injections, because the risk seems far out of balance from the
demonstrated that the needle tip had deviated dangerously too lateral toward the course of the verte-
There was no anteroposterior image stored during the con-
radiographic contrast visualized on the lateral image. Indeed,
curred, as suggested by the significant extraarticular spread of
lateral plane. As Dr. Tang points out, an anteroposterior view
about the trajectory of the needle in the medial-
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 documen-
tation 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.2

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 anterolat-
eral, lateral or posterior approach was employed ...” As
shown in figure 1 of our report, the needle enters from a
posterior approach.1 The posterior approach is well de-
scribed and potentially the safest approach to the injection
of the lateral atlantoaxial joint.1 Nonetheless, even if all appro-
site 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.”

Brian L. Edlow, M.D., Brian J. Wainger, M.D., Ph.D.,
James P. Rathmell, M.D., Natalia S. Rost, M.D.* "J. Philip
Kistler Stroke Research Center, Massachusetts General Hos-
pital, Boston, Massachusetts. nrost@partners.org

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ment. Lancet Neurol 2009; 8:959–68
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from the lateral atlanto-axial (C1-2) joint. Cephalalgia 2002;
22:15–22

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In Reply:
We thank Dr. Tang for his comments on our article1 and
agree that an anteroposterior view of the C1–C2 intraartic-
ular facet injection would have provided valuable informa-
tion regarding the trajectory of the needle in the medial-
lateral plane. As Dr. Tang points out, an anteroposterior view
would have confirmed whether needle misplacement oc-
curred, as suggested by the significant extraarticular spread of
radiographic contrast visualized on the lateral image. Indeed,
there was no anteroposterior image stored during the con-
duct 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 de-
viated dangerously too lateral toward the course of the verte-
bral artery.

We much appreciate the thoughtful response from Drs.
Datta and Manchikanti, and we agree with the recommenda-
tions to abandon the practice of intraarticular cervical injec-
tions, because the risk seems far out of balance from the

Sukdeb Datta, M.D.,* Laxmaiah Manchikanti, M.D.,
*Vanderbilt University Medical Center, Nashville, Tennessee.
sdattamd@gmail.com

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facet steroid injection: Evidence for diffuse microvascular
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anatomical evaluation of the atlantoaxial transarticular screw
fixation technique: Case report and review of the literature.
J Neurosurg 1997; 86:961–8
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 suggest, “the best course of
action may be to abandon the practice.”

Brian L. Edlow, M.D., Brian J. Wainger, M.D., Ph.D.,
James P. Rathmell, M.D., Natalia S. Rost, M.D.* "J. Philip
Kistler Stroke Research Center, Massachusetts General Hos-
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22:15–22

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Why No Casopitant-only Arm?

To the Editor:
In regard to the recent article by Singla et al.1 concerning the
neurokinin-1 receptor antagonist casopitant, I have a num-

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224 Correspondence

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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.

Miriam B. Anixter, M.D., Children’s Hospital of Pittsburgh of the University of Pittsburgh Medical Center, Pittsburgh, Pennsylvania. anixterm@anes.upmc.edu

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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.

Neil K. Singla, M.D.*, Frances Chung, F.R.C.P. "Lotus Clinical Research, Inc., Pasadena, California. neil@lotuscr.com

Reference

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