In Reply:

We thank Dr. Tang for his comments on our article and agree that an anteroposterior view of the C1–C2 intraarticular facet injection would have provided valuable information regarding the trajectory of the needle in the mediolateral plane. As Dr. Tang points out, an anteroposterior view would have confirmed whether needle misplacement occurred, as suggested by the significant extraarticular spread of radiographic contrast visualized on the lateral image. Indeed, there was no anteroposterior image stored during the conduct of this procedure at the outside institution, thus we have no way of verifying the final needle position. Likely, this image would have demonstrated that the needle tip had deviated dangerously too lateral toward the course of the vertebral artery.

We much appreciate the thoughtful response from Drs. Datta and Manchikanti, and we agree with the recommendation to abandon the practice of intraarticular cervical injections, because the risk seems far out of balance from the scant demonstrated benefit from this procedure. The nonvascular complications of cervical injections that Drs. Datta and Manchikanti describe, including penetration into the subarachnoid space, nerve roots, and spinal cord, further reinforce our position regarding the risk–benefit ratio of this procedure. Even when nonparticulate steroids are used to prevent microvascular injury in the event of an inadvertent vertebral injection, nonvascular complications can still lead to devastating neurologic injury.

Drs. Datta and Manchikanti raise several important points regarding the nomenclature that is used to describe neuroanatomic landmarks in cervical injections. We chose the term “C1–C2 intraarticular injection,” as opposed to “lateral atlantoaxial joint injection” based on the document used in the procedure note and because both are frequently used in the published literature, but we do agree that the latter term is more common, particularly in recent publications.

Perhaps most important among their comments, Drs. Datta and Manchikanti raise procedural considerations that affect the interpretation of our report and the very safety of performing injection of the lateral atlantoaxial joint. They state, “...It is not clear from the case report if an anterolateral, lateral or posterior approach was employed ...” As shown in figure 1 of our report, the needle enters from a posterior approach. The posterior approach is well described and potentially the safest approach to the injection of the lateral atlantoaxial joint. Nonetheless, even if all appropriate safety measures are implemented, the risks of cervical injections of the lateral atlantoaxial joint are so devastating that they seem to outweigh the unproven benefits. Indeed, as Drs. Datta and Manchikanti suggest, “the best course of action may be to abandon the practice.”

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References


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Why No Casopitant-only Arm?

To the Editor:

In regard to the recent article by Singla et al. concerning the neurokinin-1 receptor antagonist casopitant, I have a num-
ber of concerns. It is not surprising that casopitant with ondansetron was superior to ondansetron alone; this certainly would not be a proof of efficacy unless one could compare ondansetron alone with casopitant alone. If the authors had any questions regarding the efficacy of this drug, I am very curious as to why they decided not to analyze the data of the casopitant-alone arm. Because the authors did explain why their study did not include a placebo-only arm, I believe that they also owe an explanation for why the casopitant-only arm was not included in the efficacy analysis. I do not believe that there are enough data on casopitant for the authors to assume primary efficacy of this drug and move on to look only at its efficacy when combined with a second drug. If the study’s primary purpose was dose finding for the prevention of postoperative emesis in the first 24 h, it is concerning that the only data analyzed were the data where casopitant was given with another antiemetic.

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Reference

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In Reply:
Ondansetron is currently generic, inexpensive, and considered by most to be safe with few side effects. Presently, consensus guidelines recommend that patients at high risk for postoperative nausea and vomiting should receive combination therapy. Therefore, this study was designed to examine the most likely clinical scenario (patients at high risk of postoperative nausea and vomiting receiving combination therapy). We do agree that a direct comparison between ondansetron and casopitant would provide valuable information. To that end, we did include the casopitant-only exploratory arm in the study. In fact, the article does already provide the 0- to 24-h complete response data (primary endpoint) for the casopitant-only arm. In this arm, 71 of 142 patients (50%) achieved the primary endpoint in contrast to 56 of 140 patients (40%) in the ondansetron-only arm. In the article, statistical tests were not performed on these data because the analysis would have been post hoc and violated our statistical plan.

As designed, the study was underpowered to evaluate single-agent casopitant versus ondansetron. However, a simple chi-square analysis comparing the casopitant-alone arm to the ondansetron-alone arm would have resulted in a P value of 0.09 favoring casopitant. This analysis has important methodologic flaws and as such inferences based on this P value should be drawn with care. Thank you for your letter; it raises an issue that was the subject of much discussion among the investigators during the design of the trial and the preparation of the manuscript.

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Reference

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