Comment on “Long-term actuarial survivorship analysis of an interspinous stabilization system” (J. Sngas et al.)

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This paper evaluates the survivorship of interspinous implants implanted in association with a decompression procedure of the lumbar spine, either discectomy or fenestration, at a mean 14-years' follow-up. It gives important information about the safety of the procedure concerning possible short and long-term complications linked to the device itself. It is also noteworthy that only 10% of the patients secondarily needed adjacent-level surgery.

However, this study has a number of shortcomings.

Only 59% of the patients were available for follow-up and the information about removal of the device or other subsequent operation at the operated or at an adjacent level was obtained through a telephone interview, information being obtained through close relatives in the case of patient's death. It is common in daily practice to observe that patients do not always fully understand the nature of an operation they undergo, if the information about the removal or not of the implant is probably reliable the exact nature of another surgical procedure and the level of this procedure may be questioned.

The authors state that patients who underwent the placement of an interspinous implant would have been scheduled for a fusion in association with the decompression procedure, the indication being instability or associated low back pain. They report outcomes at least equal to those of fusion, which is understood by the reader as

clinical outcome, including at least partial relief of low back pain if present initially. On two occasions, the notion of outcome in the text clearly not only relates to survival of the implants. However, there is very little information on the clinical status of the patients preoperatively and at last follow-up regarding low back pain. Even regarding the preservation of stability at the instrumented level, outcome assessment should include systematic preoperative, post-operative and long-term follow-up radiographs, ideally with dynamic views. The long-term preservation or restoration of sagittal lumbar alignment would have interestingly been documented by radiographs in a significant number of patients.

Regarding the rate of reoperation at long term for adjacent level problems, a 10% rate is encouraging; however, it must be balanced with another 11% reoperation rate at the level of the index procedure, which is probably a much higher reoperation rate than after fusion. Overall, the need for another operation, at the same level or at an adjacent level, does not appear to be very different than after initial fusion; however, a number of secondary operations could have been less invasive than after fusion procedures.

In conclusion, I believe this paper gives important information about the safety of implanting an interspinous device of the Wallis type, about the absence of significant complications related to the implant and the technique themselves, but it gives no undebatable information on the indications to associate such a procedure with a decompression operation, neither does it permit to truly compare the result on low back pain after a fusion. The indication of a stabilisation procedure when performing a discectomy or a laminectomy remains a matter of debate; the safety of an interspinous implant when compared to an instrumented

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fusion does not allow the surgeon to supplement his indications for an associated stabilisation procedure in these circumstances. Moreover the results of this paper certainly do not support the indication of using such a device to treat low back pain *per se*.

To perfectly ascertain the pertinence of interspinous devices, randomised prospective studies comparing patients with decompression alone, decompression with dynamic stabilisation and decompression with fusion (posterior or interbody?), are required.