Central poststroke pain (CPSP) is a chronic (≥3 months) neuropathic disorder that can occur after a lesion or disease affecting the central somatosensory system. The pain may be spontaneous, occurring either constantly or intermittently, or evoked in response to external stimuli. It may develop immediately after a stroke, or years later. To date, the largest prospective study, which enrolled 15,754 participants with ischemic stroke from 35 countries, found that 2.7% of patients developed CPSP at 1 year after stroke. Because CPSP case definition is complex, however, its reported prevalence is variable, and dependent on the site of lesion: one study, for instance, found that 25% of patients with brain stem infarcts developed CPSP within 6 months. Individuals with CPSP commonly experience sensory abnormalities, including increased tactile and thermal sensitivities, which impair their quality of life. The underlying mechanisms of CPSP are poorly understood, contributing to challenges in its management.

There are several pharmacological and nonpharmacological therapies available for patients with CPSP; few systematic
reviews have, however, summarized their effectiveness and safety.10-12 The available reviews suffer from important limitations,13 including the following: (1) limited strategies to identify relevant studies, including using few search terms, omitting major literature databases, and excluding non-English language studies, (2) limited safeguards against misleading results, including failure to conduct study selection, risk of bias assessment, and data extraction in duplicate, or (3) focusing on specific types of therapies, that is, either pharmacological or nonpharmacological. As well, none of the reviews evaluated treatment effects on patient-important outcomes beyond pain and adverse events, quantitatively synthesized results using meta-analytic techniques, or used the Grading of Recommendations Assessment, Development, and Evaluation (GRADE) approach to evaluate certainty in the evidence.14

We conducted a systematic review that addresses the limitations of previous reviews to inform evidence-based management of CPSP.

**Methods**

**Standardized Reporting**

We followed the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines for reporting systematic reviews of randomized controlled trials.15

**Protocol Registration**

We registered our protocol with PROSPERO (registration number: CRD42014007189).

**Literature Search**

We searched for relevant studies, in any language, by tailored searches of AMED, CENTRAL, CINAHL, DARE, EMBASE, HealthSTAR, MEDLINE, and PsychINFO, from the inception of each database through December, 2013. An experienced academic librarian developed the search strategy for each electronic database (search strategy for MEDLINE are available in the online-only Data Supplement).

**Eligibility Criteria**

Eligible trials (1) enrolled ≥10 patients with CPSP, (2) randomly assigned them to a therapeutic intervention (pharmacological or nonpharmacological) or a control arm, and (3) collected outcome data ≥14 days after treatment. If a study enrolled a mixed clinical population, we followed a systematic approach (Figure I in the online-only Data Supplement) to determine its eligibility. Ultimately, we included such studies if they met the above criteria, and if (1) the authors provided the results separately for the participants with CPSP; or failing that, (2) at least 80% of a study’s sample comprised participants with CPSP.

We excluded trials that enrolled <10 patients with CPSP because of the limited information that we would gain from such studies, and we excluded trials with <2-week follow-up as patients with chronic pain will have little interest in short-acting treatment effects.16

**Study Selection**

Teams of reviewers worked independently and in duplicate to determine eligibility status of all identified citations, first by screening the titles and abstracts, then by reviewing the full texts of all potential eligible articles. Reviewers resolved any disagreements by discussion, or with the help of an adjudicator. We recruited reviewers proficient in the relevant languages to review the full texts of all non-English studies. At this stage, we measured chance-independent agreement (Φ)—which has several advantages over traditional approaches (eg, κ), including less vulnerability to unequal distributions of results—and interpreted results using established criteria.17 We used an online systematic review software application (DistillerSR, Evidence Partners, Ottawa, Canada; http://systematic-review.net/) to facilitate screening.

**Data Extraction**

Reviewers used a pilot-tested, standardized form to extract information from each eligible study, including participant demographics, treatment details, study methodology, and outcome data as guided by the Initiative on Methods, Measurement, and Pain Assessment in Clinical Trials (IMMPACT). Specifically, we collected outcome data, when available, across the following IMMPACT-recommended patient-important domains: (1) pain, (2) physical functioning, (3) emotional functioning, (4) participant ratings of global improvement and satisfaction with treatment, (5) symptoms and adverse events, (6) participant disposition, (7) role functioning, (8) interpersonal functioning, and (9) sleep and fatigue.18,19 Reviewers resolved any disagreements by discussion, or with the help of an adjudicator.

**Risk of Bias Assessment**

Reviewers assessed risk of bias for each eligible study using a modified Cochrane risk of bias instrument that includes response options of definitely or probably yes—assigned a low risk of bias—or definitely or probably no—assigned a high risk of bias—an approach that we have previously validated.20 Specifically, we evaluated random sequence generation, allocation concealment, blinding of participants and study personnel, and incomplete outcome data.

**Meta-Analyses**

When possible, we conducted meta-analyses using random-effects models that are conservative in that they consider both within- and between-study variability. We used the means and associated SDs of the scores from the longest follow-up time point in each study for our pooled analyses. If a study only reported a median score and a corresponding interquartile range, we assumed the mean score to be equal to the median, and calculated the SD to be equal to the interquartile range divided by 1.35.21 If investigators used ≥1 instrument within a trial to measure the same construct, we chose a single measure as guided by the following prioritization, in descending order of importance: (1) most commonly used instrument, (2) instrument with the strongest evidence of validity, or (3) instrument with the most precise estimation of effect. In our analyses, we treated data from crossover trials as if they were from parallel trials.22

**Facilitating Interpretation of Results**

For studies that provided binary outcome measures, we calculated relative risks and the associated 95% confidence intervals (CIs) to inform relative effectiveness of treatments. For any pooled comparisons that suggested a statistically significant treatment effect, we planned to generate associated measures of absolute effect, that is, risk differences and numbers needed to treat.

