LETTER TO THE EDITOR


Although it is a bit late, I would like to respond to the article published in issue No. 7 (November 2012) of Orthopaedics and Traumatology: Surgery and Research [1].

Although the author did me the honor of quoting my article which appeared in Foot and Ankle Surgery [2] unfortunately he misrepresented the last paragraph before the conclusion in relation to recurrence.

The author wrote: “Vogt, Iamamoto and Ohl reported respectively 17.8 and 26% of recurrence”. In fact, although I did indeed quote those figures in my article [2] they were the recurrence rates reported in the paper by G. Curvale concerning the Bardot technique [3].

As you will see if you read my article, the complication rate of my series was 4.5%, or 6/132 cases due to early detachment or a late tear of the transferred tendon.

In the section of the article on surgical technique, I clearly state that in case of fragile bone, the tendon transfer should go around the peroneus brevis tendon before passing through the bone tunnel.

The bone tunnel technique makes it possible to precisely control the tension of the transferred tendon whose length is different from one patient to another. Therefore this is still a pertinent technique.

The author reports 2/22 cases of recurrence or nearly 10% and one case of detachment of the anchor that was attached to the fifth metatarsal with no effect on foot stability. This complication rate is therefore higher than that in my article.

There are also other remarks that can be made in relation to the study by Gasse et al.

In relation to the lengthening of the triceps surae, Baker’s theory that it is possible to perform selective

lengthening of the soleus or the gemellus was not confirmed by the electromyographic study by J. Perry [4], which showed the influence of postural tonus, and that muscular activity was reduced during knee flexion both in the soleus and in the gemellus muscles and was markedly increased in both muscular groups during standing. These results suggest that selective lengthening should not be proposed in spastic patients with equinus foot deformity.

In relation to the postoperative protocol, there is no reason not to allow weight-bearing for the first three weeks, because these hemiplegic patients are incapable of taking weight off the operated side, which changes the quality of life in the first weeks after surgery, forcing them to use a wheel chair.

Finally, Gasse et al. did not mention the indispensable therapeutic measures necessary to treat hammer toes which most authors and myself consider to be the main complaint in patients treated for spastic foot.

Disclosure of interest

The author declares that he has no conflicts of interest concerning this article.

References


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