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Effectiveness of Epidural Steroid Injections in Lumbar Radiculopathy: A Systematic Review of Randomized Controlled Trials

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Abstract

Lumbar radiculopathy is a very common condition that is often treated through methods such as epidural steroid injections (ESIs), although the longer-term effects of these injections are unclear. The term 'radiculopathy' encompasses patients with true radiculopathy, radiculitis, and radicular pain, distinctions that carry therapeutic implications. The purpose of this study is to perform a Synthesis Without Meta-analysis (SWiM) of all Randomised Controlled Trials (RCTs) published between 2010 and 2026 and compare the effectiveness of ESIs used to treat lumbar radiculopathy. Due to substantial clinical and methodological heterogeneity in the route of ESI administration, comparator interventions, outcome measures/scales, and follow-up durations, meta-analysis was not undertaken. Variation in corticosteroid formulation, particularly between particulate steroids such as methylprednisolone and triamcinolone and non-particulate steroids such as dexamethasone, represented an additional source of clinical heterogeneity, as these preparations differ in safety profile, particularly for transforaminal injection, and possibly in duration of effect. Instead, the findings of the included RCTs were synthesised narratively. The narrative synthesis was reported in accordance with the SWiM reporting guideline. The review included caudal, interlaminar, and transforaminal ESI in adults with lumbar radiculopathy. Risk of Bias 2 (RoB 2) tool was used to assess the risk of bias. Thirteen RCTs were included. At short-term follow-up, several included trials reported improvements in pain and disability following ESI; however, the magnitude of change varied across studies, treatment arms, outcome instruments, and assessment time points. Pain outcomes were assessed using different scales, including visual analogue scale (VAS) and Numerical Rating Scale (NRS), while disability was evaluated using instruments such as the Oswestry Disability Index (ODI), Modified Oswestry Disability Questionnaire (MODQ), and Roland-Morris Disability Questionnaire (RMDQ). Therefore, these within-group changes were not considered directly comparable, and comparative treatment effects were interpreted separately according to the reporting of each trial. Short-term and longer-term outcomes varied across trials according to the ESI technique, comparator intervention, procedural modifications, and use of adjunctive treatments; therefore, no consistent superiority of one approach over another could be established. ESIs are most beneficial as short-term or bridging interventions for patients with lumbar radiculopathy. The available randomised data is inconclusive about the long-term benefit of any technique, and further robust, well-controlled studies are clearly warranted, with standardised diagnostic criteria distinguishing radiculopathy from radiculitis and radicular pain, consistent reporting of corticosteroid formulation, and designs capable of identifying patient subsets who may benefit from specific injection routes and specific steroid preparations.

Categories: Sports Medicine, Pain Management, Physical Medicine & Rehabilitation
Keywords: chronic lumbar radiculopathy, epidural steroid injection, lumbar epidural, oswestry disability index (odi), : pain

Introduction And Background

Lumbar radiculopathy denotes the objective loss of nerve-root function (dermatomal sensory loss, myotomal weakness, or diminished reflexes) resulting from conduction block along a lumbar nerve root [1]. It should be distinguished from radicular pain, which is neuropathic pain evoked by ectopic discharge from an affected dorsal root or its ganglion and radiating along the lower limb in a narrow band, and from radiculitis, which denotes inflammation of the nerve root, a presumed mechanism of radicular pain rather than a clinical finding [1]. Leg pain of nociceptive origin is more accurately termed somatic referred pain and is neither radicular pain nor radiculopathy. These presentations most commonly arise from lumbar disc herniation, although foraminal or lateral recess stenosis and other degenerative disorders may also contribute [1]. In much of the published literature, including the trials in this review, these terms are used interchangeably: most included trials enrolled patients based on radiating leg pain rather than objective neurological deficit, several explicitly excluded patients with such deficits [2-4], and only one applied a formal definition of radicular pain [5]. This inconsistency represents a recognized limitation of the literature, introducing clinical heterogeneity and limiting the precision of aggregate interpretation. Throughout this review, ‘radiculopathy’ is used as defined by the original study authors. Non-compressive causes of radicular

symptoms (for example, diabetic lumbosacral radiculoplexus neuropathy, herpes zoster, and neoplastic nerve-root infiltration) and extra-spinal conditions that mimic them (for example, lumbosacral plexopathy, piriformis syndrome, and sacroiliac joint or hip pathology) are not effectively managed by epidural steroid injections (ESIs) and are outside the scope of this review, which focuses on radiculopathy of spinal and degenerative origin.

Nerve-root compression can trigger local and neurogenic inflammatory responses, including the release of inflammatory mediators and changes in nerve-root vascular permeability, which may contribute to radicular pain and neurological symptoms even when mechanical compression is limited [6]. The natural history of lumbar radiculopathy is often favorable, with many patients experiencing substantial spontaneous improvement during the first several weeks to months through inflammation resolution and regression of the underlying disc lesion [7-9]. Consequently, initial management is generally conservative and may include education, activity maintenance, exercise-based physical therapy, analgesic or anti-inflammatory medication, and selected complementary interventions [10,11]. For patients with persistent or severe radicular symptoms despite conservative management, additional options include ESIs and, in selected patients with progressive neurological deficits, severe refractory symptoms, or appropriate structural pathology, and surgical decompression. The choice depends on symptom severity, neurological findings, imaging characteristics, symptom duration, patient preferences, and response to previous treatment [10,11].

ESIs are intended to deliver corticosteroids and, in most techniques, a local anesthetic near the affected spinal nerve root or within the epidural space. Corticosteroids may reduce local inflammation, edema, and inflammatory mediator production around an irritated nerve root, providing symptomatic relief [12,13]. Corticosteroid preparations used for ESIs are not pharmacologically equivalent. Particulate formulations (methylprednisolone acetate, triamcinolone acetonide, and betamethasone suspensions) form insoluble aggregates that may prolong local residence, whereas dexamethasone sodium phosphate is soluble and non-particulate. Inadvertent intra-arterial injection of particulate steroid can cause embolic spinal cord or brain infarction, a rare but catastrophic complication most relevant to the transforaminal route, and multisociety recommendations therefore favor dexamethasone for the initial lumbar transforaminal injection [14]. Variation in steroid agent and dose across trials is therefore a potential source of heterogeneity. ESIs are therefore primarily considered a symptom-modifying intervention rather than a treatment that directly reverses the underlying structural cause of radiculopathy. Their potential clinical benefits include reduced radicular pain and improved function, particularly when pain limits participation in rehabilitation. However, the magnitude and durability of these effects remain uncertain, and evidence regarding whether ESIs meaningfully alter the subsequent need for surgery is inconsistent [11,15].

