Reviewer's comment concerning “the percutaneous stabilization of the sacroiliac joint with hollow modular anchorage screws: a prospective outcome study” (ESJO-D-12-01011R1 by Lyndon W. Mason, Iqroop Chopra and Khitish Mohanty, doi:10.1007/s00586-013-2825-2)

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The presented article reports the result of treatment of SI joint pain with a minimal invasive surgical technique for SI joint stabilization/fusion using a hollow screw and demineralized bone matrix as fusion material.

This is a prospective study on 74 patients, 75 % of whom were available for follow-up. The indication for treatment was SI joint pain for different pathologies; 40 % of patients had a failed back surgery syndrome. The conclusion of the paper states, “percutaneous iliosacral screw fixation is a very successful procedure in improving both the pain and function in patients with SI joint disease”.

The study results presented appear solid and sound based on a 75 % follow-up rate but concerns prompted the reviewer to add a comment.

The first reason: the authors describe a new minimal invasive technique that might encourage people to perform surgeries on a badly defined pain syndrome. Certainly, there remains a controversy about the indication—SI joint pain remains an obscure symptom. Too often spine surgeons blame it for failed spine interventions! As the authors state, they used all possible diagnostic tests but their key test for the indication was related on a positive, CT-guided injection. The literature provides so many viewpoints on this topic as a final conclusion seems not possible. The authors underline to be very selective for the indication.

Furthermore, a technical aspect appears to be of great importance. The presented screw fixation appears very elegant; however, the presence of a bony fusion cannot be verified due to lack of information. To promote a new surgical technique for fusion procedure, CT imaging would be necessary to ascertain technical success, beyond the clinical outcome. Questions arise from a mechanical point of view as the cross section appears minimal in relation to the whole surface of the SI joint and furthermore, the use of demineralized bone matrix (DBM) to induce fusion. Having in mind the poor performance of DBM in interbody fusion with empty cages 2 years postoperatively, there are fair doubts about the fusion. The argument of radiation exposure for a CT control is certainly valid. On the other hand, imagining that many unnecessary surgeries could be performed as a result of this article, which might lead to many secondary imaging, it could have weighed out this conflict. The authors emphasize that their study is focused on clinical outcome and this was accepted finally.

Hopefully, the increasing numbers of reports on the treatment for SI joint problems will not misguide surgeons to perform uncontrolled interventions and I wish we do not have to state (following Nachemson) that too many SI joints are damaged by surgeons for undefined pain problems.

Conflict of interest None.

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