 weeks **EE:** 3xup to 10 reps/ 2x day/ x4 weeks + 1x day/ x6 weeks **Stretches**: 3x7 rep/ 45s bra/ 30s rest/ 2x day	You go DASH PPT Ultrasound Satisfaction	Post treatment 6 months 1 year	Results of a single group: between pre and post -**VAS:** significant improvements p<0.001 -**DASH:** significant improvements p<0.001 -**PPT**: Significant improvements p<0.001 -**Ultrasound**: significant hypervascularization from discharge and maintained, hypoechogenicity as well -**Satisfaction**: post treatment 86%, 26 and 52 weeks 100%

PE= percutaneous electrolysis/ EE= exercises/ DN= dry needling/ MT= manual therapy/ PNS= peripheral nerve stimulation/ TENS= transcutaneous electrical nerve stimulation /US= therapeutic ultrasound /VT= vacuum (myofascial) therapy/NPRS= Numeric Pain Rating Scale/VAS= Visual Analog Scale/DASH= Disability of the Arm, Shoulder and Hand /SPADI= Shoulder pain and disability index /PRTEE-S= Patient-Rated Tennis Elbow Evaluation / PPT= Pressure pain threshold/ROM= Range of motion /EMS= Surface electromyography

3.4.1 The results obtained for each variable

First item: Pain (NPRS, PP). In all the studies reviewed, a significant reduction in post-treatment pain was observed, with intergroup differences of clinically relevant magnitude (greater than 1.1 in all cases). However, the absence of blinding in the study design introduces a significant risk of performance bias, which could have influenced the magnitude of self-reported pain scores.

For the shoulder, all included studies reported a significant reduction in pain after the procedure.

At two weeks, Arias-Buria et al.[34] observed an intergroup difference of 1.5 points on the NPRS scale.

At post-treatment assessment, all studies reported statistically significant and clinically relevant differences in pain measures (NPRS or VAS) [34–36,40].

Two studies with three- and six-month follow-ups (Miguel Valtierra et al.[36] and Góngora et al.[35]) showed that intergroup differences were maintained or increased in both follow-up periods.

Finally, the study by Rodríguez-Huguet et al.[37], with a one-year follow-up, reported an intergroup difference favoring PE over dry needling.

At the elbow, significant differences were observed in all studies, although the magnitude of the clinical effect varied according to the technique applied

In the Rodriguez-Huguet 2024 article[39], the group treated with vacuum therapy (VT) showed a smaller effect than the DN group. In the PRTEE-S test, statistically significant improvements have been observed in pain[39].

At three months, the results were maintained or slightly increased[38-39]. At 12 months, Valera's study[40] reported improvements without recurrence, but did not provide intermediate values or data at 6 months.

Overall, there is consistent evidence of a significant reduction in pain in the PE groups, although it is not possible to completely isolate the specific effect of this intervention, with more homogeneous results in the shoulder than in the elbow.

Pressure Pain Threshold (PPT)

The available evidence on PPT shows globally heterogeneous results, with no consistent pattern between studies.

On the shoulder, the findings are contradictory. While Rodríguez-Huguet et al.[37] observed a clear improvement in favor of the group treated with PE, Miguel Valtierra et al.[36] did not find significant differences between groups, although they reported a progressive increase in PPT in both groups.

In the elbow, the findings were also mixed. Some studies report significant short-term improvements, such as those reported by Rodríguez-Huguet et al. 2020[38] at 1 and 3 months. On the other hand, Rodríguez-Huguet et al. 2024[39] found no significant differences in the global analysis, although specific improvements were observed in favor of the experimental group immediately after treatment and after one month. For their part, Valera et al.[40] documented an immediate

and clinically relevant increase in PPT, exceeding 20 points at discharge and maintained or even increased at six weeks.

The evidence is inconsistent, with a tendency to increase the PPT that does not always reach significant differences between groups, with the absence of a uniform pattern between studies, locations, and follow-up times.

Second item: Functionality (DASH, SPADI, PRTEE-S)

Studies that evaluated function showed significant and clinically relevant improvements [34-36; 39,40], except in Miguel Valtierra[36], who did not find statistically significant differences.

DASH

In the shoulder, clinically relevant improvements are seen in most studies (two)[34,35]. Arias-Buria et al.[34] recorded a difference of 12.2 points after two sessions (higher than the MCID), while Góngora et al. 38 reported a post-treatment difference of 33.16 points, with very high effect sizes (d>2). On the other hand, Miguel Valtierra et al.[36] did not observe a statistically significant difference (p=0.051). In the elbow, Valera et al.[40] report post-treatment and 6-week reductions, both statistically significant.

SPADI

Both studies have seen significant and clinically relevant reductions[35,36]. Improvements range from 12.5 to 35.36 points, reaching and exceeding the MCID in both the short and medium term.

PRTEE-S (elbow)

In Rodríguez-Huguet et al.[39], significant improvements were observed in specific function (p=0.004).

Overall, the evidence on function is consistent, with clinically relevant improvements after interventions, especially in shoulder, while in elbow evidence is more limited.

Third item; Force, EMS, ROM

A single study, Góngora et al.[35], analyzed muscle strength and electromyographic activity. Significant increases in abduction strength (ABD), internal rotation (IR), and external rotation (ER) were observed both post-treatment and at 3 and 6 months.

In the EMS analysis, significant differences between groups were observed across all measurements of the supraspinatus and trapezius.

Joint Mobility (ROM)

One study assessed the range of motion in the shoulder[37], and two assessed the range of motion in the elbow[38,39].

In the shoulder, Rodríguez et al.[37] observed significant improvements in flexion, extension, and external rotation.

In the elbow, the results are more variable: Rodríguez Huguet et al. 2020[38] showed significant and sustained improvement in flexion, while Rodríguez Huguet et al. 2024[39] showed only significant improvement in supination, starting at the first month and maintained at three months.

Overall, the available evidence shows improvements in strength, electromyographic activity, and ROM, but is limited by the small number of studies and suggests improvements after interventions.