ESIs can be administered through three principal approaches: transforaminal, interlaminar, and caudal. Transforaminal injections are directed toward the exiting or traversing nerve-root region and may provide relatively targeted delivery to the symptomatic level. Interlaminar injections introduce medication into the posterior epidural space. They may spread across one or more levels. In contrast, caudal injections deliver medication through the sacral hiatus and may be useful when multilevel or lower lumbar epidural spread is desired [16,17]. Differences in anatomical target, injectate distribution, technical requirements, and potential complications may therefore influence treatment response. Nevertheless, no single injection route has consistently demonstrated superior long-term clinical effectiveness across all patient populations [18,19].

The clinical interpretation of ESI effectiveness also depends on the timing of outcome assessment. In this review, short-term outcomes refer to assessments conducted within three months after treatment, intermediate-term outcomes to assessments conducted after three months but before six months, and long-term outcomes to assessments performed at or beyond six months. This distinction is clinically important because improvements observed shortly after injection may reflect transient suppression of inflammation and pain. In contrast, sustained improvement at six months or more would provide stronger evidence of a durable treatment effect. Previous systematic reviews have generally reported small to moderate improvements in pain and disability following ESIs, particularly during shorter follow-up periods, but the persistence of these effects at longer follow-up remains uncertain [20,21].

Current clinical guidance generally supports ESIs for selected patients with persistent radicular symptoms, particularly when conservative treatment has not provided adequate relief. Still, recommendations differ on patient selection, injection approach, corticosteroid formulation, and expected duration of benefit [11,20]. Important controversies also remain regarding the comparative effectiveness of transforaminal, interlaminar, and caudal approaches and whether early improvements are sustained over longer periods. Furthermore, variation in patient populations, injection techniques, corticosteroid preparations, comparators, outcome measures, and follow-up durations makes it difficult to determine whether apparent treatment effects represent durable clinical benefit or predominantly short-term symptom relief [20,21].

Although several randomized controlled trials (RCTs) have evaluated different ESI approaches, techniques, comparators, and injectates, the evidence for sustained benefit remains heterogeneous. In particular, uncertainty persists regarding whether ESIs provide clinically meaningful improvements in patient-reported pain and disability beyond the early post-treatment period and whether any specific injection approach is associated with more favorable long-term outcomes. A focused synthesis of RCT evidence with prespecified

long-term follow-up is therefore clinically relevant because it can distinguish transient treatment responses from sustained benefit and help clarify the extent to which differences in injection approach translate into meaningful patient outcomes. The review aimed to determine whether ESIs provide clinically meaningful, sustained improvements in pain and disability compared with control or alternative treatment approaches and to explore whether the magnitude or durability of benefit differs by injection route or treatment approach. An additional consideration is whether specific patient subsets, defined by symptom duration, pain mechanism, etiology, injection route, or steroid formulation, may derive greater or more sustained benefit from targeted ESI strategies. While definitive subgroup analysis lies beyond the scope of this review, this concept warrants attention in future research and cautions against applying aggregate findings uniformly across all patients. Because we anticipated substantial methodological and clinical heterogeneity, we synthesised findings narratively in accordance with the Synthesis Without Meta-analysis (SWiM) framework.

Review

Materials and methods

Guidelines and Reporting Standards

This systematic review was conducted and reported in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 statement [22]. Given the substantial methodological heterogeneity across the included randomized controlled trials, particularly in terms of intervention approaches, comparators, outcome measures, and follow-up durations, the findings were synthesized narratively in accordance with the SWiM reporting framework [23].

Search Strategy

A comprehensive literature search was conducted in PubMed, Scopus, and Google Scholar for studies published from January 1, 2010 to June 30, 2026 (Table *1*). In PubMed, database filters for randomized controlled trials, English language, human studies, exclusion of preprints, and the specified publication period were applied electronically. In Scopus, the database-applied filters were limited to document type (article), English language, and publication year (2010-2026); study design and other eligibility criteria were assessed manually during the screening process rather than treated as database filters. In Google Scholar, the search was conducted using the specified search string, yielding 658 results. The results were sorted by relevance, and all 658 records were screened. During screening, the publication year range was limited to 2010-2026, and patents and citation-only records were omitted when relevant. Clinical trial registries and grey-literature sources were not formally searched, which may have resulted in the omission of unpublished or ongoing studies. The database-specific search strategies and numbers of records retrieved before and after database-applied filters are presented in Table *1*.

Database	Search String	Before filter applied	Filter	After filter applied
PubMed	(("lumbar radiculopathy"[Title/Abstract] OR sciatica[Title/Abstract] OR "lumbar disc herniation"[Title/Abstract] OR "nerve root compression"[Title/Abstract]) AND ("epidural steroid injection"[Title/Abstract] OR "epidural steroid injections"[Title/Abstract] OR "epidural corticosteroid injection"[Title/Abstract] OR transforaminal[Title/Abstract] OR interlaminar[Title/Abstract] OR caudal[Title/Abstract]))	1580	Randomized Controlled Trial, English, Humans, Exclude preprints, from 2010/1/1 - 2026/6/30	74
Google scholar	"epidural steroid injection" "lumbar radiculopathy" ("randomized controlled trial" OR randomized OR randomised) (VAS OR "visual analog scale" OR "visual analogue scale") (ODI OR "Oswestry Disability Index")	658	Sorted by relevance; all 658 results screened Year range: 2010–2026 Patents and citation-only records were excluded during screening	63
Scopus	TITLE-ABS-KEY ("epidural steroid injection" AND (lumbar radiculopathy OR sciatica) AND (VAS OR NRS OR ODI OR "Roland Morris") AND (randomized OR randomised OR trial) AND (follow-up OR "long term" OR outcome))	73	Document type: Article Language: English Year range: 2010–2026	58

TABLE 1: Search Strings Used for Literature Search

Eligibility Criteria

RCTs enrolling adults with clinically diagnosed lumbar radiculopathy or sciatica were eligible for inclusion. We based clinical diagnosis on documented radicular leg pain and/or associated neurological symptoms or signs consistent with lumbar nerve-root involvement; imaging confirmation was not required once a clinical diagnosis was established. Participants with lumbar radiculopathy attributable to lumbar disc herniation, spinal stenosis, or other documented lumbar degenerative causes were eligible. Coexisting axial low back pain was permitted but was not required. Studies were eligible if participants received epidural steroid injections through caudal, interlaminar, or transforaminal approaches and reported short-, intermediate-, and/or long-term outcomes related to pain and/or disability. Authors excluded non-randomized studies, observational studies, case series, case reports, editorials, conference abstracts, and other non-primary research publications. We also excluded studies published in languages other than English.

