

Letters



**Request for Withdrawal of Publication of “Rapid Recommendations by Busse et al of Commonly Used Interventional Procedures for Non-Cancer Chronic Spine Pain: A Clinical Practice Guidelines”**

American Society of Interventional Pain Physicians®  
“The Voice of Interventional Pain Management”  
81 Lakeview Drive, Paducah, KY 42001  
Phone: (270) 554-9412 - Fax: (270) 554-5394  
[www.asipp.org](http://www.asipp.org)

---

April 16, 2025

Kamran Abbasi  
Editor in Chief, British Medical Journal  
[kabbasi@bmj.com](mailto:kabbasi@bmj.com)

Theodora Bloom  
Executive editor, British Medical Journal  
[tbloom@bmj.com](mailto:tbloom@bmj.com)

Peter Doshi  
Senior Editor, British Medical Journal  
[pdoshi@bmj.com](mailto:pdoshi@bmj.com)

Helen Macdonald  
Research integrity, British Medical Journal  
[hmacdonald@bmj.com](mailto:hmacdonald@bmj.com)

Tessa Richards  
Associate Editor, British Medical Journal  
[trichards@bmj.com](mailto:trichards@bmj.com)

Re: Request for withdrawal of publication of Rapid Recommendations by Busse et al of Commonly Used Interventional Procedures for Non-Cancer Chronic Spine Pain: A Clinical Practice Guidelines

Dear Editors:

We are writing to you—not as a Letter to the Editor for publication, but as a formal communication on behalf of the American Society of Interventional Pain Physicians (ASIPP), 48 state societies, and over 4,500 members. While this may be an unconventional request, we are asking for the withdrawal of the Clinical Practice Guideline authored by Busse et al (Commonly used interventional procedures for non-cancer chronic spine pain: A clinical



practice guideline. BMJ 2025;388:e079970). The network meta-analysis by Wang et al (Common interventional procedures for chronic non-cancer spine pain: A systematic review and network meta-analysis of randomised trials. BMJ 2025;388:e079971) from publication. Though we recognize this guideline shares similar limitations with others, we are specifically requesting the removal of this particular set of recommendations.

Our primary concern centers on the reliance on numerous systematic reviews that have produced conflicting conclusions. Despite this variability, the current guideline makes internationally significant recommendations based on a rapid review process—an approach that is inappropriate for decisions of such broad impact. This raises serious ethical concerns, which are widely shared among physicians specializing in pain medicine, as well as patients worldwide.

A crucial issue is the disparity in expertise. While the guideline seeks to eliminate interventional techniques, it does so from a perspective lacking the clinical depth and specialization of physicians who have undergone years of rigorous training, including residency and one to two-year fellowships in interventional pain management. It is troubling that recommendations with far-reaching implications may be driven by individuals with comparatively limited clinical experience, whose motivations may include academic or career advancement. Should these guidelines influence reimbursement policies, they risk jeopardizing the professional stability of highly trained specialists.

Most importantly, these guidelines threaten to restrict access to necessary treatments for patients living with chronic pain. The authors appear to undervalue patient-reported outcomes such as satisfaction and treatment efficacy—key considerations in real-world clinical practice. Patients are unlikely to accept or continue treatments that fail to provide relief. Similar recommendations have been made to eliminate opioid therapy.

We respectfully urge reconsideration and withdrawal of this guideline to protect both physician autonomy and patient care.

Furthermore, major organizations such as Medicare, Medicaid, and similar agencies worldwide have already developed procedural guidance based on the same body of evidence cited in the rapid review. Notably, several comprehensive Cochrane Reviews and assessments by the Agency for Healthcare Research and Quality (AHRQ), which utilized the same data, have never recommended dismantling the pain management specialty (Chou et al. Epidural corticosteroid injections for radiculopathy and spinal stenosis: A systematic review and meta-analysis. *Ann Intern Med* 2015;163:373-381; Oliveira et al. Epidural corticosteroid injections for lumbosacral radicular pain. *Cochrane Database Syst Rev* 2020; 4:CD013577; Manchikanti et al. Epidural injections for lumbar radiculopathy and spinal stenosis: A comparative systematic review and meta-analysis. *Pain Physician* 2016;19:E365-E410; and Manchikanti et al. Comparative systematic review and meta-analysis of Cochrane Review of epidural injections for lumbar radiculopathy or sciatica. *Pain Physician* 2022;25:E889-E916).

A publication in BMJ Evidence-Based Medicine by Garritty et al (Rapid reviews methods series: Assessing the appropriateness of conducting a rapid review. *BMJ Evid Based Med* 2025;30:55-60) who has also contributed to numerous related articles—clearly outlines when it is appropriate to conduct a rapid review. These criteria include:

1.     Urgent Decision Making:  
      There was no urgency in decision-making related to commonly used interventional techniques.
2.     Informing Guidelines:  
      No new or recent evidence warranted guideline updates. Instead, the authors independently generated new guidelines without a basis in emergent data.
3.     New or Emerging Technologies and Interventions:  
      None of the evaluated interventional techniques are new or emerging technologies.
4.     Rapidly Evolving Research Areas:  
      No such rapidly evolving research areas were identified.
5.     Identifying Evidence Gaps:  
      While evidence gaps exist across all specialties—including epidemiology and within rapid review methodologies themselves—these do not justify the issuance of practice guidelines.