When pooling continuous outcomes in which studies used the same instrument, we planned to calculate the weighted mean difference, which maintains the original unit of measurement and represents the average difference between groups. For trials that used different continuous outcome measures that addressed the same construct, we converted all instruments to the most commonly used outcome measure among studies, then pooled results using the weighted mean difference.23 For any pooled comparisons that suggested a statistically significant treatment effect, we planned to calculate the proportion of participants who benefited, that is, demonstrated improvement greater than or equal to the minimally important difference in each trial, then aggregate the results across all studies, and generate measures of relative and absolute treatment effects. For studies that reported effects of therapies on reducing pain, we also planned to use thresholds of ≥20%, ≥30%, and ≥50% improvement from baseline to optimize interpretation of treatment effects.26
Assessment of Heterogeneity and Subgroup Analyses
For each pooled analysis, we examined heterogeneity using both the $\chi^2$ test and the $I^2$ statistic, which represents the percentage of variability that is because of true differences between studies (heterogeneity) rather than sampling error (chance).23

We generated six a priori hypotheses to explain variability between studies: (1) interventions will show larger effects in trials that excluded participants in receipt of disability benefits or involved in litigation versus trials that included such participants,24 (2) interventions will show smaller effects among trials with longer follow-up times versus trials with shorter follow-up times, (3) interventions will show smaller effects among trials enrolling participants with psychiatric comorbidities versus trials that do not, (4) interventions will show smaller effects among trials enrolling participants with longer duration of CPSP before therapy versus trials that enroll participants with shorter duration of CPSP, (5) interventions will show larger effects in trials testing them at higher doses versus trials testing them at lower doses, and (6) interventions will show larger effects in trials with greater risk of bias versus trials with lower risk of bias. We planned to conduct this last subgroup analysis on a risk of bias component-by-component basis, only if there was considerable variability within the risk of bias component. We planned to conduct tests of interaction to establish if the effect size from the subgroups differed significantly from each other.23 We did not conduct subgroup analyses if there were <3 studies in a given subgroup.

Certainty in Treatment Estimates
We used the GRADE approach to categorize certainty in effect estimates for all reported outcomes as high, moderate, low, or very low.4 Using this approach, randomized controlled trials begin as high certainty but can be rated down because of (1) risk of bias,26 (2) inconsistency,27 (3) indirectness,28 (4) imprecision,29 and (5) publication bias.30 For any pooled comparisons that suggested a statistically significant treatment effect, we planned to use recent approaches to address missing participant data for binary and continuous outcomes.31–33 When plausible worst-case scenarios reversed treatment effects, we planned to rate down for risk of bias. We presented our results in GRADE evidence profiles.34–36

Analytic Software
We conducted meta-analyses using Review Manager (RevMan), version 5.3 (Copenhagen: The Nordic Cochrane Center, The Cochrane Collaboration, 2014). We rated our certainty in effect estimates and created GRADE evidence profiles using GRADEproGDT (http://www.guidelinedevelopment.org/).

Results
We identified 5015 unique records, of which we retrieved 324 in full text (Figure 1). After reviewing the full texts, we deemed 8 English language studies that enrolled 459 patients with CPSP eligible for our review (Table 1).37–44 There was almost perfect agreement ($P=0.82$) between reviewers at the full-text review stage. All trials evaluated treatment effects on pain, and none reported effects on physical functioning, or interpersonal functioning (Figure 2). The longest follow-up among eligible studies ranged from 2 to 12 weeks. No study reported the number of participants that were receiving disability benefits or were involved in litigation during the study period. One study reported no difference in the number of participants (in the pregabalin and placebo groups) who presented with psychiatric comorbidities, specifically depression and insomnia.41 Figure 3 portrays the risk of bias assessment.

Effects of Pharmacotherapy on Patient-Important Outcomes

Anticonvulsants
Very low certainty evidence from 4 trials (Table 2), which enrolled a total of 307 participants,37,40–42 showed that, when compared with placebo, anticonvulsants did not significantly reduce pain intensity (weighted mean difference on an 11-step scale, $-0.75; 95\%$ CI, $-1.71$ to $0.21; P=69\%$; Figure 4A), or increase adverse events (relative risk, 1.61; 95\% CI, 0.90–2.88; $P=80\%$; Figure 4B). Because of the small number of studies in each meta-analysis, and in line with our a priori criteria, we did not conduct our prespecified subgroup analyses to explain inconsistency in results.

Low certainty evidence from 3 studies evaluated the effects of anticonvulsants on emotional functioning, most commonly in context of managing depression.37,41,42 None reported a significant effect; variability in the presentation of the data precluded statistical pooling. Low certainty evidence from 1 study found that pregabalin (versus placebo) did not affect patient-reported global improvement, but did improve sleep (difference between least square means, $-4.2; 95\%$ CI, $-8.4$ to $0.0; P=0.049$; Table 2).

Tricyclic Antidepressants
Low certainty evidence (Table I in the online-only Data Supplement) from 1 trial of 15 participants reported that, when compared with placebo, amitriptyline significantly reduced pain intensity during the last (fourth) week of treatment, although our reanalysis of the data did not find a significant effect.37 The authors also reported that amitriptyline did not affect depressive symptoms, and was associated with significantly more adverse events than placebo (relative risk, 2.00; 95\% CI, 1.15–3.49).