Fourth item: Satisfaction

Two studies evaluated patient-perceived outcomes[36,40].

In Valera et al.[40], at 6 weeks, 83.3% of patients reported a "successful" outcome, and at 26- and 52-week follow-ups, all participants maintained this perception.

In Miguel Valtierra et al.[36], using a cut-off point of ≥+5 on the GROC scale, a higher percentage of successful results was recorded in the PE+MT+EE group. At 3 months, 56% of the ultrasound-guided group achieved large changes compared with 12% in the unguided group ($p=0.006$), and at 6 months, these differences increased to 68% versus 22% ($p<0.001$).

The available studies show more favorable satisfaction and perceptions of functional and clinical improvement in groups that include PE.

Fifth item: Ultrasound changes

A shoulder study[35] and an elbow study[40] analyzed structural modifications using ultrasound. In both cases, an early and significant decrease in hypervascularization was observed post-treatment. A progressive reduction in hypoechogenicity has been observed from 12 weeks onward, with significant differences in the shoulder[35]. Later, changes in tendon thickness were observed: in the shoulder, with significant differences at 12 weeks that were maintained at 24 weeks; in the elbow[40], with a favorable trend, although without reaching statistical significance.

Evidence is limited (one study in each region), but suggests favorable ultrasound changes after interventions, with significant results in the shoulder, mainly in intermediate phases of follow-up.

4. Discussion

4.1. Synthesis of main findings and heterogeneity between studies

Overall, the included studies suggest that percutaneous electrolysis may provide early analgesia when applied as an adjuvant to therapeutic exercise in upper limb tendinopathies. In shoulder studies, pain reduction tends to be maintained or increased in the medium term, especially between 3 and 6 months, and even persists at 12 months compared to dry needling in some trials. In lateral epicondylalgia, although clinical improvements are also observed, evidence for the maintenance of long-term analgesic effect is less consistent.

However, the interpretation of these results should be done with caution due to the marked clinical and methodological heterogeneity between studies. In most trials, therapeutic exercise was the common component of treatment, but a wide range of co-interventions were added, such as dry needling, percutaneous neuromodulation, manual therapy, TENS, ultrasound or vacuum therapy. In addition, the doses of these interventions were not always equivalent between groups, making it difficult to isolate the specific effect of percutaneous electrolysis and potentially increasing the influence of non-specific or contextual effects.

There was also heterogeneity in the outcome variables used. Pain was measured using VAS or NPRS, functionality using different instruments such as DASH, SPADI, or PRTEE, and mechanosensitivity using pressure pain threshold. However, not all studies assessed the same domains or used the same measurement points, particularly for PPT. Likewise, the ultrasound variables were not homogeneous: some studies analyzed hypervascularization, others tendon thickness, hypoechogenicity, or structural changes, and some used different follow-up times. This variability in scales, measurement points, ultrasound procedures, dosing, and co-interventions limits direct comparison between studies and makes it difficult to draw firm conclusions about the true magnitude of the effect.

4.2. Role of exercise versus the specific effect of percutaneous electrolysis

A relevant aspect is that, in the included studies, the control groups were usually not pure placebos, but active treatments. For example, in the study by Rodríguez-Huguet et al.[37], the comparator group received exercise and dry needling. Therefore, when percutaneous electrolysis combined with exercise yields better results, this difference should be interpreted as a possible additional gain on top of an already potentially effective active treatment, and not as a substitution for therapeutic exercise.

This interpretation aligns with the current literature, which identifies exercise as the central pillar in the treatment of tendinopathies and musculoskeletal pain in the shoulder and elbow. Passive interventions, including dry needling, shock waves, or percutaneous electrolysis, appear to show better results when integrated within structured physiotherapy and progressive loading programs, rather than when applied in isolation[49-53]. In this sense, percutaneous electrolysis should be considered an adjunctive intervention, potentially useful for reducing pain and mechanosensitivity and facilitating exercise progression, but not as an alternative to the active rehabilitation program.

In the functional field, the results must also be interpreted from this perspective. In shoulder studies, functional differences were heterogeneous: in Góngora-Rodríguez et al.[35], improvements far exceeded the MCID; in Arias-Buria et al.[34], they were close to the clinical threshold; and in Miguel Valtierra et al.[36], no significant differences were observed. The use of generic instruments, such as the DASH, may limit sensitivity to change, whereas more specific scales, such as the SPADI for the shoulder or the PRTEE for the elbow, could better capture functional evolution. In general, functional improvement appears to be consolidated in the medium term, consistent with the central role of therapeutic exercise.

Regarding electromyographic strength and activity, the evidence comes from a single randomized clinical trial, which limits the generalizability of the results. In Góngora-Rodríguez et al.[35], both groups performed the same exercise program, comparing PE + PNS + eccentric exercise versus TENS + US + eccentric exercise. The improvements in abduction strength, internal and external rotation, along with the effects observed on the sEMG of the supraspinatus, are physiologically plausible and consistent with the reduction of pain and the increase in PPT. However, it is not possible to attribute these effects exclusively to PD, as all participants exercised. This reading aligns with studies on subacromial pain and the rotator cuff, in which scapular strength-

ening and control programs account for a large share of the clinical and strength improvements in the medium term[54-56].

4.3. Comparison with evidence in the lower limb

The findings of this review are partially consistent with the available evidence in tendinopathies and musculoskeletal disorders of the lower limb. The review by Augustyn et al.[43] suggests that percutaneous electrolysis may obtain greater benefits when combined with eccentric exercise, which is consistent with the interpretation of the upper limb results.

In plantar fasciitis, Fernandez-Rodriguez et al.[44] observed significant reductions in pain with sustained effects at 12-24 weeks versus placebo. On the other hand, Al-Boloushi et al.[45] reported decreases in pain from the first session and maintenance through 52 weeks, although there were no differences between percutaneous electrolysis and dry needling for some pain variables, with differences favoring PE in long-term quality of life.

