Editorial

# Evaluating post-operative pain management at the iliac crest bone graft site: an editorial

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As academicians and spine surgeons, we appreciate the effort and time required to plan and execute a randomized clinical trial (RCT). Given the current medical climate and importance of evidence-based medicine, the RCT is the gold-standard and for this reason alone, we applaud the authors, Samartizis *et al*. (1) in their pursuit of evidence-based knowledge. To summarize, Samartizis *et al*. aimed to examine the effect of postoperative pain management at the iliac crest bone graft (ICBG) site in patients undergoing surgery for adolescent idiopathic scoliosis (AIS). The goal was to assess the ability to affect post-operative pain in patients undergoing ICBG for scoliosis surgery. They performed a randomized, prospective study in which the treatment group was given a continuous local anesthetic infusion at the ICBG site while the control group received a continuous infusion of saline. The infusions were administered for 47 hours. There were five patients in the treatment group and seven patients in the control group. There were no differences in demographic between the two groups. There were no differences in regards to type of curve, levels fused, or length of hospitalization. They recorded changes in pain according the visual analog scale (VAS) at the surgical site, ICBG site, and contralateral ICBG site. Pain was recorded until post-operative day 4, at which time the patients were discharged.

The authors noted that there was not a statistically significant difference ($P>0.05$) at any time interval in regards to surgical site pain. This was expected as the patients had similar procedures with similar demographics. There was a two-fold decrease in ICBG (donor site) pain and contralateral ICBG (non-donor site) pain in the treatment group in the first two days after surgery, but these differences were not statistically significant. Additionally, there was normalization of the pain scores between the groups after the interventions were discontinued.

We are proponents of ICBG and use it routinely in our practice (2). Thus, when asked to give a perspective on the article by Samartizis *et al*., we agreed to enthusiastically. First, allow us to address the science. The study is a well-designed RCT. However, without statistical power, the results are unfortunately meaningless trends. The authors acknowledge this weakness and label the underpowered RCT a pilot study, yet give the reader the impression that the results are meaningful.

Secondly, if this is a pilot study, we might recommend that the study be abandoned altogether. The expected results based on the pilot are that infusion of an anesthetic in the donor site of the graft harvest area will decrease immediate donor site pain for 2–3 days. We would agree. Just as we would agree that making an incision in a patient would be less painful after the injection of an anesthetic than with saline. Anesthetics are useful for pain control during surgical procedures and during the perioperative period. Thus, if the RCT is completed with enough patients to allow for statistical power, it would seem to be an enormous undertaking to show what seems to be intuitively obvious: infusion of an anesthetic in a graft harvest site for 47 hours after surgery decreases immediate donor site pain for a 2–3 days. There are several well-established medical interventions long held to be highly effective based on experience that predate the availability of evidence-based analysis, and we would include the use of local anesthetic for pain control as one of them.

The third point has to do with the issue of ICBG

donor site pain. Historically, the primary criticism of using ICBG has been association with chronic donor site pain. The literature that is frequently cited regarding the morbidity of ICBG harvest consists of level III and IV data. In a retrospective review and mail survey, Banwart *et al*. (2) reported a 10% major (18 of 180) complication rate, three of which affected function secondary to pain. Fernyhough *et al*. (3) reported that twice as many donor sites harvested for reconstructive spinal procedures were reported as having chronic pain as compared with those harvested for spinal trauma (39% *vs*. 18%), concluding that donor site pain was more dependent on the pre-operative diagnosis than on surgical approach used to harvest the graft (primary midline incision *vs*. separate incision). However, there was no data regarding how many of the reconstructive spinal procedure patients had posterior iliac crest pain pre-operatively. This is a variable (pre-existing posterior iliac crest pain) that must be taken into account. In a study comparing short segment spine fusion in which ICBG was harvested in 53 patients and recombinant human bone morphogenetic protein-2 (rhBMP-2) was used in 59 patients, Howard *et al*. (4) reported that the incidence of posterior iliac crest pain at an average of two years after surgery was similar in both groups. It would be useful to assess the incidence of posterior iliac crest pain after harvest in a subgroup of patients without pre-existing pain. This makes the AIS subgroup an interesting patient population to assess. Interestingly, Lansford *et al*. (5) in comparing different bone grafting techniques for AIS with a minimum of four year follow-up found that adding autograft to allograft was no better than allograft alone. Additionally, there was no difference in the pain scores between the two groups. Although no specific measurement of donor site pain was administered, chronic donor site pain presumably would have been reflected in worsening pain scores for the autograft group, which was not the case. In the case of Samartizis *et al*. (1), the infusion of anesthetic for 47 hours does not address the issue of chronic pain, emphasized by the normalization of the VAS measurement by post operative day 4 and the short duration of follow-up.

Lastly, the use of autograft harvested from the posterior iliac crest for AIS may also limit the population of spine surgeons who would find Samartizis *et al*. (1) study clinically useful. The use of allograft exclusively in the AIS population is well supported with over two decades of practice backed by peer-reviewed manuscripts (5-9). With evidence that allograft has equivalent fusion rates to autograft in this population, the decision to proceed with harvesting ICBG would be primarily surgeon preference or regional availability of suitable allograft. Otherwise, it would seem that exposing the patient to the potential harvest site morbidity isn't worth the benefit of having autograft bone.

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## Footnote

*Conflicts of Interest:* The authors have no conflicts of interest to declare.

*Comment on:* Samartzis D, Bow C, Cheung JP, *et al*. Efficacy of Postoperative Pain Management Using Continuous Local Anesthetic Infusion at the Iliac Crest Bone Graft Site in Patients with Adolescent Idiopathic Scoliosis: A Parallel, Double-Blinded, Randomized Controlled Pilot Trial. Global Spine J 2016;6:220-8.

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