Opioid Antagonists
Low certainty evidence (Table II in the online-only Data Supplement) from 1 trial of 20 participants reported that naloxone had no effect on pain when compared with placebo.38
<table>
<thead>
<tr>
<th>Author</th>
<th>Country of Study</th>
<th>Study Design</th>
<th>Treatments</th>
<th>Frequency and Duration of Treatment</th>
<th>No. of Total CPSP Randomized</th>
<th>Age of CPSP Participants</th>
<th>Sex of CPSP Participants</th>
<th>Duration of CPSP</th>
<th>Participant Disposition/Notes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Leijon et al&lt;sup&gt;37&lt;/sup&gt;</td>
<td>Sweden</td>
<td>Crossover</td>
<td>Amitriptyline (75 mg, final dose) Carbamazepine (800 mg, final dose) Placebo</td>
<td>4 wk (7-d washout)</td>
<td>15</td>
<td>Mean, 66 yr; Range, 53–74</td>
<td>Female, 3; Male, 12</td>
<td>Mean, 54 mo; Range, 11–154</td>
<td>One participant discontinued intervention because of interaction with existing medication</td>
</tr>
<tr>
<td>Bainton et al&lt;sup&gt;38&lt;/sup&gt;</td>
<td>United Kingdom</td>
<td>Crossover</td>
<td>Naloxone (8 mg) Placebo</td>
<td>One-time treatment (2- to 3-wk washout)</td>
<td>20</td>
<td>Mean, 61.1 yr; Range, 45–74</td>
<td>Female, 13; Male, 7</td>
<td>Mean, 7.5 yr; Range, 1–20</td>
<td>Three participants withdrew because of adverse events</td>
</tr>
<tr>
<td>Jiang et al&lt;sup&gt;39&lt;/sup&gt;</td>
<td>China</td>
<td>Parallel</td>
<td>Electroacupuncture (30 min)</td>
<td>Frequency</td>
<td>60</td>
<td>NR</td>
<td>Electroacupuncture: Female, 10; Male, 20 Carbamazepine</td>
<td>Electroacupuncture: Mean, 3.6 mo; Control: Mean, 3.8 mo</td>
<td>NR</td>
</tr>
<tr>
<td>Vestergaard et al&lt;sup&gt;40&lt;/sup&gt;</td>
<td>Denmark</td>
<td>Crossover</td>
<td>Lamotrigine (200 mg, final dose) Placebo</td>
<td>8 wk (2-wk washout)</td>
<td>30</td>
<td>Median, 59 yr; Range, 37–77</td>
<td>Female, 12; Male, 18</td>
<td>Median, 2 yr; Range, 0.3–12</td>
<td>Three participants withdrew because of adverse events One participant did not complete the first treatment period, but continued the study in the second treatment period Four participants withdrew because of lack of efficacy Three participants withdrew because of protocol violations</td>
</tr>
<tr>
<td>Kim et al&lt;sup&gt;41&lt;/sup&gt;</td>
<td>Asia Pacific region</td>
<td>Parallel</td>
<td>Pregabalin (600 mg/d, final maximum dose) Placebo</td>
<td>12 wk (4-wk dose adjustment, 8-wk maintenance)</td>
<td>220</td>
<td>Pregabalin: Mean, 59.4 yr; SD, 9.8; Placebo: Mean, 57.1; SD, 10.2</td>
<td>Pregabalin: Female, 43; Male, 67; Placebo: Female, 39; Male, 70</td>
<td>Pregabalin: Mean, 2.2 yr; Range, 0.1–17.7; Placebo: Mean, 2.5, Range, 0.2–14.1</td>
<td>One participant did not receive intervention Nine participants withdrew because of reasons related to the study drug Twenty-seven participants withdrew because of reasons not related to the study drug</td>
</tr>
<tr>
<td>Jungehulsing et al&lt;sup&gt;42&lt;/sup&gt;</td>
<td>Germany</td>
<td>Crossover</td>
<td>Levetiracetam (3000 mg/d, maximum dose) Placebo</td>
<td>8 wk (2-wk washout)</td>
<td>42</td>
<td>Median, 61.5 yr; Range, 40–76</td>
<td>Female, 16; Male, 26</td>
<td>Median, 4 yr; Range, 0.4–11</td>
<td>Three participants withdrew because of protocol violations Three participants withdrew consent Three participants withdrew because of adverse events</td>
</tr>
<tr>
<td>Hosomi et al&lt;sup&gt;43&lt;/sup&gt;</td>
<td>Japan</td>
<td>Crossover</td>
<td>Repetitive transcranial magnetic stimulation (5 Hz) Stem stimulation</td>
<td>Once daily, 10 d (at least 17-d washout)</td>
<td>NR (see notes)</td>
<td>NR</td>
<td>NR</td>
<td>NR</td>
<td>Seventy participants randomized (unclear how many with CPSP) Two participants did not receive intervention (unclear how many with CPSP) Four participants did not provide data (unclear how many with CPSP) Three participants discontinued intervention (unclear how many with CPSP) Sixty-four participants included in authors’ intention-to-treat analysis set; 52 with CPSP</td>
</tr>
<tr>
<td>Cho et al&lt;sup&gt;44&lt;/sup&gt;</td>
<td>Republic of Korea</td>
<td>Parallel</td>
<td>Acupuncture (0.05 mL) Saline Acupuncture</td>
<td>Twice weekly, 3 wk</td>
<td>20</td>
<td>NR</td>
<td>NR</td>
<td>NR</td>
<td>One participant withdrew because of adverse event Three participants discharged/left hospital before follow-up</td>
</tr>
</tbody>
</table>

CPSP indicates central poststroke pain; and NR, not reported.
Effects of Nonpharmacotherapy on Patient-Important Outcomes

**Repetitive Transcranial Magnetic Stimulation**

Low certainty evidence (Table III in the online-only Data Supplement) from 1 trial (n=52) of repetitive transcranial magnetic stimulation versus sham stimulation found no significant differences in adverse events, depressive symptoms, or patient-reported global improvement.43

**Acupuncture**

Low certainty evidence (Table IV in the online-only Data Supplement) from 1 study (n=20) reported a significant effect of acupuncture over saline acupuncture for pain reduction (median 100-point Visual Analogue Scale score decrease: 36.50 versus 11.50; \(P=0.009\)).44 Very low certainty evidence (Table V in the online-only Data Supplement) from another study (n=60) found no significant effect of electroacupuncture versus carbamazepine on a composite measure of joint pain, dysfunction, and tenderness.39

**Discussion**

Our systematic review found low or very low certainty evidence suggesting that anticonvulsants, tricyclic antidepressants, opioid antagonists, and electroacupuncture have no effect on reducing pain associated with CPSP. Low certainty evidence suggests that acupuncture may reduce pain, anticonvulsants may improve sleep, repetitive transcranial magnetic stimulation has no effect on depressive symptoms or patient-reported global improvement, and tricyclic antidepressants do not improve depressive symptoms and produce significantly more side effects.

**Strengths and Limitations**

Our review has several strengths. First, we reviewed all non-pharmacological and pharmacological therapies for managing patients with CPSP. Second, we explored a wider range of literature databases than previous reviews, and searched for eligible studies in all languages. Third, teams of reviewers, who worked independently and in duplicate, made all subjective decisions, including study selection, risk of bias assessment, and data extraction. Fourth, we followed a systematic approach, which included working with expert clinicians and contacting study authors, to assess the eligibility of studies that enrolled mixed clinical populations. Fifth, we collected all patient-important outcomes across IMMPACT-recommended core outcome domains. Finally, we used the GRADE approach to evaluate our certainty in the evidence, and presented our findings with GRADE evidence profiles. Our findings, however, are limited by shortcomings of the primary studies that were eligible for our review. This led to our ratings of low or very low certainty for all treatment effects.

**Implications**

Our findings are inconsistent with clinical practice guidelines by 3 major professional groups—the International Association...
for the Study of Pain Neuropathic Pain Special Interest Group, the European Federation of Neurological Societies, and the Canadian Pain Society—all of whom recommend tricyclic antidepressants as first-line therapy for managing patients with CPSP.45–47 These recommendations are because of 1 trial of 15 participants that concluded that amitriptyline significantly reduced pain intensity versus placebo after 4 weeks of treatment.37 Follow-up scores on the 10-step scale for pain, Table 2.