In patellar tendinopathy, the results are more mixed. López-Royo et al.[46] found no advantage of adding PE or dry needling to exercise compared with sham interventions of 10 and 22 weeks, as all groups showed significant intragroup improvements in pain and disability. In addition, a secondary analysis of the aforementioned study by Lopez-Royo et al.[47] showed similar intra- and intersessional pain reductions with all the interventions, underscoring the potential influence of contextual effects. As observational support, Muñoz-Fernández et al.[48] reported pain reduction at 3 weeks and near-absence at 8 weeks with PE combined with exercise; however, as this is observational evidence, these findings should be considered preliminary.

Therefore, the evidence in the lower limb reinforces the idea that PE may be promising as an adjunctive intervention, but also shows that its specific effect against placebo, sham, or other active treatments remains not fully established. This comparison supports a cautious interpretation of the magnitude of the effect observed in the upper limb.

4.4. Clinical and ultrasound variables and dosage of percutaneous electrolysis

The increase in PPT observed in some studies supports a possible reduction in mechanosensitivity and reinforces the physiological plausibility of the analgesic effect. However, heterogeneity in measurement points, applied pressure, anatomical location, and follow-up times limits direct comparison between studies.

The changes in range of motion were of modest magnitude. In the shoulder, Rodríguez-Huguet et al.[37] observed significant improvements in flexion, extension, and external rotation. At the elbow, differences in flexion were reported in Rodríguez-Huguet et al.[38] and in supination in Rodríguez-Huguet et al.[39]. Although the use of a goniometer provides an objective measure, the evaluation of ROM can be affected by procedural variability, the evaluator's experience, and the instrument used[57].

The satisfaction and GROC scores were consistent with changes in pain and function, and provided a patient-centered perspective. However, their interpretation requires caution, since they

are subjective variables, sensitive to expectations, the patient's current state, and recall bias[58]. In addition, its relationship to objective functional changes is not always consistent.

At the ultrasound level, the included studies show potentially relevant, although not homogeneous, patterns of change. In the study by Góngora-Rodríguez et al.[35] on the shoulder, ultrasound showed sequential improvement: first, hypervascularization decreased, and, subsequently, between 12 and 14 weeks, hypoechogenicity and tendon thickness improved. In the study by Valera et al.[40] on the elbow, tendon thickening appeared to modify more slowly. These findings suggest that hypervascularization could serve as an early signal of response, whereas structural changes may require more time.

These patterns are partially consistent with the literature in the lower limb. In plantar fasciitis, Fernandez-Rodriguez et al.[44] observed a decrease in fascial thickness at 12-24 weeks after PE versus placebo. In patellar tendinopathy, Muñoz-Fernández et al.[48] described ultrasound changes consistent with recovery after PE combined with exercise, including a reduction in tendon thickness from the first weeks. Likewise, Sánchez-Ibáñez et al.[59] observed an early reduction in the Doppler signal at 2 weeks after PE, along with a more organized fibrillar pattern.

Regarding dosing, most of the included protocols used low intensity and prolonged duration, such as EPTE 350 µA for 72-90 seconds, once a week[34-39]. In contrast, one study employed a higher-intensity protocol using ECC Cesmar PPE at 4-6 mA for 3-4 seconds, with three applications[40]. In the included studies, the criteria for choosing the dose were not always explicitly explained. Although both low- and high-intensity protocols were associated with clinical and ultrasound improvements, there are no direct comparisons that allow for the establishment of the superiority of one dosage over another. Consequently, the observed differences seem to depend more on the comparator, co-interventions, and study design than on the dose of PD itself.

Another relevant methodological aspect is the location of the needle. In shoulder studies, most studies describe the application on the tendon in a general way, without specifying precisely whether the target was the midsubstance, the enthesis, the articular face, or the bursal face. In contrast, in lateral epicondylalgia, the localization was more precise, usually directed to the enthesis or to the deep aspect of the tendon of the extensor carpi radialis brevis[38,40]. This lack of precision in the shoulder limits clinical reproducibility and comparison between studies.

From a biological perspective, this information is relevant because tendon tissue can vary by anatomical region. The midsubstance of the supraspinatus, approximately 1 cm proximal to the enthesis, is composed mostly of type I collagen, although in chronic processes it can increase type III collagen[60,61]. The enthesis of the supraspinatus presents a fibrocartilaginous transition to the bone, characterized by the presence of non-mineralized and mineralized fibrocartilage, with relative enrichment of type II collagen, proteoglycans, and progressive mineralization[62]. On the other hand, in the supraspinatus tendon, the distribution of collagens I, II, and III is regionally heterogeneous; according to Buckley et al., type II collagen shows greater expression in joint regions than in the medial bursal region, so it would not be accurate to define the bursal face in a general way only by the predominance of type I collagen[63]. In addition, cell and animal models have shown that intratendinous PD can activate the NLRP3 inflammasome, increase IL-1β release, and subsequently stimulate TGF-β signaling, which, combined with progressive

loading, could favor a remodeling towards a type I collagen-rich matrix[18,19]. In this context, degenerate midsubstance could be considered a biologically plausible target, although this hypothesis requires clinical confirmation.

4.5. Methodological limitations

From an internal validity perspective, the included trials are at moderate risk of bias according to the RoB 2 tool. The main limitations were unclear or limited assessor blinding, baseline imbalances, and reliance on self-reported measures, such as NPRS or VAS, which may overestimate the effect of the intervention. In addition, the inclusion of non-randomized studies may increase the risk of overestimation of effects, so the results should be interpreted with caution.

Another relevant limitation is the difficulty in isolating the specific effect of PE. In many studies, PE was applied in conjunction with therapeutic exercise or other co-interventions, and comparators also received active treatments. In addition, doses, the number of sessions, the duration of interventions, and follow-up periods varied across studies. This heterogeneity reduces the ability to directly compare results and limits the possibility of making firm recommendations on indications, dosage, or superiority over other treatments.

It should also be considered a limitation that the data extraction was carried out by a single researcher, with occasional consultation from a second reviewer, which may introduce a risk of methodological bias. Likewise, the lack of clear standardization in ultrasound measurement, PPT points, the selection of functional scales, and the description of the exact needle location hinders the reproducibility of the results.