Study Selection

After eliminating duplicate records, the authors reviewed titles and abstracts against the following predetermined eligibility criteria. We then evaluated full-text articles for inclusion.

Data Extraction

The authors used a standardized data-collection form during data extraction to ensure consistency across the included studies. Extracted variables included study design and country of origin, sample size, and participant characteristics. We also gathered intervention characteristics, such as the route of epidural steroid injection, the composition of the injectates used, and information on the comparator intervention. We extracted the following data from each included study: study characteristics, group-specific sample sizes, participant characteristics, intervention and comparator details, follow-up duration, and outcome data. For pain and disability, we extracted group-specific baseline and follow-up scores using validated measures, including the Visual Analogue Scale (VAS) [24], Numeric Rating Scale (NRS) [25], Oswestry Disability Index (ODI) [26], Modified Oswestry Disability Questionnaire (MODQ) [27], and Roland-Morris Disability Questionnaire (RMDQ) [28], where reported. We also extracted between-group treatment effects, measures of dispersion (e.g., standard deviations or standard errors), 95% confidence intervals, p-values, and change-from-baseline data, where available. Safety and clinical outcome data included adverse events, treatment crossover, additional epidural injections or other procedures, and subsequent surgery, where reported. We recorded follow-up duration to distinguish intermediate- and long-term outcomes. Two reviewers independently extracted data and resolved disagreements through discussion or consultation with a third reviewer.

Risk of Bias Assessment

Risk of bias in the results for each prespecified primary outcome and relevant time point was assessed using the Cochrane Risk of Bias 2 (RoB 2) tool for randomized trials. We assessed the risk of bias at the outcome/result level across the five RoB 2 domains and judged each result as low risk of bias, some concerns, or high risk of bias, according to the RoB 2 guidance [29]. Two reviewers independently assessed risk of bias and resolved disagreements through discussion or consultation with a third reviewer.

Data Synthesis

We did not conduct a statistical meta-analysis because of substantial clinical and methodological heterogeneity across the included studies, including differences in injection techniques, comparator groups, outcome measures, and follow-up durations. Therefore, we synthesized the findings narratively in accordance with the SWiM reporting guidance. We categorized studies by injection technique, comparator intervention, outcome domain, and follow-up period and compared findings within clinically relevant groups.

Results

The PRISMA 2020 flow diagram illustrates the study selection process: 195 records were identified from PubMed (n=74), Scopus (n=58), and Google Scholar (n=63). After removing 35 duplicate records, 160 records were screened, and 105 were excluded. We sought 55 reports for retrieval, and all 55 were successfully retrieved and assessed for eligibility. After full-text assessment, 42 reports were excluded because of an irrelevant intervention (n=18), incomplete outcome data (n=15), a non-randomized design (n=6), or other reasons (n=3). Ultimately, 13 studies met the eligibility criteria and were included in the systematic review. The PRISMA 2020 flow diagram (Figure *1*) presents the identification, screening, eligibility assessment, and inclusion of studies.

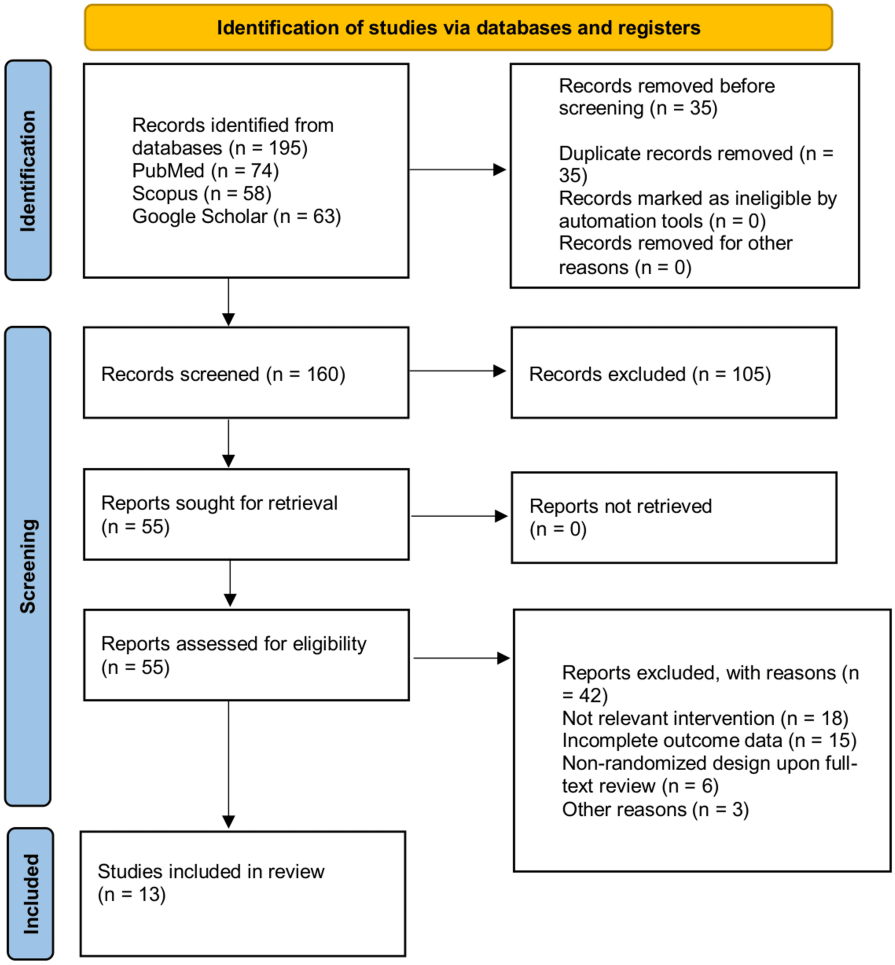

FIGURE 1: PRISMA 2020 flow diagram

PRISMA: Preferred Reporting Items for Systematic Reviews and Meta-Analyses.