<table>
<thead>
<tr>
<th>Quality Assessment</th>
<th>No. of Studies</th>
<th>Study Design</th>
<th>Risk of Bias</th>
<th>Inconsistency</th>
<th>Indirectness</th>
<th>Imprecision</th>
<th>Publication Bias</th>
<th>Anticonvulsants</th>
<th>Placebo</th>
<th>Effect</th>
<th>Relative (95% CI)</th>
<th>Absolute (95% CI)</th>
<th>Certainty</th>
<th>Importance</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pain intensity (follow-up: range, 4–12 wk; assessed with: Visual Analog Scale 0 [no pain] to 10 [worst pain])</td>
<td>4</td>
<td>Randomized trials</td>
<td>Serious*</td>
<td>Serious†</td>
<td>Not serious</td>
<td>Serious‡</td>
<td>Undetected§</td>
<td>184</td>
<td>184</td>
<td>Not significant</td>
<td>0.000</td>
<td>Very low</td>
<td>Important</td>
<td></td>
</tr>
<tr>
<td>Any adverse event (follow-up: range, 4–12 wk)</td>
<td>3</td>
<td>Randomized trials</td>
<td>Serious*</td>
<td>Serious</td>
<td></td>
<td>Not serious</td>
<td>Serious¶</td>
<td>Undetected§</td>
<td>154</td>
<td>154</td>
<td>Not significant</td>
<td>0.000</td>
<td>Very low</td>
<td>Important</td>
</tr>
<tr>
<td>Depression (follow-up: range, 4–12 wk; assessed with: Various instruments)</td>
<td>3</td>
<td>Randomized trials</td>
<td>Serious*</td>
<td>Not serious#</td>
<td>Not serious</td>
<td>Serious‡</td>
<td>Undetected§</td>
<td>145</td>
<td>145</td>
<td>No study found a significant reduction in depression symptoms</td>
<td>0.000</td>
<td>Low</td>
<td>Important</td>
<td></td>
</tr>
<tr>
<td>Patient-reported global improvement (follow-up: range, 12 wk; assessed with: Patient Global Impression of Change; 1 [very much improved] to 7 [very much worse])</td>
<td>1</td>
<td>Randomized trial</td>
<td>Serious**</td>
<td>Not serious</td>
<td>Not serious</td>
<td>Serious‡</td>
<td>Undetected§</td>
<td>110</td>
<td>109</td>
<td>Not significant</td>
<td>0.000</td>
<td>Low</td>
<td>Important</td>
<td></td>
</tr>
<tr>
<td>Sleep (follow-up: 12 wk; assessed with: Sleep Problems Index, Medical Outcomes Study Sleep Scale; 0 [no problems] to 100 [most severe problems])</td>
<td>1</td>
<td>Randomized trial</td>
<td>Serious**</td>
<td>Not serious</td>
<td>Not serious</td>
<td>Serious‡</td>
<td>Undetected§</td>
<td>110</td>
<td>109</td>
<td>Study found that pregabalin improved sleep versus placebo; difference between least-squares mean, −4.2; 95% CI, −8.4 to 0.0; P=0.049</td>
<td>0.000</td>
<td>Low</td>
<td>Important</td>
<td></td>
</tr>
</tbody>
</table>

CI indicates confidence interval; and GRADE, Grading of Recommendations Assessment, Development, and Evaluation.

*Serious because of selection bias (unclear or inadequate allocation concealment), detection bias (unclear blinding of data analysts), and attrition bias (incomplete outcome reporting).
†Serious because of statistical heterogeneity (I²=69%; P=0.02).
‡Serious because of small sample size (<400 participants).
§Insufficient number of studies to detect publication bias.
¶Serious because of statistical heterogeneity (I²=80%; P=0.007).
#Serious because of small number of events (<325).
Not serious because of all studies showing no significant treatment effect.
**Serious because of detection bias (unclear blinding of data analysts).

Figure 4. A. Effects of anticonvulsants versus placebo on pain intensity (11-point scale, higher score is worse). B. Effects of anticonvulsants versus placebo on any adverse events. CI indicates confidence interval.
however, were very similar for amitriptyline (mean, 4.2; SD, 1.6) and placebo (mean, 5.3; SD, 2.0), and our reanalysis of the data found no significant effect (*P* = 0.11).

The European Federation of Neurological Societies and Canadian Pain Society also recommend anticonvulsants as first-line pharmacological treatment for CPSP,\(^4,5,6\) our review found no evidence that they reduce pain. The European Federation of Neurological Societies, however, formulated its recommendations on the success of anticonvulsants in patients with other chronic neuropathic pain conditions. This assumes that treatment responses are consistent across chronic neuropathic pain conditions. A recent systematic review provides some support for this assumption,\(^4,8\) and we are further validating this hypothesis in an ongoing network meta-analysis of all therapies for all chronic neuropathic pain conditions.\(^4,8\)

In the face of only low, or in most cases very low, certainty evidence, with initial evidence providing minimal or no support for benefit, management of CPSP remains extremely challenging. Investigators should mount large, multicenter, randomized trials using standardized instruments with known, satisfactory measurement properties to assess patient-important outcomes, including function. Such trials should include longer observation, and should implement strategies to reduce risk of bias, including generating the randomization sequence, concealing treatment allocation, and implementing strategies to minimize loss to follow-up. Given results thus far, such trials should evaluate both existing and innovative therapeutic options.

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### Disclosures

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### References


Management of Central Poststroke Pain: Systematic Review of Randomized Controlled Trials


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[mp=title, abstract, original title, name of substance word, subject heading word, keyword heading word, protocol supplementary concept, rare disease supplementary concept, unique identifier]
Search Strategy:
--------------------------------------------------------------------------------
1  peripheral nervous system diseases/ or brachial plexus neuropathies/ or brachial plexus neuritis/ or complex regional pain syndromes/ or causalgia/ or reflex sympathetic dystrophy/ or diabetic neuropathies/ or giant axonal neuropathy/ or guillain-barre syndrome/ or mononeuropathies/ or femoral neuropathy/ or median neuropathy/ or peroneal neuropathies/ or radial neuropathy/ or sciatic neuropathy/ or sciatica/ or tibial neuropathy/ or tarsal tunnel syndrome/ or ulnar neuropathies/ or cubital tunnel syndrome/ or ulnar nerve compression syndromes/ or nerve compression syndromes/ or carpal tunnel syndrome/ or piriformis muscle syndrome/ or pudendal neuralgia/ or thoracic outlet syndrome/ or cervical rib syndrome/ or neuralgia/ or neuralgia, postherpetic/ or neuritis/ or polyneuropathies/ or alcoholic neuropathy/ or "hereditary sensory and motor neuropathy"/ or alstrom syndrome/ or charcot-marie-tooth disease/ or refsum disease/ or spastic paraplegia, hereditary/ or poems syndrome/ or polyradiculoneuropathy/ or polyradiculoneuropathy, chronic inflammatory demyelinating/ or polyradiculopathy/ or radiculopathy/ (93826)
2  exp central nervous system disease/ (1153336)
3  "autoimmune diseases of the nervous system"/ or myelitis, transverse/ or neuromyelitis optica/ or polyradiculoneuropathy/ or guillain-barre syndrome/ or "hereditary sensory and autonomic neuropathies"/ or polyradiculoneuropathy, chronic inflammatory demyelinating/ (10906)
4  Fabry Disease/ (2616)
5  Angiokeratoma/ (592)
6  Paraneoplastic Polyneuropathy/ (209)
7  Glossalgia/ (247)
8  Burning Mouth Syndrome/ (754)
9  Syringomyelia/ (3161)
10  Paroxysmal Hemicrania/ (86)
11  Trigeminal Autonomic Cephalalgias/ (123)
12  Phantom Limb/ (1529)
13  Thalamic Diseases/ (1094)
14  neuropath*.mp. (104242)
15  mononeuropath*.mp. (1490)
16  polyneuropath*.mp. (13220)
17  polyradiculoneuropath*.mp. (5034)
18  (Guillian adj Barre).mp. (89)
19  (Guillain adj Barre).mp. (7164)
20  (lewis adj sumner).mp. (51)
21  (charcot adj marie adj tooth).mp. (3762)
22  HMSN.mp. (419)
23  Peroneal muscular atrophy.mp. (160)
24  Guyon.ti,ab. (137)
25  Pronator teres.mp. (262)
26  (Struther$ adj ligament).mp. (18)
27  Wartenberg$.mp. (120)
28  Angiokeratoma.mp. (882)
Obesity and Pain: A Systematic Review and Meta-Analysis

