

## Response to “Branch Blockade of the Dorsal Sacral Rami”

Dear Editor,

We thank the authors for their interest in our paper [1], although we have difficulty detecting their purpose. However, they do provide us with an opportunity to discuss and clarify anatomical issues as they affect experimental methodology.

The letter opens with a declaration that “some questions arise out of the described methodology.” Yet, we can find little in the letter that pertains to our methods. Rather, the letter appears to be a précis of a study conducted by the authors that is tangentially related to our own study.

The authors offer a description of the dorsal sacral plexus, but we are already familiar with this, and it is not germane to the methodology, which is the stated concern of the authors. Nor is the anatomy of the medial branches of the sacral dorsal rami relevant to our study, which explicitly addressed lateral branch blocks.

With respect to the technique that we used, the authors raise the speculation *ad hoc* that injectate may have leaked to the dorsal ramus itself. In the first instance, our previous studies of sacral lateral branch blocks [2] found no evidence of this phenomenon. In the second instance, spread to the dorsal ramus makes no difference to the clinical effect of the block. There is nothing that the dorsal ramus does with respect to the sacroiliac joint that is not already done by the lateral branches. The authors go on to state that “this might account for the persistence of sensation of sacroiliac joint capsular distension”; but the authors do not explain how this might be so. They argue that “in such cases the lateral branch blockade might be incomplete”; but it cannot be incomplete if the dorsal ramus is blocked, because the dorsal ramus contains the afferents contained in the lateral branches.

The authors recount that we excluded certain patients from our study. Their attempted criticism seems to imply that we should have studied all comers. This contention conflicts with opposing concerns. Had we complied with the authors’ suggestion, others would have complained that we included potentially confounding patients, who had leakage of local anaesthetic to inappropriate sites. It is a requirement for validation studies that the sample be homogeneous, and free of confounding influences. In

other words, we are obliged to study the best cases in order to get a clear answer to the question posed. The virtue of our methods is that we used placebo-controls, in a randomized sample. Under those conditions, the “purity” of our subjects is equally distributed between the samples, and with no confounding effects, only one question is answered: did the active agent have an attributable effect?

Otherwise, the comments about defects in the sacroiliac joint and about tracking out of the joint to other sites are immaterial to our study. Those phenomena might be relevant to a study of intra-articular blocks, but they are not relevant to our study, which explicitly focused on lateral branch blocks.

Although we fail to see how the material raised by the authors challenges or discredits our methodology and our conclusions, we appreciate their interest and attention to these important questions and our opportunity to discuss them further.

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### References

- 1 Dreyfuss P, Henning T, Malladi N, Godlstein B, Bogduk N. The ability of multi-site, multi-depth sacral lateral branch blocks to anesthetize the sacroiliac joint complex. *Pain Med* 2009;10:679–88.
- 2 Dreyfuss P, Snyder BD, Park K, et al. The ability of single site, single depth sacral lateral branch blocks to anesthetize the sacroiliac joint complex. *Pain Med* 2008;9:844–50.