Characteristics of the Included Randomized Controlled Trials

Table 2 summarizes the characteristics of the included randomized controlled trials, including study design, sample size, participant population, intervention and comparator details, follow-up duration, and prespecified outcomes.

Study	Design	Comparison	N	Corticosteroid (dose)	Follow-up
Rados et al., 2011 [4]	Parallel-group RCT	ILESI vs. TFESI	70	Methylprednisolone 80 mg (IL) / 40 mg (TF)	6 months
Maadawy et al., 2018 [30]	Parallel-group RCT	Infraneural vs. parasagittal ILESI	40	Methylprednisolone 80 mg	6 months
Sencan et al., 2019 [31]	Parallel-group RCT	TFESI with vs. without sedation	64	Methylprednisolone 80 mg	3 months
Makkar, 2019 [32]	Parallel-group RCT	PIL-TFESI vs. midline TFESI	65	Methylprednisolone 80 mg	6 months
Khan and Akram, 2025 [33]	Randomized comparative trial	CESI vs. ILESI	338	Methylprednisolone 120 mg	3 months
Moazzam et al., 2021 [34]	Parallel-group RCT	Kambin triangle vs. conventional ESI	80	Not reported	8 weeks
Wilby et al., 2021 [35]	Multicenter RCT	TFESI vs. microdiscectomy	163	Triamcinolone 20–60 mg	18 weeks
Kumar et al., 2022 [36]	Parallel-group RCT	Parasagittal vs. midline ILESI	60	Methylprednisolone 80 mg	6 months
Ghai et al., 2013 [37]	Parallel-group RCT	ESI vs. autologous conditioned serum	68	Methylprednisolone 80 mg	6 months
Kumar et al. 2023 [38]	Parallel-group RCT	ILESI vs. TFESI	60	Triamcinolone 40 mg	4 weeks
Ozturk et al. 2023 [39]	Parallel-group RCT	TFESI vs CESI	60	Dexamethasone 12 mg	3 months
Ter Meulen et al. 2023 [40]	Parallel-group RCT	TFESI + bupivacaine vs bupivacaine alone/NSAIDs	141	Methylprednisolone 40 mg	6 months
Napoli et al. 2023 [41]	Parallel-group RCT	TFESI + PRF vs TFESI alone	351	Dexamethasone 20 mg or triamcinolone 80 mg	12 months

TABLE 2: Characteristics of included randomized controlled trials

Methylprednisolone acetate and triamcinolone acetonide are particulate corticosteroids; dexamethasone is non-particulate.

ESI=Epidural steroid injection; TFESI=transforaminal ESI; ILESI=interlaminar ESI; PIL=parasagittal interlaminar ESI; CESI=caudal ESI; PRF=pulsed radiofrequency.

Thirteen RCTs met the inclusion criteria. Most used parallel-group designs; three were multicenter, and two had three arms. Total participants ranged from 40 to 351, and follow-up ranged from four weeks to 12 months. Across the included trials, short-term pain and disability improved after ESI. Transforaminal approaches often showed stronger early pain reduction than interlaminar or caudal approaches, while long-term benefit was variable and more evident in trials that used procedural optimization or adjunctive treatment such as pulsed radiofrequency.

Quantitative Outcome Extraction for Pain and Disability

Table 3 reports outcome data by randomized group at each prespecified follow-up time point, including between-group differences, measures of dispersion, and 95% confidence intervals where available.

Study	Intervention vs Comparator	n/arm	Outcome & scale	Time point	Baseline, Intervention vs. Comparator	Follow-up, Intervention vs. Comparator	Between-group effect/p-value

Rados et al., 2011 [4]	Interlaminar ESI vs. transforminal ESI	32/32	VAS (0–10; higher = worse); Oswestry	24 wk	VAS: NR; Oswestry: 52% vs 53%	VAS: 4.0±2.2 vs 3.8±2.1; Oswestry: 39% vs 38%	VAS p = .717; Oswestry exact comparative effect NR
Maadawy et al., 2018 [30]	Infraneural TEFSI vs. parasagittal IL ESI	20/20	VAS (0–10); MODQ (0–100; higher = worse)	Up to 6 mo	NR	NR	VAS is significantly lower with infraneural TFESI; exact MD/CI NR.
Sencan et al., 2019 [31]	TFESI with sedation vs TFESI without sedation	31/33	Procedural pain: NRS-11 (0–10; higher = worse); patient/physician satisfaction: Likert scale	Periprocedural	NRS: NR	NRS: NR; satisfaction: NR	Periprocedural NRS is significantly lower with sedation (p=.0001); patient and physician satisfaction are significantly higher (p=.0001); desire for the same technique is p=.001. Regression: NRS OR=0.341; same-technique preference OR=0.648; patient satisfaction OR=0.329; physician satisfaction OR=0.514; all p=.0001.
Makkar, 2019 [32]	PIL vs MIL vs TFESI	PIL 20; TF 20; MIL NR	VAS; MODQ	3 and 6 mo	NR	NR	PIL vs MIL: VAS p=.02 at 3 mo, <.001 at 6 mo; TF vs MIL: p=.50 at 3 mo, .02 at 6 mo; MODQ favored PIL/TF vs MIL
Khan and Akram, 2025 [33]	Interlaminar ESI vs Caudal ESI	208/208*	NPRS (0–10); ODI (0–100)	12 wk	NR	NPRS: 2.65±1.084 vs 3.25±1.332; ODI: 13.97±2.045 vs 15.91±3.277	NPRS MD −0.60, p=.0001; ODI MD −1.94, p=.0001
Moazzam et al., 2021 [34]	Conventional TFESI vs. Kambin's triangle TFESI	40/40	NRS (0–10; higher = worse)	4-8 wk	7.28±1.26 vs 7.33±1.16	2.14±0.81 vs 2.70±0.94	Between-group p=.21
Wilby et al., 2021 [35]	TFESI vs Microdiscectomy	63/61 at 18 wk	ODQ (higher = worse)	18 wk	NR	30.02±24.38 vs 22.30±19.83	MD −4.25 (95% CI −11.09 to 2.59), p= 22
Kumar et al., 2022 [36]	Midline ILESI vs Parasagittal ILESI	30/30	Pain; ODI (0–100; higher = worse)	Up to 6 mo	NR	NR	No significant between-group pain difference; disability clinically favored the parasagittal approach.
Ghai et al., 2013 [37]	Parasagittal vs. midline ILESI;	37	VAS; MODQ	6 mo	NR	NR	≥50% pain relief: 68.4% vs 16.7%; RR 4.10 (95% CI 1.40–12.05), p=.001
Kumar et al. 2023 [38]	TFESI vs ILESI	30/30	NRS and VAS	2-4 wk	NR	NR	Greater pain reduction with TFESI at 2 wk; exact effect NR
Ozturk et al. 2023 [39]	Caudal ESI vs TFESI	30/30	NRS (0–10); ODI (0–100)	3 mo	NR	NR	Treatment success: 77% vs. 73%, p = .766; no significant between-group difference
Ter Meulen et al. 2023 [40]	TF corticosteroid injection vs. usual care	3-arm RCT; arm n NR	Leg pain; physical function	Up to 6 mo	NR	NR	Leg pain MD −0.96 (95% CI −1.83 to −0.09) vs. usual care
Napoli et al. 2023 [41]	PRF + TFESI vs TFESI alone	174/177	NRS (0–10); ODI (0–100); RMDQ (0–24)	1 and 52 wk	NRS: 8.1±1.1 vs 7.9±1.1	Week 1 NRS: 3.2±0.2 vs 5.4±0.2; Week 52: 1.0±0.2 vs 3.9±0.2	Week 1 NRS ATE 2.3 (95% CI 1.9–2.8), p<.001; Week 52 ATE 3.0 (2.4–3.5), p<.001; ODI ATE 11.0 (6.4–15.6), p<.001; RMDQ ATE 2.9 (1.6–4.3), p<.001

