Letter to the Editor

A New Fixation Technique for the Lapidus Bunionectomy

To the Editor:

As a foot and ankle surgeon who performs the Lapidus bunionectomy, I find the study reported by James C. Wang, DPM, and Brendan M. Riley, DPM, published in the July/August 2005 issue of the *Journal* ("A New Fixation Technique for the Lapidus Bunionectomy") has clinical relevance, but the conclusions drawn are suspect.

In the "Materials and Methods" section, Drs. Wang and Riley describe the population of interest; however, there is no mention of the training, skills, and experience of the involved surgeons. The authors state that the "minirail external fixator was applied," but there is no mention of the name and model number, manufacturer's name and location, and function and technical specifications of the external fixator used. Photographs show an Orthofix minirail external fixator. It would have been ideal for the authors to declare whether there was any conflict of interest or financial relationship with the manufacturer of the product used in this study. It makes one wonder whether one or both of the authors have a consulting arrangement with the manufacturer.

There is an inherent selection bias conveyed in the "Materials and Methods" section. The authors state that "each patient was given the choice of either traditional internal fixation with a prolonged nonweightbearing postoperative course or external fixation with earlier weightbearing." This type of language is sure to influence a patient population in one direction, and it also sets up an expectation. What patient would not choose the method that allows for weightbearing? What is the purpose of offering any patient a choice when the arms are not blinded and are so diametrically appealing?

The authors claim that "the average time to fusion was 5.3 weeks." This implies that there were some patients who "fused" well before this time. Is this fusion time physiologically possible? The authors report that the external fixator was removed at an average of 5.5 weeks. Again, there is an implication that some patients had the external fixator removed well before 5.5 weeks. Unfortunately, the authors did not report the range of either the time to fusion or the length of time the external fixator remained in place.

The authors go on to claim that "the use of external fixation allows . . . the opportunity for earlier weightbearing *and fusion* than with traditional fixation methods" (italics added). Earlier fusion? The primary bone callus phase lasts from 6 to 10 weeks and includes condensation of the fibrocartilaginous callus into bone that bridges the fracture interface. The process of haversian remodeling has been well demonstrated, and the cutting cone advances at a known rate with regeneration of about 60% of the osteons at approximately 8 weeks.[1] How the use of the external fixator speeds up this natural physiologic process remains distinctly unclear. In addition, the lack of follow-up past the 5.5-week mark seems to ignore the possibility of a late manifestation of nonunion.

In this study of 102 patients—or any study involving this many patients—the absence of a single delayed union or nonunion is highly suspect, regardless of the fixation technique used. The reported nonunion rate for the Lapidus procedure varies from 3% to as high as 12%.[2-8] Can the authors really expect the reader to believe that the union rate was 100%?

In light of the comments above, the conclusion that this study "has shown external fixation to be a viable alternative to traditional fixation methods that does not compromise the fusion process" simply cannot be drawn. Some practitioners may attempt this reported procedure believing it has value, based on the fact that it was published. A higher standard must be upheld and encouraged.

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