4.6. Implications for future studies

Future research should be designed with greater methodological standardization to improve the internal validity and clinical applicability of the results. First, it would be advisable to standardize PD dosimetry, including intensity, duration, number of impacts, number of sessions, weekly frequency, and the device used. Comparator doses should also be equalized, especially in the number and duration of sessions, to reduce the influence of therapeutic care and contextual effects.

Secondly, the tests should more precisely pre-specify the anatomical location of the intervention, indicating whether the needle is directed at the midsubstance, enthesis, articular face, or bursal face, as well as the depth, trajectory, ultrasound location criteria, and structural characteristics of the treated tendon. The inclusion of standardized ultrasound variables, such as Doppler hypervascularization, thickness, hypoechogenicity, and fibrillar pattern, would allow for better comparison of results across studies.

Third, it would be convenient to homogenize the clinical variables using validated, region-specific scales, such as SPADI for the shoulder and PRTEE for the elbow, together with pain measures such as NPRS or VAS and mechanosensitivity variables such as PPT. In addition to statistical significance, clinically relevant changes, proportion of responders achieving MCID, exercise adherence, adverse events, and medium- to long-term follow-up should be reported.

Finally, future studies should pre-register their protocols and statistical analyses, including adjusted analyses such as ANCOVA when baseline differences exist. It would also be useful to describe in detail the type of exercise performed, the progression of the load, adherence, and home compliance, since exercise appears to be a central element of treatment. Together, these improvements would allow us to determine more precisely whether PD provides an additional specific effect to therapeutic exercise and in which clinical profiles it could be most beneficial.

5. Conclusions

In adults with upper limb tendinopathies, PE added to exercise provides early analgesia that often reaches clinical relevance, especially in the shoulder, while the evidence for the elbow remains limited. However, functional improvements, strength, and most objective variables depend mainly on the exercise program, with an additional effect of PD that is more variable and conditioned by comparators and co-interventions; in this context, PPT can increase and ROM improve modestly, while strength/EMG changes are mostly explained by training. At the ultrasound level, a progressive normalization (an initial decrease in hypervascularization, followed by changes in echogenicity and thickness) is observed. However, the heterogeneity of interventions and the risk of moderate bias limit the precision of the results and prevent firm recommendations, so better-designed RCTs, patient-centered measures with a standardized anatomical target, and follow-ups of at least 12 months are required.

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Editorial information

Correspondence

javiergt15@gmail.com

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Not applicable.

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Data Availability Statement

The data supporting the findings of this systematic review are those reported in the primary studies included herein, all of which are cited in the reference list. No new datasets were generated. The data extraction tables and the risk-of-bias assessments (RoB 2 and MINORS) are available upon reasonable request from the corresponding author at lolarouger0@gmail.com.

Conflicts of Interest

L.R. declares no conflicts of interest. J.G.T. declares no conflicts of interest. None of the authors has any financial, commercial, or personal relationship with the manufacturers of the EPTE or EPI Cesmar/ECC Cesmar devices evaluated in this study.

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Appendix A

Complete search strategy:

03.03.25 : "tendinopathy"[MeSH Terms] OR

"tendinopathy" OR "tendinopathies" OR "tendonopathies" OR "tendineous" OR "tendinitis" OR "tendons" OR "tendons" OR "tendinous" OR "tendonitis" OR "tendon s" OR "tendonous" OR "tendon" OR "tendinosis" OR "tendinoses" OR "tendonosis" AND “electrolysis”[MeSH] OR “electrolysis” OR “electrolyses” OR “Percutaneous electrolysis” OR “Percutaneous needle electrolysis” OR “Intratissue percutaneous electrolysis” OR “EPI”

Rob 2 Results:

Arias-Buria et al.[34]

Domain	Signalling question	Response	Comments
Bias arising from the randomization process	1.1 Was the allocation sequence random?	Y	1.1. Random sequence generated with EPIDAT 3.1 1.2. Opaque sealed envelopes used
	1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?	Y	
	1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?	N	1.3. Groups were balanced at baseline (Table 1)
	Risk of bias judgment	**Low**	
Bias due to deviations from intended interventions	2.1 Were participants aware of their assigned intervention during the trial?	Y	2.1. Patients necessarily knew whether they received electrolysis or not 2.2 Clinicians knew the assignments
	2.2 Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?	Y	
	2.3. If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the experimental context?	PN	2.3. No deviations were reported
	2.4 If Y/PY to 2.3: Were these deviations likely to have affected the outcome?	ON	
	2.5. If Y/PY/NI to 2.4: Were these deviations from intended intervention balanced between groups?	ON	
	2.6 Was an appropriate analysis used to estimate the effect of assignment to intervention?	Y	2.6. ITT analysis used
	2.7 If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyse participants in the group to which they were randomized?	ON	
	Risk of bias judgment	**Some concerns**	Some concerns: Participants and clinicians were aware of group assignment, and no blinding was reported

Domain	Signalling question	Response	Comments
Bias due to missing outcome data	3.1 Were data for this outcome available for all, or nearly all, participants randomized?	Y	3.1. All randomized participants completed follow-up
	3.2 If N/PN/NI to 3.1: Is there evidence that the result was not biased by missing outcome data?	ON	
	3.3 If N/PN to 3.2: Could missingness in the outcome depend on its true value?	ON	
	3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?	ON	
	Risk of bias judgment	**Low**	
Bias in the measurement of the outcome	4.1 Was the method of measuring the outcome inappropriate?	N	4.1. NPRS is a valid pain scale
	4.2 Could measurement or ascertainment of the outcome have differed between intervention groups?	N	4.2. Timing and instruments appear consistently across both groups
	4.3 Were outcome assessors aware of the intervention received by study participants?	PN	4.3. It's not specified whether outcome assessors were blinded, but surely because the person who performed the intervention could not be blinded, and neither could the patient
	4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of the intervention received?	ON	
	4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of the intervention received?	ON	
	Risk of bias judgment	**Low**	Some concerns: Outcome assessors were likely unblinded, and outcomes were self-reported, which may have introduced bias
Bias in the selection of the reported result	5.1 Were the data that produced this result analyzed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis?	PY	5.1. This trial is registered with NCT02196948
	5.2 ... multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain?	PN	5.2. No evidence
	5.3 ... multiple eligible analyses of the data?	PN	5.3. Statistical approach appears pre-planned and consistently applied
	Risk of bias judgement	**Low**	Although the trial was registered, no prespecified analysis plan was available to confirm outcome selection
Overall bias	**Risk of bias judgement**	**Some concerns**	Some concerns arise due to lack of blinding, potential performance and detection biases, and absence of a prespecified analysis plan