TABLE 3: Quantitative outcome extraction for pain and disability

NR=Not reported; NS=not statistically significant; ESI=epidural steroid injection; TFESI=transforaminal ESI; ILESI=interlaminar ESI; PIL=parasagittal interlaminar ESI; MIL=midline interlaminar ESI; VAS=Visual Analogue Scale; NRS/NPRS=Numeric Rating Scale; ODI=Oswestry Disability Index; MODQ=Modified Oswestry Disability Questionnaire; RMDQ=Roland-Morris Disability Questionnaire; ODQ=Oswestry Disability Questionnaire; ATE=average treatment effect, PRF=pulsed radiofrequency, IL=interlaminar; TF=transforaminal; RCT=randomized controlled trial.

The quantitative extraction table above reports mean baseline score, mean follow-up score, and mean change for pain and disability outcomes as presented in the study-level synthesis dataset. Negative change values indicate improvement from baseline to follow-up. Pain change was calculated as follow-up minus baseline on the reported VAS/NRS-type scale, and disability change was calculated as follow-up minus baseline on the reported ODI/MODQ/RMDQ-type scale.

Quality Assessment of Included Randomized Controlled Trials

Table *4* summarizes the RoB 2 assessment of the included randomized controlled trials across the five domains of risk of bias.

Study	Randomization	Deviations	Missing data	Outcome measurement	Selective reporting	Overall judgment
Rados et al., 2011 [4]	Low	Some concerns	Low	Some concerns	Low	Some concerns
Maadawy et al., 2018 [30]	Some concerns	Some concerns	Low	Some concerns	Some concerns	Some concerns
Sencan et al., 2019 [31]	Some concerns	Some concerns	Low	Some concerns	Low	Some concerns
Makkar, 2019 [32]	Low	Some concerns	Low	Some concerns	Low	Some concerns
Khan & Akram, 2025 [33]	Low	Some concerns	Low	Some concerns	Low	Some concerns
Moazzam et al., 2021 [34]	Low	Some concerns	Low	Some concerns	Low	Some concerns
Wilby et al., 2021 [35]	Low	Some concerns	Low	Low	Low	Low
Kumar et al., 2022 [36]	Low	Some concerns	Low	Some concerns	Low	Some concerns
Ghai et al., 2013 [37]	Low	Some concerns	Low	Some concerns	Low	Some concerns
Kumar et al. 2023 [38]	Some concerns	Some concerns	Low	Some concerns	Low	Some concerns
Ozturk et al. 2023 [39]	Low	Some concerns	Low	Some concerns	Low	Some concerns
Ter Meulen et al. 2023 [40]	Low	Some concerns	Low	Some concerns	Low	Some concerns
Napoli et al. 2023 [41]	Low	Some concerns	Low	Some concerns	Low	Some concerns

TABLE 4: Cochrane Risk-of-Bias 2 (RoB 2) assessment.

We assessed risk of bias using the Cochrane Risk of Bias 2 (RoB 2) tool. Most included trials were judged to have some concerns regarding the overall risk of bias based on the RoB 2 signaling question algorithms. These judgments primarily reflected concerns across domains related to randomization, deviations from

intended interventions, missing outcome data, outcome measurement, and selection of the reported result. One trial was judged to have a low overall risk of bias across the assessed domains. Overall, although none of the included studies had a high overall risk of bias, concerns in most trials suggest that findings should be interpreted with appropriate caution.

Discussion

The present synthesis, conducted using the SWiM approach, indicates that several included trials reported improvements in pain intensity and disability during short- to intermediate-term follow-up among patients with lumbar radiculopathy. It must be acknowledged that the term 'radiculopathy' was applied inconsistently across the included trials, with most studies enrolling patients based on radiating leg pain rather than confirmed objective neurological deficit, and several explicitly excluding patients with such deficits. This terminological imprecision is not the original authors' fault. Still, it represents a recognized limitation of the existing literature that introduces clinical heterogeneity and limits the generalizability of aggregate findings. Several studies reported improvements within the first weeks to months after treatment; however, the magnitude and consistency of these effects varied across trials and comparators. Evidence for sustained benefit beyond six months was limited and inconsistent. Longer-term outcomes varied by injection approach, route of administration, comparator intervention, and follow-up duration. Some studies evaluating targeted transforaminal approaches, including transforaminal ESI combined with adjunctive pulsed radiofrequency, reported favorable outcomes; however, heterogeneity in interventions and outcome reporting limits conclusions about the comparative or sustained effectiveness of specific techniques. The clinical importance of the observed changes could not be consistently determined because studies used different outcome instruments, thresholds, and reporting formats.