1. Introduction
- Obesity is associated with a variety of chronic pain conditions.
- The relationship between obesity and pain is complex and multifactorial.

2. Methods
- Systematic search of databases (PubMed, Embase, Cochrane Library) for studies comparing obese and non-obese individuals on pain outcomes.
- Eligibility criteria:
  - Human studies
  - Comparative designs
  - Published in English

3. Results
- A total of 20 studies were included in the meta-analysis.
- Findings are consistent across different pain domains:
  - Nociophobia
  - Hypersensitivity
  - Pain intensity

4. Discussion
- Obesity increases pain sensitivity and symptom reporting.
- Possible mechanisms:
  - Inflammation
  - Autonomic nervous system changes

5. Conclusion
- Obesity is a significant contributor to chronic pain.
- Public health initiatives targeting weight management may reduce pain burden.

6. References
- A comprehensive list of studies included in the meta-analysis.

7. Appendix
- Detailed methodology and data analysis procedures.

8. Funding
- Support from National Institutes of Health (NIH) grant number T32AR007315.

9. Acknowledgments
- Thanks to the anonymous reviewers for their valuable feedback.

10. Appendix
- Additional tables and figures for detailed results.

11. Appendix
- Supplementary materials for further exploration of the topic.

12. Appendix
- Contact information for corresponding author.

13. Appendix
- Declaration of interests.

14. Appendix
- Ethical approval.

15. Appendix
- Data availability.

16. Appendix
- Conflict of interest.

17. Appendix
- Supplemental material.

18. Appendix
- Data. (5466924)
randomized.ab. (306656)
placebo.ab. (164174)
drug therapy.fs. (1769994)
randomly.ab. (216948)
trial.ab. (322792)
groups.ab. (1377407)
or/119-126 (3435195)
exp animals/ not humans.sh. (4063721)
127 not 128 (2944807)
118 and 129 (38503)
limit 130 to "therapy (maximizes sensitivity)" (32458)
limit 131 to "review articles" (6827)
131 not 132 (25631)
Transcranial Magnetic Stimulation/ (6714)
rtms.mp. (2389)
magnetics/tu (811)
134 or 135 or 136 (8161)
pain.mp. (481994)
137 and 138 (544)
133 or 139 (26089)
(Dejerine adj Roussy).mp. (37)
CPS.P.ti,ab. (157)
(critical adj3 pain).mp. (2340)
((neurogen* or neuropath*) adj pain).mp. (12164)
((post-stroke or thalamic) and (pain or hyperpathia or allodynia or hyperalgesia or neuralgia)).mp. (1899)
Lateral Medullary Syndrome/ or wallenberg*.mp. (679)
or/141-146 (16162)
140 and 147 (2385)
### Supplemental Table I: GRADE Evidence Profile: Amitriptyline vs. Placebo

<table>
<thead>
<tr>
<th>Quality assessment</th>
<th>№ of patients</th>
<th>Effect</th>
<th>Certainty</th>
<th>Importance</th>
</tr>
</thead>
<tbody>
<tr>
<td>№ of studies</td>
<td>Study design</td>
<td>Risk of bias</td>
<td>Inconsistency</td>
<td>Indirectness</td>
</tr>
<tr>
<td>1</td>
<td>randomised trial</td>
<td>serious ½</td>
<td>not serious</td>
<td>not serious</td>
</tr>
</tbody>
</table>

**Pain intensity** (follow up: 4 weeks; assessed with: Verbal scale; 10-step (larger value is worse pain))

|№ of studies | Study design | Risk of bias | Inconsistency | Indirectness | Imprecision | Publication Bias | Amitriptyline | Placebo | Relative (95% CI) | Absolute (95% CI) |
| 1 | randomised trial | serious ½ | not serious | not serious | serious ½ | undetected ½ | 15 | 15 | RR 2.00 (1.15 to 3.49) | 47 more per 100 (from 7 more to 100 more) | ![☆☆☆☆☆](#) | IMPORTANT |

**Any adverse event** (follow up: 4 weeks)

|№ of studies | Study design | Risk of bias | Inconsistency | Indirectness | Imprecision | Publication Bias | Amitriptyline | Placebo | Relative (95% CI) | Absolute (95% CI) |
| 1 | randomised trial | serious ½ | not serious | not serious | serious ½ | undetected ½ | 15 | 15 | RR 2.00 (1.15 to 3.49) | 47 more per 100 (from 7 more to 100 more) | ![☆☆☆☆☆](#) | IMPORTANT |

**Depression** (follow up: mean 4 weeks; assessed with: Comprehensive Psychopathological Rating Scale; 0 (no symptoms) to 30 (most severe symptoms))

|№ of studies | Study design | Risk of bias | Inconsistency | Indirectness | Imprecision | Publication Bias | Amitriptyline | Placebo | Relative (95% CI) | Absolute (95% CI) |
| 1 | randomised trial | serious ½ | not serious | not serious | serious ½ | undetected ½ | 15 | 15 | Not significant | ![☆☆☆☆☆](#) | IMPORTANT |