Góngora-Rodriguez et al.[35]

Domain	Signalling question	Response	Comments
Bias arising from the randomization process	1.1 Was the allocation sequence random?	Y	1.1. Randomization conducted using software 1.2. Randomization process described, but concealment not specified
	1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?	PY	
	1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?	N	1.3. Table 1 shows well-balanced groups
	Risk of bias judgement	**Low**	
Bias due to deviations from intended interventions	2.1.Were participants aware of their assigned intervention during the trial?	Y	2.1. Open-label study 2.2. Treatment delivery not blinded
	2.2.Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?	Y	
	2.3. If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the experimental context?	N	2.3. No deviations reported
	2.4 If Y/PY to 2.3: Were these deviations likely to have affected the outcome?	ON	
	2.5. If Y/PY/NI to 2.4: Were these deviations from intended intervention balanced between groups?	ON	
	2.6 Was an appropriate analysis used to estimate the effect of assignment to intervention?	Y	2.6. ITT clearly stated
	2.7 If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyze participants in the group to which they were randomized?	ON	
	Risk of bias judgment	**Some concerns**	
Bias due to missing outcome data	3.1 Were data for this outcome available for all, or nearly all, participants randomized?	Y	3.1. No dropouts reported
	3.2 If N/PN/NI to 3.1: Is there evidence that result was not biased by missing outcome data?	ON	
	3.3 If N/PN to 3.2: Could missingness in the outcome depend on its true value?	ON	
	3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?	ON	
	Risk of bias judgement	**Low**	

Domain	Signalling question	Response	Comments
Bias in measurement of the outcome	4.1 Was the method of measuring the outcome inappropriate?	N	4.1. NPRS is a validated pain outcome
	4.2 Could measurement or ascertainment of the outcome have differed between intervention groups?	N	4.2. Same method for all participants
	4.3 Were outcome assessors aware of the intervention received by study participants?	IT	4.3. Not described
	4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of the intervention received?	IT	4.4. Possibly, given the subjective nature 4.5. Cannot be ruled out
	4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of intervention received?	IT	
	Risk of bias judgment	**Some concerns**	
Bias in the selection of the reported result	5.1 Were the data that produced this result analyzed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis?	IT	5.1. Not mentioned
	5.2 ... multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain?	IT	5.2. Multiple time points used
	5.3 ... multiple eligible analyses of the data?	IT	5.3. Not excluded
	Risk of bias judgment	**Some concerns**	
Overall bias	**Risk of bias judgment**	**Some concerns**	

Miguel Valtierra et al.[36]

Domain	Signalling question	Response	Comments
Bias arising from the randomization process	1.1 Was the allocation sequence random?	Y	1.1. A computer-generated randomization was used 1.2. Use of sealed opaque envelopes indicates proper allocation concealment
	1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?	Y	
	1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?	N	1.3. No baseline imbalance was observed
	Risk of bias judgment	**Low**	
Bias due to deviations from intended interventions	2.1 Were participants aware of their assigned intervention during the trial?	PY	2.1. Although unaware of the study purpose, participants likely knew which procedures they received 2.2. Blinding was not feasible for therapists due to the nature of the treatments
	2.2 Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?	Y	
	2.3. If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the experimental context?	N	2.3. No deviations reported
	2.4 If Y/PY to 2.3: Were these deviations likely to have affected the outcome?	ON	
	2.5. If Y/PY/NI to 2.4: Were these deviations from intended intervention balanced between groups?	ON	
	2.6 Was an appropriate analysis used to estimate the effect of assignment to intervention?	Y	2.6. Intention-to-treat analysis with appropriate handling of missing data was applied
	2.7 If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyze participants in the group to which they were randomized?	ON	
	Risk of bias judgment	**Some concerns**	Some concerns: Participants and therapists were not blinded, which raises moderate concerns for performance or adherence-related bias
Bias due to missing outcome data	3.1 Were data for this outcome available for all, or nearly all, participants randomized?	PY	3.1. Only 2 dropouts (4%), minimal impact on data completeness
	3.2 If N/PN/NI to 3.1: Is there evidence that the result was not biased by missing outcome data?	ON	
	3.3 If N/PN to 3.2: Could missingness in the outcome depend on its true value?	ON	
	3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?	ON	
	Risk of bias judgment	**Low**	Missing data were computed using regression equations

Domain	Signalling question	Response	Comments
Bias in measurement of the outcome	4.1 Was the method of measuring the outcome inappropriate?	N	4.1. NPRS, DASH, and SPADI are validated and appropriate for this population
	4.2 Could measurement or ascertainment of the outcome have differed between intervention groups?	PN	4.2. Same scale/method used for both groups
	4.3 Were outcome assessors aware of the intervention received by study participants?	N	4.3. Outcome assessor was blinded, limiting measurement bias
	4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of the intervention received?	ON	
	4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of the intervention received?	ON	
	Risk of bias judgment	**Low**	Some concerns: Outcomes like pain were self-reported by unblinded participants, which may be influenced by knowledge of the intervention received
Bias in the selection of the reported result	5.1 Were the data that produced this result analyzed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis?	PY	5.1. The clinical trial was registered at ClinicalTrials.gov (NCT02569281)
	5.2 ... multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain?	PN	5.2. Primary and secondary outcomes were clearly predefined and consistent
	5.3 ... multiple eligible analyses of the data?	PN	5.3. No indication of selective reporting; multiple timepoints and stats reported for all measures
	Risk of bias judgment	**Low**	Some concerns: The article does not clarify whether the statistical analysis plan was finalized before data unblinding, leading to moderate concerns about selective reporting.
Overall bias	**Risk of bias judgment**	**Some concerns**	