Comparison With Previous Reviews

These results are generally consistent with the current evidence base and continue to support the current trend of considering ESIs as a short-term treatment. A revision of the systematic review collated results from 90 randomized controlled trials indicates that ESIs likely reduced short-term pain and disability in people with radiculopathy. Still, there was not enough evidence to confirm the long-term pain reduction. However, the review also noted uncertainty about sustained benefits, possibly only for long-term disability outcomes [20]. Likewise, for lumbosacral (lumbar-sacral) radicular pain, the 2020 Cochrane review found very few benefits from epidural corticosteroid injections compared with placebo, and these benefits were small and limited to the short term immediately after the injection [21]. The agreement between the conclusions in the present study and the results of these large-scale evidence syntheses strengthens confidence that ESIs provide some symptom relief, especially in the acute and subacute phases of radicular pain; however, ESIs cannot be used as a long-term solution in every case.

Comparison of Epidural Injection Techniques

A key consideration in this review was the variability in findings across injection approaches and sites. Direct comparisons of injection approaches produced heterogeneous results, limiting conclusions regarding the superiority of any specific technique. A 2021 systematic review and meta-analysis of fluoroscopically guided RCTs found strong evidence for epidural injections in chronic low back and lower-extremity pain with sciatica or lumbar radiculopathy at one and three months and moderate evidence at six and 12 months. The review also argued that local anesthetic alone may provide sustained benefit comparable to local anesthetic combined with steroids [16]. A network meta-analysis found that the parasagittal interlaminar (PIL) approach was most effective for short-term pain reduction, whereas the transforaminal (TF) approach was most effective for long-term pain reduction, although heterogeneity and risk of bias were important limitations [18]. The present review similarly identified variability in pain outcomes across injection approaches, with some trials reporting greater reductions following targeted techniques. One proposed explanation is that targeted injection techniques may improve corticosteroid delivery near the affected nerve root; however, this mechanism remains speculative and was not directly evaluated in the included trials.

Another underappreciated source of heterogeneity is the corticosteroid preparation (Table *2*). Most included trials used particulate steroids: methylprednisolone (40-120 mg) in seven trials and triamcinolone (20-60 mg) in two. Only one used dexamethasone alone [39], one permitted either dexamethasone or triamcinolone [3], and one did not report the agent [34]. Doses also varied considerably, and one trial found half the methylprednisolone dose delivered transforaminally as effective as the full dose delivered interlaminarly [4]. Because so few trials used non-particulate steroids and none compared formulations directly, the included evidence cannot show whether the formulation modifies efficacy. Notably, most transforaminal injections in the included trials used particulate steroids, which current safety recommendations discourage for the initial injection [14]. Agent, dose, and route should therefore be regarded as interrelated, unquantified contributors to between-trial variability.

Clinical Implications

The findings of this review have clinical implications for the use of ESIs in patients with lumbar radiculopathy. The included trials suggest that some ESI techniques may be associated with short-term improvements in pain and disability; however, the magnitude and durability of benefit relative to comparator interventions remain uncertain across heterogeneous studies. Evidence for sustained benefit beyond six months is limited and inconsistent. Therefore, ESI use should be considered within an individualized, multimodal management strategy rather than as a stand-alone intervention, with treatment decisions informed by the patient's diagnosis, preferences, potential benefits and risks, alternative treatment options, and shared decision-making. This review evaluated RCTs published from January 2010 through June 2026 and synthesized changes in pain and disability reported in individual studies. Despite advances in injection techniques and image guidance, the available evidence does not demonstrate consistent long-term benefits across ESI techniques. Differences in injection approaches, comparator interventions, outcome measures, and follow-up durations limit direct comparison of treatment effects, particularly for longer-term outcomes.

An important unresolved question is whether specific patient subsets benefit from specific drugs delivered by specific routes. Although formal subgroup analysis lies beyond the scope of this review, the included trials suggest several hypotheses. The only trial restricted to acute sciatica found a small leg-pain benefit over usual care. The trials enrolling patients with spinal stenosis showed small or no between-route differences [34], consistent with evidence that ESIs do not reduce short-term pain in stenosis [20]; in S1 radiculopathy, caudal and transforaminal dexamethasone were equally effective [39], and both trials favoring the parasagittal over the midline interlaminar approach used 80 mg methylprednisolone [32,37]. Moreover, most included trials compared ESI techniques with each other, which can indicate which technique performs better but not which patients benefit from ESI at all. The concept of patient-specific responsiveness, defined by symptom duration, aetiology, spinal level, route, and steroid agent and dose, warrants explicit attention in future trial design and cautions against applying aggregate findings uniformly to all patients.

Strengths

This review has several strengths. The literature search covered multiple major databases, and the review followed a predefined systematic approach to study selection and evidence synthesis. The authors assessed risk of bias using an established methodological framework and synthesized findings according to the characteristics and comparability of the available evidence.

Limitations

Several limitations should also be considered. Clinical and methodological heterogeneity across the included studies precluded calculation of a single overall pooled estimate. Differences in ESI techniques, comparator interventions, outcome definitions, follow-up periods, and reporting methods limited direct comparison across studies. The inconsistent use of diagnostic terminology across the included trials, with 'radiculopathy,' 'radiculitis,' and 'radicular pain' applied interchangeably, and the lack of consistent reporting of corticosteroid formulation represent additional limitations that affect the interpretability and comparability of the findings. Because imaging confirmation was not required for eligibility, some participants may have had non-compressive or extra-spinal causes of leg pain that are unlikely to respond to ESIs. Where quantitative pooling was not appropriate, we synthesized the findings using a SWiM approach. Evidence beyond six months was limited, reducing our ability to determine the durability of treatment effects. In addition, we cannot exclude publication bias because of the limited number and heterogeneity of the available studies.

Future research

Future research should focus on larger, well-designed randomized controlled trials with standardized outcome measures, clearly defined comparator interventions, and longer follow-up periods. Studies should report outcomes at clinically relevant time points, particularly beyond six months, to better establish the durability of treatment effects. Further research is also needed to determine which patient characteristics and ESI techniques are associated with clinically meaningful improvements in pain and disability. Future trials should clearly distinguish radiculopathy from radiculitis and radicular pain, specify and standardize the corticosteroid formulation used, including whether particulate or non-particulate steroids are administered, and identify patient subsets that may benefit from specific injection routes and steroid preparations. Standardizing intervention protocols and outcome reporting would enable more reliable comparisons across studies and improve the feasibility of future meta-analyses.