**RR** – relative risk

1. Serious due to selection bias (unclear and/or inadequate allocation concealment), and detection bias (unclear blinding of data analysts)
2. Serious due to small sample size (<400 participants)
3. Insufficient number of studies to detect publication bias
4. Serious due to small number of events (<325)
### Supplemental Table II: GRADE Evidence Profile: Naloxone vs. Placebo

<table>
<thead>
<tr>
<th>Quality assessment</th>
<th>№ of patients</th>
<th>Effect</th>
<th>Certainty</th>
<th>Importance</th>
</tr>
</thead>
<tbody>
<tr>
<td>№ of studies</td>
<td>Study design</td>
<td>Risk of bias</td>
<td>Inconsistency</td>
<td>Indirectness</td>
</tr>
<tr>
<td>1</td>
<td>randomised trial</td>
<td>serious ↓</td>
<td>not serious</td>
<td>not serious</td>
</tr>
</tbody>
</table>

Pain intensity (follow up: 2 weeks; assessed with: Visual Analogue Scale; 100 mm (larger value indicates worse pain))

1. Serious due to selection bias (unclear and/or inadequate allocation concealment), and detection bias (unclear blinding of data analysts)
2. Serious due to small sample size (<400 participants)
3. Insufficient number of studies to detect publication bias
Supplemental Table III: GRADE Evidence Profile: Repetitive transcranial magnetic stimulation vs. Sham stimulation

<table>
<thead>
<tr>
<th>№ of studies</th>
<th>Study design</th>
<th>Risk of bias</th>
<th>Inconsistency</th>
<th>Indirectness</th>
<th>Imprecision</th>
<th>Publication Bias</th>
<th>№ of patients</th>
<th>Effect</th>
<th>Certainty</th>
<th>Importance</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Repetitive transcranial magnetic stimulation</td>
<td>Sham stimulation</td>
<td>Relative (95% CI)</td>
<td>Absolute (95% CI)</td>
<td></td>
</tr>
</tbody>
</table>

Any adverse event (follow up: 29 days)

| 1            | randomised trial | serious § | not serious | not serious | serious ¶ | undetected ¶| 52           | 52                | Not significant | ΘΘΟΟ LOW | IMPORTANT |

Depression (follow up: 29 days; assessed with: Beck Depression Inventory; 0 (no symptoms) to 63 (most severe symptoms))

| 1            | randomised trial | serious § | not serious | not serious | serious ¶ | undetected ¶| 52           | 52                | Not significant | ΘΘΟΟ LOW | IMPORTANT |

Patient-reported global improvement (follow up: 29 days; assessed with: Patient Global Impression of Change; 1 (very much improved) to 7 (very much worse))

| 1            | randomised trial | serious § | not serious | not serious | serious ¶ | undetected ¶| 52           | 52                | Not significant | ΘΘΟΟ LOW | IMPORTANT |

1. Serious due to detection bias (unclear blinding of data analysts), and attrition bias (incomplete outcome reporting)
2. Serious due to small sample size (<400 participants)
3. Insufficient evidence to detect publication bias
# Supplemental Table IV: GRADE Evidence Profile: Apipuncture vs. Saline acupuncture

<table>
<thead>
<tr>
<th>Quality assessment</th>
<th>No of patients</th>
<th>Effect</th>
<th>Certainty</th>
<th>Importance</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Apipuncture</td>
<td>Saline acupuncture</td>
<td>Relative (95% CI)</td>
</tr>
<tr>
<td>№ of studies</td>
<td>Study design</td>
<td>Risk of bias</td>
<td>Inconsistency</td>
<td>Indirectness</td>
</tr>
<tr>
<td>1</td>
<td>randomised trial</td>
<td>serious ¹</td>
<td>not serious</td>
<td>not serious</td>
</tr>
<tr>
<td>10</td>
<td>10</td>
<td>Study found that apipuncture decreased pain versus saline acupuncture, p=0.009</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

### No. of patients: Quality assessment

1. Serious due to selection bias (unclear and/or inadequate allocation concealment), performance bias (unclear blinding of health care providers, data collectors, and outcome assessors), and detection bias (unclear blinding of data analysts)
2. Serious due to small sample size (<400 participants)
3. Insufficient number of studies to detect publication bias

### No. of studies: Quality assessment

Pain intensity (follow up: 3 weeks; assessed with: Visual Analogue Scale; 0 (no pain) to 100 (worst pain))
## Supplemental Table V: GRADE Evidence Profile: Electroacupuncture vs. Carbamazepine

<table>
<thead>
<tr>
<th>Quality assessment</th>
<th>№ of patients</th>
<th>Effect</th>
<th>Certainty</th>
<th>Importance</th>
</tr>
</thead>
<tbody>
<tr>
<td>№ of studies</td>
<td>№ of studies</td>
<td>Electroacupuncture</td>
<td>Carbamazepine</td>
<td>Relative (95% CI)</td>
</tr>
<tr>
<td>Study design</td>
<td>Risk of bias</td>
<td>Inconsistency</td>
<td>Indirectness</td>
<td>Imprecision</td>
</tr>
<tr>
<td>Randomised trial</td>
<td>Very serious</td>
<td>Not serious</td>
<td>Not serious</td>
<td>Serious</td>
</tr>
</tbody>
</table>

Composite of joint pain, dysfunction, and tenderness (follow up: 30 days; assessed with: Total symptom score; 0 (no symptoms) to 30 (most severe symptoms))

1. Very serious due to selection bias (unclear and/or inadequate allocation concealment), performance bias (unclear blinding of health care providers, data collectors, detection bias (unclear blinding of outcome assessors, and data analysts), and outcome assessors), and detection bias (unclear blinding of data analysts)
2. Serious due to small sample size (<400 participants)
3. Insufficient number of studies to undetected publication bias
Supplemental Figure I: Full-text screening algorithm

Study enrolls mixed clinical population

Clear that study enrolls participants with CPSP
- Results reported separately for participants with CPSP
  - Include study
- Results not reported separately for participants with CPSP
  - Contact authors to gather results for CPSP participants
    - Authors provide results
      - Study enrolls at least 80% participants with CPSP
        - Include study
      - Study enrolls less than 80% participants with CPSP
        - Exclude study
    - Authors do not provide results
      - Study eligibility determined (include/exclude)

Unclear whether study enrolls participants with CPSP
- Expert clinicians attempt to determine eligibility
  - Study eligibility unclear
    - Contact authors to gather information to determine study eligibility
      - Authors provide information
        - Study eligibility determined (include/exclude)
      - Authors do not provide information
        - Exclude study
SUPPLEMENTAL MATERIAL

Online Supplement for manuscript entitled:

NIHSS Item Profiles as Predictor of Patient Outcome: an External Validation on SITS-MOST data

Authors:


On behalf of the SITS-MOST steering committee *

Supplemental Tables: I - III
Supplemental Figures and Figure Legends: I - IV
Supplemental Data
### SUPPLEMENTAL TABLES

#### Supplementary Table I: Baseline characteristics of the cohort, overall and by 24-hour-NIHSS item profiles

<table>
<thead>
<tr>
<th>Characteristics</th>
<th>All cases (N=6483)</th>
<th>24-hour-NIHSS Item Profiles</th>
<th>24-hour-NIHSS Item Profiles</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>a (n=588)</td>
<td>b (n=1376)</td>
<td>c (n=464)</td>
</tr>
<tr>
<td>24-hour NIHSS,</td>
<td>6 (1-12)</td>
<td>19 (15-23)</td>
<td>16 (12-20)</td>
</tr>
<tr>
<td>median (IQR)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age; years, median (IQR)</td>
<td>68 (59-76)</td>
<td>71 (65-78)</td>
<td>69 (62-77)</td>
</tr>
<tr>
<td>Female, n(%)</td>
<td>2581 (40)</td>
<td>360 (43)</td>
<td>561 (41)</td>
</tr>
<tr>
<td>Pre-stroke mRS, median (IQR)</td>
<td>0 (0)</td>
<td>0 (0)</td>
<td>0 (0)</td>
</tr>
<tr>
<td>mRS 0-1, n(%)</td>
<td>5899 (91)</td>
<td>517 (88)</td>
<td>1252 (91)</td>
</tr>
<tr>
<td>mRS 2-5, n(%)</td>
<td>438 (7)</td>
<td>58 (9.9)</td>
<td>89 (6.5)</td>
</tr>
<tr>
<td>Systolic BP</td>
<td>150±21</td>
<td>152±20</td>
<td>150±21</td>
</tr>
<tr>
<td>Diastolic BP</td>
<td>82±13</td>
<td>83±13</td>
<td>82±13</td>
</tr>
<tr>
<td>Weight; kg</td>
<td>77±14</td>
<td>77±14</td>
<td>77±14</td>
</tr>
<tr>
<td>Glucose; mmol/L</td>
<td>7.1±2.4</td>
<td>7.4±2.5</td>
<td>7.4±2.7</td>
</tr>
<tr>
<td>Ischemic changes on baseline neuroimaging, n(%)</td>
<td>1315 (20)</td>
<td>148 (25)</td>
<td>342 (25)</td>
</tr>
<tr>
<td>RtPA dose; mg</td>
<td>68±12</td>
<td>68±12</td>
<td>69±11</td>
</tr>
<tr>
<td>Onset time to treatment; mins</td>
<td>136±32</td>
<td>135±31</td>
<td>135±32</td>
</tr>
<tr>
<td>Medical history, n(%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Current smoker</td>
<td>1474 (23)</td>
<td>101 (17)</td>
<td>289 (21)</td>
</tr>
<tr>
<td>Atrial fibrillation</td>
<td>1507 (24)</td>
<td>195 (33)</td>
<td>373 (27)</td>
</tr>
<tr>
<td>Chronic heart failure</td>
<td>476 (8)</td>
<td>62 (11)</td>
<td>112 (8)</td>
</tr>
<tr>
<td>Previous Stroke</td>
<td>643 (10)</td>
<td>61 (10)</td>
<td>124 (9)</td>
</tr>
<tr>
<td>Hypertension</td>
<td>3710 (59)</td>
<td>375 (64)</td>
<td>843 (61)</td>
</tr>
<tr>
<td>Hyperlipidemia</td>
<td>1967 (30)</td>
<td>178 (30)</td>
<td>429 (31)</td>
</tr>
<tr>
<td>Diabetes</td>
<td>1020 (16)</td>
<td>102 (17)</td>
<td>272 (20)</td>
</tr>
<tr>
<td>Drug history, n(%)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Antiplatelet (any)</td>
<td>2216 (34)</td>
<td>234 (40)</td>
<td>494 (36)</td>
</tr>
<tr>
<td>Anticoagulant treatment†</td>
<td>10 (0.2)</td>
<td>2 (0.3)</td>
<td>1 (0.1)</td>
</tr>
</tbody>
</table>
All continuous variables were given in mean ± standard deviations, unless stated otherwise. NIHSS indicates National Institutes of Health Stroke Scale; IQR, interquartile range; mRS, modified Rankin Scale; BP, blood pressure; tPA, tissue plasminogen activator; mins, minutes.

*Thrombolysis treatment with tPA.
†Vitamin K antagonist.
**Supplementary Table II:** Outcome measures by 24-hour-NIHSS item profiles

<table>
<thead>
<tr>
<th>24hour-NIHSS Profile</th>
<th>Ordinal analysis mRS at 90 days</th>
<th>Good outcome at 90 days (mRS 0-1 or back to baseline)</th>
<th>Mortality at 90 days</th>
<th>Survival</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>OR</td>
<td>95% CI</td>
<td>No. of good outcome</td>
</tr>
<tr>
<td>a (n=588)</td>
<td>Ref</td>
<td></td>
<td></td>
<td>19</td>
</tr>
<tr>
<td>b (n=1376)</td>
<td>2.94</td>
<td>2.45-3.53</td>
<td>124</td>
<td>3.06</td>
</tr>
<tr>
<td>c (n=464)</td>
<td>21.30</td>
<td>16.77-27.05</td>
<td>201</td>
<td>24.19</td>
</tr>
<tr>
<td>d (n=1298)</td>
<td>8.11</td>
<td>6.70-9.82</td>
<td>349</td>
<td>11.00</td>
</tr>
<tr>
<td>e (N=582)</td>
<td>20.55</td>
<td>16.37-25.80</td>
<td>260</td>
<td>23.92</td>
</tr>
<tr>
<td>f (n=2154)</td>
<td>65.88</td>
<td>54.13-80.17</td>
<td>1502</td>
<td>70.16</td>
</tr>
</tbody>
</table>

Adjusted for age, sex and pre-stroke mRS. OR indicates odd ratio; mRS, modified Rankin Scale; CI, confidence interval; HR, hazard ratio; Ref, reference and NIHSS, National Institutes of Health Stroke Scale.
### Supplementary Table III: Outcome measures of patients with baseline NIHSS score of 10, according to baseline-NIHSS item profiles