6. Justify or Inform New Primary Research:  
There was no identified need for new primary research stemming from this review.
7. Resource Constraints:  
No resource limitations were cited as justification for using a rapid review method.
8. Time-Sensitive Opportunities:  
None were present. Numerous systematic reviews and guidelines already exist—many from Cochrane and AHRQ. Additionally, U.S. government agencies such as CMS have published Local Coverage Determinations (LCDs) or medical policies in each jurisdiction covering these procedures. Private insurers have also established their own medical policies.
9. Other Possible Scenarios:  
In some cases, rapid reviews may serve as a precursor to a systematic review to identify whether a more comprehensive review is warranted. However, no such need was demonstrated in this instance, yet guidance was still published.

Garritty et al (Rapid reviews methods series: Assessing the appropriateness of conducting a rapid review. *BMJ Evid Based Med* 2025;30:55-60) also outlined circumstances in which it is inappropriate to conduct a rapid review:

One key concern arises when researchers lack sufficient experience in conducting systematic reviews and instead choose a rapid review due to the perception that it is simpler or quicker. In reality, rapid reviews may be more complex than anticipated, and the use of accelerated methods can introduce significant bias. Even traditional systematic reviews are often met with scrutiny—conducting a rapid review without clear necessity only deepens skepticism about the validity of its conclusions.

A common but problematic motivation for pursuing a rapid review is the desire for quick publication, under the assumption that it requires less effort. This compromises both the rigor and the comprehensiveness of the review process.

This particular review and its resulting guidelines exemplify these issues precisely.

Most notably, the inappropriateness lies in the fact that multiple, up-to-date, full systematic reviews already exist on these topics, in addition to established guidelines and medical policies.

One of the strongest arguments against conducting a rapid review is when the evidence synthesis is intended to inform large-scale decision-making—such as international or regional guidelines—which may have far-reaching consequences in terms of resource allocation and clinical implementation.

In addition to methodological concerns, there is a substantial confluence of interest among the authors, as defined by the Institute of Medicine (Eden J, Levit L, Berg A, Morton S (eds); Committee on Standards for Systematic Reviews of Comparative Effectiveness Research; Institute of Medicine. *Finding What Works in Health Care. Standards for Systematic Reviews*. The National Academies Press, Washington, DC, 2011) and further described by Cappola and FitzGerald (Confluence, not conflict of interest: Name change necessary. *JAMA* 2015;314:1791-1792), raising further concerns about the objectivity of this guidance.

As you are aware, the Institute of Medicine (IOM) defines a conflict of interest as “a set of circumstances that creates a risk that professional judgment or actions regarding a primary interest will be unduly influenced by a secondary interest.” While financial conflicts of interest are the most commonly recognized, the IOM has also identified secondary interests—such as the pursuit of professional advancement, future funding opportunities, academic recognition, and personal favors for colleagues or collaborators—as equally relevant sources of potential bias.

Cappola and FitzGerald, along with the Institute for Translational Medicine and Therapeutics, have further expanded on this concept by introducing the idea of a confluence of interest—a broader and more nuanced framework than traditional conflict of interest. They argue that conflicts of interest represent a complex ecosystem that demands a consistent, standardized approach to reduce bias in clinical research across academic institutions. Importantly, they point out that the term “conflict of interest” is often viewed as pejorative, and that current disclosure



policies tend to focus narrowly on financial incentives. In contrast, within academic environments, the lure of prestige, influence, or future opportunities may, in some cases, be more compelling than financial gain.

In addition, there are numerous methodological shortcomings that have been raised and detailed through multiple letters. Given these substantial concerns, we believe that, in order to preserve the reputation of The BMJ and the MAGIC group, the most appropriate course of action would be to withdraw this publication. Ironically, this situation echoes the statement made in the first issue by Siemieniuk et al (Introduction to BMJ Rapid Recommendations. BMJ 2016 Sep 28;354:i5191), "Find a committee. Add evidence, opinion, politics, and money in varying measures, and a murky set of recommendations can emerge."

Should withdrawal not be possible, we kindly ask that you inform us so we may explore other avenues to address these concerns.

Thank you for your time and consideration. Please do not hesitate to reach out if any further clarification is needed.

Laxmaiah Manchikanti, MD  
Chairman of the Board and Chief Executive Officer, ASIPP and SIPMS  
Director, Pain Management Centers of America  
Ambulatory Surgery Center and Pain Care Surgery Center  
Clinical Professor  
Anesthesiology and Perioperative Medicine  
University of Louisville, Kentucky  
Professor of Anesthesiology-Research  
Department of Anesthesiology, School of Medicine  
LSU Health Sciences Center  
Shreveport, LA