Conclusions

Epidural steroid injections may provide short-term improvement in pain and disability in some patients with lumbar radiculopathy; however, the magnitude and consistency of benefit relative to comparator interventions remain uncertain. The available long-term randomized evidence does not establish a consistent advantage of caudal, interlaminar, or transforaminal approaches. This review did not calculate a pooled average effect because of heterogeneity and incomplete reporting of comparable between-group outcomes. Future adequately powered, sham-controlled RCTs should use standardized, validated pain and function measures; report prespecified outcomes at common follow-up intervals, including six or more

months; and provide complete comparative effect estimates and safety outcomes.

Appendices

Section/Topic	Item #	Checklist item	Location where item is reported
TITLE			
Title	1	Identify the report as a systematic review.	Title: “Effectiveness of Epidural Steroid Injections in Lumbar Radiculopathy: A Systematic Review of Randomized Controlled Trials”
ABSTRACT			
Abstract	2	See the PRISMA 2020 for Abstracts checklist.	Abstract (unstructured; reports rationale, methods, RoB tool, number of RCTs, principal findings, and conclusion)
INTRODUCTION			
Rationale	3	Describe the rationale for the review in the context of existing knowledge.	Introduction & Background, paragraphs 1–6 (clinical background, natural history, ESI rationale, injection routes, timing of outcome assessment, evidence heterogeneity)
Objectives	4	Provide an explicit statement of the objective(s) or question(s) the review addresses.	Introduction & Background, final paragraph (“The review aimed to determine whether ESIs provide clinically meaningful and sustained improvements...”)
METHODS			
Eligibility criteria	5	Specify the inclusion and exclusion criteria for the review and how studies were grouped for the syntheses.	Methods, “Eligibility Criteria”; grouping for synthesis described in “Data Synthesis”
Information sources	6	Specify all databases, registers, websites, organisations, reference lists and other sources searched or consulted to identify studies. Specify the date when each source was last searched or consulted.	Methods, “Search Strategy” (PubMed, Scopus, Google Scholar; searched 1 Jan 2010–30 June 2026); Table 1. Note: trial registries and grey literature were not searched (stated as a limitation)
Search strategy	7	Present the full search strategies for all databases, registers and websites, including any filters and limits used.	Methods, “Search Strategy” and Table 1 (database-specific strings, filters, and record counts before/after filtering)
Selection process	8	Specify the methods used to decide whether a study met the inclusion criteria of the review, including how many reviewers screened each record and each report retrieved, whether they worked independently, and if applicable, details of automation tools used in the process.	Methods, “Study Selection”. Not reported: number of reviewers who screened titles/abstracts and full texts, and whether screening was independent (recommend adding)
Data collection process	9	Specify the methods used to collect data from reports, including how many reviewers collected data from each report, whether they worked independently, any processes for obtaining or confirming data from study investigators, and if applicable, details of automation tools used in the process.	Methods, “Data Extraction” (“Two reviewers independently extracted the data, and disagreements were resolved through discussion or consultation with a third reviewer”). Not reported: contact with study authors to obtain/confirm missing data
Data items	10a	List and define all outcomes for which data were sought. Specify whether all results that were compatible with each outcome domain in each study were sought, and if not, the methods used to decide which results to collect.	Methods, “Data Extraction” (pain: VAS/NRS; disability: ODI/MODQ/RMDQ; between-group effects, dispersion, 95% CI, p-values, change-from-baseline)
		List and define all other variables for which data were sought (e.g. participant and intervention	Methods, “Data Extraction” (study design, country, sample size, participant characteristics, intervention/comparator details, follow-up duration,

Section/Topic	Item #	Checklist item	Location where item is reported
Data items	10b	characteristics, funding sources). Describe any assumptions made about any missing or unclear information.	adverse events, crossover, additional procedures, surgery). Not reported. funding sources of included studies, or assumptions applied to missing/unclear data
Study risk of bias assessment	11	Specify the methods used to assess risk of bias in the included studies, including details of the tool(s) used, how many reviewers assessed each study and whether they worked independently, and if applicable, details of automation tools used in the process.	Methods, “Risk of Bias Assessment” (Cochrane RoB 2, assessed at outcome/result level; two reviewers independently, disagreements resolved with a third reviewer)
Effect measures	12	Specify for each outcome the effect measure(s) (e.g. risk ratio, mean difference) used in the synthesis or presentation of results.	Table 3 (mean difference, risk ratio, average treatment effect [ATE], and odds ratio reported per study, where available); footnote to Table 3 defines abbreviations
Synthesis methods	13a	Describe the processes used to decide which studies were eligible for each synthesis (e.g. tabulating study intervention characteristics and comparing against the planned groups for each synthesis, item #5).	Methods, “Data Synthesis” (studies categorised by injection technique, comparator, outcome domain, and follow-up period)
Synthesis methods	13b	Describe any methods required to prepare the data for presentation or synthesis, such as handling of missing summary statistics, or data conversions.	Table 3 notes where data were “not reported” (NR) rather than imputed. No formal method for handling missing summary statistics or scale conversion is described (recommend adding)
Synthesis methods	13c	Describe any methods used to tabulate or visually display results of individual studies and syntheses.	Table 2 (study characteristics), Table 3 (outcome data by group), Table 4 (RoB 2 domains); no forest plots or other visual synthesis displays were produced (meta-analysis not performed)
Synthesis methods	13d	Describe any methods used to synthesize results and provide a rationale for the choice(s). If meta-analysis was performed, describe the model(s), method(s) to identify statistical heterogeneity, and software used.	Methods, “Data Synthesis” (narrative synthesis per SWiM guidance; rationale: substantial clinical/methodological heterogeneity). Not applicable: no meta-analysis was performed, so no statistical model, heterogeneity statistic, or software is reported
Synthesis methods	13e	Describe any methods used to explore possible causes of heterogeneity among study results (e.g. subgroup analysis, meta-regression).	Not performed. Sources of heterogeneity are discussed narratively (Discussion, “Comparison of Epidural Injection Techniques”) but no subgroup analysis or meta-regression was conducted
Synthesis methods	13f	Describe any sensitivity analyses conducted to assess robustness of the synthesized results.	Not performed — not applicable, as no meta-analysis or pooled estimate was calculated
Reporting bias assessment	14	Describe any methods used to assess risk of bias due to missing results in a synthesis (arising from reporting biases).	No formal assessment (e.g. funnel plot, Egger’s test) was conducted. Addressed only narratively in Discussion, “Strengths and Limitations” (“publication bias cannot be excluded”)
Certainty assessment	15	Describe any methods used to assess certainty (or confidence) in the body of evidence for an outcome.	Not performed. No GRADE or equivalent certainty-of-evidence assessment was conducted (recommend adding or explicitly justifying its omission)
RESULTS			
Study selection	16a	Describe the results of the search and selection process, from the number of records identified in the search to the number of studies included in the review, ideally using a flow diagram.	Results, opening paragraph (195 records identified; 35 duplicates removed; 160 screened; 105 excluded; 55 sought and retrieved; 42 excluded at full text; 13 included) and Figure 1, PRISMA 2020 flow diagram
Study selection	16b	Cite studies that might appear to meet the inclusion criteria, but which were excluded, and explain why they were excluded.	Results, opening paragraph gives exclusion reasons by category and count (irrelevant intervention n=18; incomplete outcome data n=15; non-randomized design n=6; other n=3). Individual excluded studies are not cited by reference (recommend adding a supplementary excluded-studies list)
Study characteristics	17	Cite each included study and present its characteristics.	Results, “Characteristics of included randomized controlled trials” and Table 2 (design, comparison, sample size, follow-up); references 20–32