Rodriguez-Huguet et al.[37]

Domain	Signalling question	Response	Comments
Bias arising from the randomization process	1.1 Was the allocation sequence random?	Y	1.1. Randomization using software is mentioned, Epidat 4.1 1.2. Opaque envelope concealment supports allocation concealment
	1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?	Y	
	1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?	PN	1.3. The groups were comparable concerning demographic, morphologic, and outcome variables at baseline except for NPRS (p = 0.025)
	Risk of bias judgment	**Low**	If SOME CONCERNS: Baseline imbalance in NPRS suggests potential residual confounding
Bias due to deviations from intended interventions	2.1 Were participants aware of their assigned intervention during the trial?	Y	2.1. Only the assessor was blinded 2.2. The intervention was physically administered and could not be blinded
	2.2 Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?	Y	
	2.3. If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the experimental context?	N	2.3. No deviations reported
	2.4 If Y/PY to 2.3: Were these deviations likely to have affected the outcome?	ON	
	2.5. If Y/PY/NI to 2.4: Were these deviations from intended intervention balanced between groups?	ON	
	2.6 Was an appropriate analysis used to estimate the effect of assignment to intervention?	Y	2.6. Proper mixed-model ANOVA with covariate adjustment suggests appropriate analysis
	2.7 If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyze participants in the group to which they were randomized?	ON	
	Risk of bias judgment	**Some concerns**	Some concerns: The lack of blinding of participants and providers may have led to changes in behavior.
Bias due to missing outcome data	3.1 Were data for this outcome available for all, or nearly all, participants randomized?	Y	3.1. All participants completed the trial
	3.2 If N/PN/NI to 3.1: Is there evidence that the result was not biased by missing outcome data?	ON	
	3.3 If N/PN to 3.2: Could missingness in the outcome depend on its true value?	ON	
	3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?	ON	
	Risk of bias judgement	**Low**	

Domain	Signalling question	Response	Comments
Bias in measurement of the outcome	4.1 Was the method of measuring the outcome inappropriate?	N	4.1. NPRS, ROM and PPT were validated tools
	4.2 Could measurement or ascertainment of the outcome have differed between intervention groups?	N	4.2. Same for both groups, blinded assessor reduces measurement bias between groups
	4.3 Were outcome assessors aware of the intervention received by study participants?	N	4.3. The assessor was blinded
	4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of intervention received?	ON	
	4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of intervention received?	ON	
	Risk of bias judgment	**Low**	Some concerns: Outcomes like pain were self-reported by unblinded participants, which may be influenced by knowledge of the intervention received
Bias in the selection of the reported result	5.1 Were the data that produced this result analyzed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis?	PY	5.1. The trial is registered at ClinicalTrials.gov: identifier, NCT03184181
	5.2 ... multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain?	PN	5.2. Primary outcome clearly identified and justified; secondary outcomes are reasonable
	5.3 ... multiple eligible analyses of the data?	PN	5.3. The statistical plan is consistent with the outcomes listed
	Risk of bias judgment	**Low**	
Overall bias	**Risk of bias judgment**	**Some concerns**	Although the study is well-designed and well-described, key limitations include a lack of blinding of participants and care providers, an imbalance in baseline pain, and unclear reporting of deviations from intended interventions.

Rodriguez-Huguet et al.[38]

Domain	Signalling question	Response	Comments
Bias arising from the randomization process	1.1 Was the allocation sequence random?	Y	1.1. Randomization with Epidat 4.0 1.2. Sealed, sequentially numbered envelopes were used
	1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?	Y	
	1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?	N	1.3. No significant differences in Table 1
	Risk of bias judgment	**Low**	
Bias due to deviations from intended interventions	2.1 Were participants aware of their assigned intervention during the trial?	Y	2.1. Not blinded, outcome assessors were blinded; participants were likely aware. Single-blind 2.2. Clinician was not blinded
	2.2 Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?	Y	
	2.3. If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the experimental context?	N	2.3. Adherence was reported as complete, so no protocol deviations are suggested
	2.4 If Y/PY to 2.3: Were these deviations likely to have affected the outcome?	ON	
	2.5. If Y/PY/NI to 2.4: Were these deviations from intended intervention balanced between groups?	ON	
	2.6 Was an appropriate analysis used to estimate the effect of assignment to intervention?	Y	2.6. Appropriate comparative statistics were used for group differences
	2.7 If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyze participants in the group to which they were randomized?	ON	
	Risk of bias judgment	**Some concerns**	Some concerns: Participants and providers were likely aware of treatment allocation, which may have influenced behavior
Bias due to missing outcome data	3.1 Were data for this outcome available for all, or nearly all, participants randomized?	Y	3.1. All participants completed the study
	3.2 If N/PN/NI to 3.1: Is there evidence that result was not biased by missing outcome data?	ON	
	3.3 If N/PN to 3.2: Could missingness in the outcome depend on its true value?	ON	
	3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?	ON	
	Risk of bias judgment	**Low**	

Domain	Signalling question	Response	Comments
Bias in measurement of the outcome	4.1 Was the method of measuring the outcome inappropriate?	N	4.1. NPRS is a validated pain scale
	4.2 Could measurement or ascertainment of the outcome have differed between intervention groups?	N	4.2. All measurements were conducted by a well-trained physician who was blinded as to which group the patient belonged to
	4.3 Were outcome assessors aware of the intervention received by study participants?	N	4.3. The outcome assessor was blinded
	4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of the intervention received?	ON	
	4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of the intervention received?	ON	
	Risk of bias judgment	**Low**	Some concerns: Outcomes like pain were self-reported by unblinded participants, which may be influenced by knowledge of the intervention received.
Bias in the selection of the reported result	5.1 Were the data that produced this result analyzed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis?	PY	5.1. The trial was registered in the Clinical Trials Registry (reference number NCT03225404)
	5.2 ... multiple eligible outcome measurements (e.g., scales, definitions, time points) within the outcome domain?	N	5.2. NPRS, PPT, ROM, and SF-12 were all reported as planned
	5.3 ... multiple eligible analyses of the data?	N	5.3. One analysis approach used across outcomes.
	Risk of bias judgment	**Low**	No pre-specified statistical analysis plan was available to confirm outcome selection, and timing was decided in advance
Overall bias	**Risk of bias judgment**	**Some concerns**	Although the randomization and data completeness were robust, awareness of treatment assignment and reliance on self-reported outcomes introduce potential performance and detection bias, with unclear prespecification of results.