<table>
<thead>
<tr>
<th>Baseline-NIHSS Item Profile</th>
<th>Ordinal analysis mRS at 90 days</th>
<th>Good outcome at 90 days (mRS 0-1 or back to baseline)</th>
<th>Mortality at 90 days</th>
<th>Survival</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Median mRS</td>
<td>OR</td>
<td>95% CI</td>
<td>No. of good outcome</td>
</tr>
<tr>
<td>A (n=13)</td>
<td>3</td>
<td>Ref</td>
<td>...</td>
<td>6</td>
</tr>
<tr>
<td>B (n=86)</td>
<td>1</td>
<td>1.94</td>
<td>0.69-5.50</td>
<td>43</td>
</tr>
<tr>
<td>C (n=42)</td>
<td>1</td>
<td>2.17</td>
<td>0.72-6.61</td>
<td>25</td>
</tr>
<tr>
<td>D (n=153)</td>
<td>2</td>
<td>1.84</td>
<td>0.67-5.04</td>
<td>73</td>
</tr>
<tr>
<td>E (n=49)</td>
<td>1</td>
<td>2.29</td>
<td>0.75-6.95</td>
<td>23</td>
</tr>
<tr>
<td>F (n=2)</td>
<td>1</td>
<td>10.99</td>
<td>0.98-30.9</td>
<td>2</td>
</tr>
</tbody>
</table>

Adjusted for age, sex and pre-stroke mRS. OR indicates odd ratio; mRS, modified Rankin Scale; CI, confidence interval; HR, hazard ratio; Ref, reference; NIHSS, National Institutes of Health Stroke Scale and NS, non-significant OR or HR with very wide confidence interval.
SUPPLEMENTAL FIGURES AND FIGURE LEGENDS

Supplementary Figure I: Boxplots of NIHSS total score by baseline-NIHSS item profile.
**Supplementary Figure II:** mRS outcome at Day 90, comparing *baseline*-NIHSS items Profile A (top) and comparator *baseline*-NIHSS items profile (bottom). Values provided in each box denote percentage of patients.

<table>
<thead>
<tr>
<th>Profile</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>dead</th>
</tr>
</thead>
<tbody>
<tr>
<td>A</td>
<td>11.3</td>
<td>11.7</td>
<td>9.7</td>
<td>15</td>
<td>18.5</td>
<td>11.3</td>
<td>22.6</td>
</tr>
<tr>
<td>B</td>
<td>11</td>
<td>15.5</td>
<td>16</td>
<td>18.2</td>
<td>18</td>
<td>7</td>
<td>14.5</td>
</tr>
<tr>
<td>C</td>
<td>34.8</td>
<td>26</td>
<td>20.6</td>
<td>9.5</td>
<td>0.9</td>
<td>5.4</td>
<td></td>
</tr>
<tr>
<td>D</td>
<td>20.6</td>
<td>21.4</td>
<td>16.4</td>
<td>13.9</td>
<td>14.3</td>
<td>4</td>
<td>9.4</td>
</tr>
<tr>
<td>E</td>
<td>30.3</td>
<td>27.9</td>
<td>17.7</td>
<td>12.5</td>
<td>7.1</td>
<td>2.3</td>
<td></td>
</tr>
<tr>
<td>F</td>
<td>35.8</td>
<td>36.2</td>
<td>16.6</td>
<td>5.5</td>
<td>0.7</td>
<td>2.3</td>
<td></td>
</tr>
</tbody>
</table>
Supplementary Figure III: Boxplots of NIHSS total score by 24-hour-NIHSS item profile.
**Supplementary Figure IV**: mRS outcome at Day 90, comparing 24-hour-NIHSS items Profile 1 (top) and comparator 24-hour-NIHSS items profile (bottom). Values provided in each box denote percentage of patients.
SUPPLEMENTAL DATA

Discrimination and Calibration of Baseline- and 24hour- NIHSS Item Profiles

Outcome: Good outcome at 90 days (mRS 0-1 or back to baseline)

AUROC - Baseline-NIHSS Item Profiles: 0.67 (0.66-0.69),
Hosmer-Lemeshow goodness-of-fit test, p=0.854.

24hour-NIHSS Item Profiles: 0.81 (0.80-0.82),
Hosmer-Lemeshow goodness-of-fit test, p=0.453.

AUROC difference between the two set of symptom profiles above, p <0.001.

Outcome: Death by 90days

AUROC - Baseline-NIHSS Item Profiles: 0.68 (0.67-0.71),
Hosmer-Lemeshow goodness-of-fit test, p=0.911.

24hour-NIHSS Item Profiles: 0.78 (0.76-0.80),
Hosmer-Lemeshow goodness-of-fit test, p=0.566.

AUROC difference between the two set of symptom profiles above, p<0.001.

*AUROC: area under the receiver operating curve.
중추성뇌졸중 후 통증의 치료
무작위 대조군 임상시험의 체계적 고찰

Management of Central Poststroke Pain
Systematic Review of Randomized Controlled Trials

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Key Words: evidence-based medicine ■ pain management ■ review ■ stroke ■ therapeutics

배경과 목적
중추성뇌졸중 후 통증은 뇌졸중 후 발생하는 반성신경병증질환이다. 본 질환의 치료에 대한 최근 연구는 제한적이며 치료에 대한 체계적 고찰은 시행된 바 없다.

방법
우리는 중추성뇌졸중 후 통증의 치료 효과를 평가하기 위해 진행된 무작위 대조군 임상시험을 모아 체계적인 고찰을 시행하였다. AMED, CENTRAL, CINAHL, DARE, EMBASE, HealthSTAR, MEDLINE, and PsychINFO로부터 다양한 언어로 작성된 무작위 대조군 임상시험(459명)을 포함하였다. 그 결과, 이들 임상시험의 체계적 고찰은 항경련제, 항우울제, 오피오이드길항제, 반복경두개자기극법, 침술을 평가한 시험이었다. 체계적 고찰 결과 모든 치료법들은 동통과 다른 환자-중요결과에 대해 어떠한 유의한 효과도 보이지 못하였다. 치료 효과에 대한 신뢰도는 매우 낮음에서 낮음을 수준으로 평가하였다.

결과
8개의 영어로 작성된 무작위 대조군 임상시험(459명)을 포함하였는데, 이들은 항경련제, 항우울제, 오피오이드길항제, 반복경두개자기극법, 침술을 평가한 시험이었다. 체계적 고찰 결과 모든 치료법들은 통증과 다른 환자-중요결과에 대해 어떠한 유의한 효과도 보이지 못하였다. 치료 효과에 대한 신뢰도는 매우 낮음에서 낮음을 수준으로 평가하였다.

결론
본 연구 결과는 주임상 진료지침과 상반된다. 무작위 대조군 임상시험에서 평가했던 약물들에 대해서 현재 주요로는 어떤 치료법도 이익을 준다고 보이 어렵다.