Platelet-rich plasma treatment improves outcomes for chronic proximal hamstring injuries in an athletic population

Angelo Cacchio

Department of life, health and environmental sciences, University of L'Aquila, Italy

Dear Editor,

it was with great interest that we read the article by Fader et al.1: “Platelet-rich plasma treatment improves outcomes for chronic proximal hamstring injuries in an athletic population”, which appeared on Muscle, Ligaments and Tendons Journal 4(4) 2014, and we commend with the authors for their effort into the research for new treatments for patients with proximal hamstring tendinopathy. However, as stated by the Authors in the introduction section (page 462): “A recently published study by Cacchio et al. described shockwave therapy ... This study, though promising, is limited by a short-term follow-up of only 3 months”7. In fact, our RCT study included a follow-up to 12 months. I would like to invite the Authors to be more careful in their statements, but also the Reviewers in verifying its accuracy.

Moreover I would like the Reviewer to accurately verify the methodological quality of the manuscripts who review. In this manuscript by Fader et al.1 there is a lack of scientific methodology. There is a lack of data: i) final data of the VAS scale (there is only a statistical increase for some patients); ii) initial and final data of the mentioned “questionnaire”; iii) complete absence of patients’ clinical data. Moreover, there is a complete absence of statistical analysis. Although this is a preliminary work, some basic scientific procedures should be respected. So that, the conclusive statements of the Authors on the therapeutic procedure that their used in his study, are absolutely speculative, and not supported by data and their scientific analysis.

References


Letter to the editor

Dear Dr. Cacchio,

We appreciate your interest in our work on proximal hamstring tendinopathy, and commentary regarding our study “Platelet-rich plasma treatment improves outcomes for chronic proximal hamstring injuries in an athletic population”. We apologize for any confusion that may have occurred in the interpretation of your data from the randomized controlled trial entitled “Shockwave Therapy for the Treatment of Chronic Proximal Hamstring Tendinopathy in Professional Athletes”. When reviewing your results, all commentary in the text for both the primary outcome measures (Pain, NPRS), as well as the secondary outcome measures, specifically and only addressed the results at 3 months, with no comment on the outcomes at 6 and 12 months for any endpoint. This was the origin of our misinterpretation, and we apologize for that.

Furthermore, we appreciate your review and comments on our technique, methodology, and limitations. We were very aware at the time of publication that the strength of this paper is limited by a small cohort, lack of control, and short term follow up, and we did attempt to address these in the “Limitations” section of the paper. Undoubtedly, there are ample clinical measurements, such as strength or specific clinical tests, which could have been obtained and incorporated. As you stated very nicely in your paper, there is “no validated disease-specific questionnaires available for PHT”. Therefore, we also chose pain severity (VAS) and recovery (our return to activity and patient-perceived recovery questionnaire) as our outcome measures. We appreciate your interest and commentary, and hope this can stimulate further investigation into both PRP and proximal hamstring injuries in the future.

References