Rodriguez-Huguet et al.[39]

Domain	Signalling question	Response	Comments
Bias arising from the randomization process	1.1 Was the allocation sequence random?	Y	1.1. Randomization with Epidat 4.0 mentioned 1.2. There is no explicit mention of allocation concealment before assignment, and patients could identify their treatment
	1.2 Was the allocation sequence concealed until participants were enrolled and assigned to interventions?	PY	
	1.3 Did baseline differences between intervention groups suggest a problem with the randomization process?	PN	1.3. There was a statistically significant difference in baseline pain levels between groups. The experimental group reported a lower mean NPRS score (5.96 ± 1.38) compared to the control group (6.92 ± 1.28), with p = 0.015. This suggests a possible issue with the randomization process.
	Risk of bias judgment	**Low**	There was a statistically significant difference in baseline pain levels, suggesting a possible issue with randomization.
Bias due to deviations from intended interventions	2.1 Were participants aware of their assigned intervention during the trial?	Y	2.1. Participants were aware of their treatment 2.2. Different treatments by nature (invasive vs non-invasive) make blinding of providers impossible
	2.2 Were carers and people delivering the interventions aware of participants' assigned intervention during the trial?	Y	
	2.3. If Y/PY/NI to 2.1 or 2.2: Were there deviations from the intended intervention that arose because of the experimental context?	IT	2.3. Self-reported adherence might imply deviations, but no objective data confirms this
	2.4 If Y/PY to 2.3: Were these deviations likely to have affected the outcome?	ON	
	2.5. If Y/PY/NI to 2.4: Were these deviations from intended intervention balanced between groups?	ON	
	2.6 Was an appropriate analysis used to estimate the effect of assignment to intervention?	Y	2.6. Mixed ANOVA and ANCOVA are appropriate for assessing repeated outcomes over time.
	2.7 If N/PN/NI to 2.6: Was there potential for a substantial impact (on the result) of the failure to analyse participants in the group to which they were randomized?	ON	
	Risk of bias judgment	**Some concerns**	Participants and providers were not blinded, and self-reported adherence may have varied
Bias due to missing outcome data	3.1 Were data for this outcome available for all, or nearly all, participants randomized?	Y	3.1. All of the volunteers completed the treatment and the follow-up.
	3.2 If N/PN/NI to 3.1: Is there evidence that the result was not biased by missing outcome data?	ON	
	3.3 If N/PN to 3.2: Could missingness in the outcome depend on its true value?	ON	
	3.4 If Y/PY/NI to 3.3: Is it likely that missingness in the outcome depended on its true value?	ON	
	Risk of bias judgment	**Low**	

Domain	Signalling question	Response	Comments
Bias in the measurement of the outcome	4.1 Was the method of measuring the outcome inappropriate?	N	4.1. All outcome measures are validated and appropriate
	4.2 Could measurement or ascertainment of the outcome have differed between intervention groups?	N	4.2. All assessments were performed by a blinded assessor
	4.3 Were outcome assessors aware of the intervention received by study participants?	N	4.3. All assessments were performed by a blinded assessor
	4.4 If Y/PY/NI to 4.3: Could assessment of the outcome have been influenced by knowledge of intervention received?	ON	
	4.5 If Y/PY/NI to 4.4: Is it likely that assessment of the outcome was influenced by knowledge of intervention received?	ON	
	Risk of bias judgment	**Low**	Some concerns: Primary outcome (pain) was self-reported by unblinded participants, increasing the risk of detection bias.
Bias in the selection of the reported result	5.1 Were the data that produced this result analyzed in accordance with a pre-specified analysis plan that was finalized before unblinded outcome data were available for analysis?	PY	5.1. The trial was registered in the Clinical Trials Registry (reference number NCT04729400)
	5.2 ... multiple eligible outcome measurements (e.g., scales, definitions, time points) within the outcome domain?	PN	5.2. Multiple subscales used; selection bias can't be ruled out
	5.3 ... multiple eligible analyses of the data?	PN	5.3. Possible outcome reporting bias if analytic strategy wasn't fully pre-specified
	Risk of bias judgment	**Low**	Trial was registered, but no confirmation that the analyses followed a pre-specified plan
Overall bias	**Risk of bias judgment**	**Some concerns**	

Resultados MINORS (Methodological Index for Non-Randomized Studies)

Valera et al.[40]