Section/Topic	Item #	Checklist item	Location where item is reported
Risk of bias in studies	18	Present assessments of risk of bias for each included study.	Results, “Quality assessment of included randomized controlled trials” and Table 4 (RoB 2 domain-level and overall judgements for each of the 13 studies)
Results of individual studies	19	For all outcomes, present, for each study: (a) summary statistics for each group (where appropriate) and (b) an effect estimate and its precision, ideally using structured tables or plots.	Table 3, “Quantitative outcome extraction for pain and disability” (group-specific baseline/follow-up values, between-group effect estimates, 95% CIs and p-values where reported)
Results of syntheses	20a	For each synthesis, briefly summarise the characteristics and risk of bias among contributing studies.	Discussion, opening paragraph and “Comparison of Epidural Injection Techniques” (narrative summary linking outcome patterns to RoB 2 findings)
Results of syntheses	20b	Present results of all statistical syntheses conducted. If meta-analysis was done, present the summary estimate and precision and measures of heterogeneity. If comparing groups, describe the direction of the effect.	Not applicable — no meta-analysis was performed. Narrative direction of effect by comparison is presented in Discussion and Table 3
Results of syntheses	20c	Present results of all investigations of possible causes of heterogeneity among study results.	Not performed (no formal subgroup analysis or meta-regression; see item 13e)
Results of syntheses	20d	Present results of all sensitivity analyses conducted to assess the robustness of the synthesized results.	Not applicable (no sensitivity analyses were conducted; see item 13f)
Reporting biases	21	Present assessments of risk of bias due to missing results (arising from reporting biases) for each synthesis assessed.	Not formally assessed; discussed narratively only in Discussion, “Strengths and Limitations”
Certainty of evidence	22	Present assessments of certainty (or confidence) in the body of evidence for each outcome assessed.	Not performed (see item 15). RoB 2 “some concerns” judgements are described narratively as a basis for cautious interpretation (Results, Table 4 narrative)
DISCUSSION			
Discussion	23a	Provide a general interpretation of the results in the context of other evidence.	Discussion, opening paragraph and “Comparison With Previous Reviews” (AAN review [33]; 2020 Cochrane review [34])
Discussion	23b	Discuss any limitations of the evidence included in the review.	Discussion, “Strengths and Limitations” (heterogeneity, limited long-term outcome data, publication bias)
Discussion	23c	Discuss any limitations of the review processes used.	Discussion, “Strengths and Limitations” (no meta-analysis performed; SWiM approach adopted). Note: absence of registered protocol, PRISMA checklist availability, and reporting-bias/certainty assessment are not explicitly discussed as process limitations (recommend adding)
Discussion	23d	Discuss implications of the results for practice, policy, and future research.	Discussion, “Clinical implication” and “Future Research Directions”
OTHER INFORMATION			
Registration and protocol	24a	Provide registration information for the review, including register name and registration number, or state that the review was not registered.	Methods, “Protocol and Reporting Standards” (“The review protocol was not prospectively registered in PROSPERO or another systematic review registry”)
Registration and protocol	24b	Indicate where the review protocol can be accessed, or state that a protocol was not prepared.	Not explicitly stated whether a written protocol was prepared but unregistered, or not prepared at all (recommend adding an explicit statement)
Registration and protocol	24c	Describe and explain any amendments to information provided at registration or in the protocol.	Not applicable (no registered protocol)
Support	25	Describe sources of financial or non-financial support for the review, and the role of the funders or sponsors in the review.	Not reported in the manuscript (recommend adding a funding statement, e.g. “No funding was received for this review”, if accurate)
Competing interests	26	Declare any competing interests of review authors.	Not reported in the manuscript (recommend adding an author competing-interests statement)

Section/Topic	Item #	Checklist item	Location where item is reported
Availability of data, code and other materials	27	Report which of the following are publicly available and where they can be found: template data collection forms; data extracted from included studies; data used for all analyses; analytic code; any other materials used in the review.	Not reported

TABLE 5: PRISMA checklist 2020.

PRISMA: Preferred Reporting Items for Systematic Reviews and Meta-Analyses.

Page MJ, McKenzie JE, Bossuyt PM, Boutron I, Hoffmann TC, Mulrow CD, et al. The PRISMA 2020 statement: an updated guideline for reporting systematic reviews. BMJ 2021;372:n71. doi: 10.1136/bmj.n71. This work is licensed under CC BY 4.0.

Additional Information

Author Contributions

All authors have reviewed the final version to be published and agreed to be accountable for all aspects of the work.

Concept and design: Jawad Khan, Marcus Malone

Acquisition, analysis, or interpretation of data: Jawad Khan, Marcus Malone

Drafting of the manuscript: Jawad Khan, Marcus Malone

Critical review of the manuscript for important intellectual content: Jawad Khan, Marcus Malone

Supervision: Jawad Khan

Disclosures

Conflicts of interest: In compliance with the ICMJE uniform disclosure form, all authors declare the following: **Payment/services info:** All authors have declared that no financial support was received from any organization for the submitted work. **Financial relationships:** All authors have declared that they have no financial relationships at present or within the previous three years with any organizations that might have an interest in the submitted work. **Other relationships:** All authors have declared that there are no other relationships or activities that could appear to have influenced the submitted work.

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