Item	Description	Score (0-2)	Justification	Citation of the article
1	Clearly defined objective	2	The objective is clearly defined in the summary and introduction.	*Objective: To evaluate the clinical and ultrasonographic effectiveness of a multimodal program…*
2	Consecutive Patient Inclusion	2	It is explicitly mentioned that patients were included consecutively.	*Consecutive patients from a clinical setting in Spain were considered eligible… 36 consecutive patients were included in the study*
3	Prospective data collection	2	Measurements were carried out prospectively at different times: before, at discharge, and at three follow-ups	*A one-way repeated measures study over time. Measures were obtained pre-intervention, at treatment discharge and at 6, 26 and 52 weeks follow-up*
4	Endpoints appropriate to the objective of the study	2	Validated clinical, functional and ultrasound measures were used	*The main outcome measures were severity of pain, disability (DASH questionnaire), structural tendon changes (US), hypervascularity…*
5	Blind Evaluation of Objective Endpoints and Double-Blind Evaluation of Subjective Endpoints	2	The ultrasounds were performed by a professional blind to treatment, ensuring an objective evaluation	*The same experienced professional performed all US and Power Doppler examinations and was blinded to the treatment*
6	Adequate follow-up period	1	The follow-up period (up to 52 weeks) was sufficient to evaluate the clinical and ultrasound evolution	*Measurements at 6, 26 and 52 weeks follow-up…*
7	Tracking loss <5%	1	4 patients out of 36 (11.1%) were lost, which exceeds 5%	*All patients participated in the 6-week follow-up, but only 32 (88.8%) were interviewed at 26 and 52 weeks*
8	Prospective sample size calculation	0	No mention of sample size calculation	
9	Adequate control group (comparative studies only)	NA		
10	Contemporary groups (comparative studies only)	NA		
11	Baseline equivalence between groups (comparative studies only)	NA		
12	Adequate statistical analysis (comparative studies only)	NA		
Total		**13/16** **Good methodological quality**		

Appendix B

Table A1. Outcome variables

Variables	Description	Interpretation of results	Quality/Evidence
NPRS= Numeric Pain Rating Scale	Numerical scale 0–10 of perceived pain; Higher scores indicate greater pain intensity	↓ score = improvement in pain. Changes ≥1 are usually noticeable; on shoulder MCID +1.1	High Evidence [64]
VAS= Visual Analog Scale	Line 0–100 mm (or 0–10 cm) on which the patient marks his pain; distance is measured from 0, greater values = more pain	↓ mm/cm=improvement. 13-22mm shifts are usually clinically relevant	High Evidence [65]
DASH= Disability of the Arm, Shoulder and Hand	Questionnaire 0–100 that quantifies symptoms and limitation of the upper limb; High scores reflect greater disability	↓ points = functional improvement. MCID= 10.8	High evidence [66]
SPADI= Shoulder pain and disability index	Shoulder-specific index (pain + disability), 0–100; higher score = worse status	↓ points = improvement	High evidence [67]
PRTEE-S = Patient-Rated Tennis Elbow Evaluation	Epicondylalgia Specific Questionnaire with Pain and Function Domains, 0–100; high values = greater involvement	↓ points = improvement	Moderate evidence [68]
PPT = Pressure Pain Threshold	Pain threshold at pressure with algometer (kPa/N), higher values indicate lower sensitivity to pain	↑ threshold = lower mechanosensitivity (improvement). Interpreting with the same point	High Evidence [69]
ROM= Range of motion	Joint amplitude in degrees (goniometer/inclinometer).	↑ degrees = improved mobility. 4-9° in MS	High Evidence [57]
sEMG= Surface Electromyography	Non-invasive recording of muscle electrical signal during standardized tasks; Its breadth informs recruitment, but it does not directly equate to force	Interpret changes with task/normalization: ↑ target muscle activation or ↓ coactivation, may be desirable	Moderate evidence [70]
GROC=global rating of change	Scale (usually −7 to +7) that captures the change perceived by the patient from the beginning	Positive values = improvement (+5 or more clinical "success"); 0=no change; negative = worse. Expectations Sensitive/Current State	Moderate evidence [58]
Hypervascularization	Intra/peritendinous Doppler signal indicating neovascularization/increased flow	↓grade/area = improvement/normalization. Standardize transducer point, gain, angle, and pressure	Moderate evidence [71]
Hypoechogenicity	Low reflectivity and heterogeneous tendon structure (loss of fibrillar pattern)	↓hypoechogenicity and more organized fibrillar pattern = structural improvement. Control gain, measure long and/or short axis.	Moderate evidence [72]
Tendon thickness	Tendon thickness (mm) measured at a standardized point	↓thickness = improvement, although some thickening may persist. Use same location.	High Evidence [73]

Table A2. Interventions used in the studies

Intervention	Description	Dosage	Quality/Evidence
PE= percutaneous electrolysis	Ultrasound-guided invasive technique: galvanic current through an intratendon needle to modulate tissue and facilitate remodeling	Low intensity (EPTE): 350 µAx 72-90s, 1/week, 3-5 weeks. High intensity (ECC PPE): 4-6 mA x 3-4s, 1/ week, 3-4 weeks.	Low-moderate evidence [1,2]
EE=exercises	Therapeutic program (eccentric/concentric and scapular/forearm control); MS management basis	General: 3x 10-12 reps, 2x/day, 3-5 weeks; Load progression according to tolerance	High Evidence [3]
MT=manual therapy	Joint/soft tissue mobilizations as an adjunct to exercise	Usual: 1 session/week, lasts 3-5 weeks, 5-10 min per technique.	Low Evidence [4]
PNS=peripheral nerve stimulation	Low-frequency percutaneous neuromodulation with needle near the peripheral nerve	10 Hz, 90s, immediately after main intervention; 1/ week, 4 weeks	Low Evidence [5]
TENS=transcutaneous electrical nerve stimulation	Cutaneous electrical stimulation by analgesia	100µs, 150Hz, 20min, 5x week, x4 weeks	Moderate evidence [6]
US=ultrasound	Therapeutic ultrasound for pain modulation and tissue effect	1MHz, 1.5W/^{cm2}, 5min, 5x week, x4 weeks (pulsed mode)	Low Evidence [7]
DN=dry needling	Needle on myofascial points/trigger points/tissue to modulate pain and function	1 session/week for 3-4 weeks	Moderate evidence [8]
VT=vacuum therapy	Local application of negative pressure (cupping) for myofascial/analgesic purposes	100mbar with ON/OFF cycles, 15min, 1 session/ week x4 weeks	Low Evidence [9]
Stretching	Specific musculotendinous stretches as an adjunct to exercise.	3x7 rep, 45s bra, 30s rest, 2x day	Moderate evidence [3]

Note:

- Dose: range of RCTs included, exact protocols may vary between studies

- Quality/Evidence: A Guideline Synthesis of the External Literature