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Letter to Editor

Letter to the Editor regarding "Comparison of High-intensity Laser Therapy with Extracorporeal Shock Wave Therapy in the Treatment of Patients with Plantar Fasciitis: A Double-blind Randomized Clinical Trial"

Dear Editor

We recently have read with great interest an article entitled "Comparison of High-intensity Laser Therapy with Extracorporeal Shock Wave Therapy in the Treatment of Patients with Plantar Fasciitis: A Double-blind Randomized Clinical Trial", by Zare Bidoki and colleagues, which was published in your esteemed journal (IJMS Volume 49, Issue 3, March 2024).[1] They conducted a randomized clinical trial designed to assess the efficacy of high-intensity laser therapy (HILT) versus extracorporeal shock wave therapy (ESWT) in the treatment of plantar fasciitis (PF). The authors' extensive examination showed that ESWT and HILT reduced pain and enhanced patient satisfaction in individuals with PF. Of these, HILT was preferred because it was more effective in relieving pain and quality of life, as well as being more accessible, less painful, and less expensive. We acknowledge the authors for their valuable contribution in evaluating the efficacy of ESWT and HILT therapies for the treatment of this disease. However, following a comprehensive discussion with our panel of professional peers, we have some pertinent questions, which we hope you will address:

1. Plantar fasciitis is the most frequent cause of heel pain, estimated to affect up to 7% of the general population, with bilateral involvement in 20-30% of patients.[2] However, the uni-bilateral prevalence of plantar fasciitis was not addressed in the inclusion and exclusion criteria of the study. In this case of non-uniform prevalence, patients were assigned randomly rather than the affected foot. This could have led to the existence of individual differences and varying adherence to physiotherapy, resulting in significant differences in treatment and ultimately affecting the accuracy of the experiment. Therefore, it would be helpful to provide more details so that the reader could have a better understanding.

2. This study assessed pain levels by using the visual analog scale (VAS) without providing the reader with the patient's state, such as rest, first step, or activity. PF is frequently associated with alterations in the arch, foot shape, and hindfoot posture, and there are disparities in plantar pressure distribution and hindfoot postural alignment between the two when standing or walking, which results in varied degrees of pain.[3] Additionally, patients have reported that first-step pain was worsened after standing up from bed or sitting for a prolonged period of time.[4] Given this situation, we are concerned that inconsistencies in patient status during pain evaluations might influence the primary outcome assessment in this study. Therefore, I am eager to get detailed clarification from the authors regarding this issue, which has the potential to significantly influence the measurement of pain outcomes.

3. I think there might be some disagreement with the author regarding the use of insoles. The authors believed that insoles should only be used when necessary, such as in patients with flat or arched feet or other foot structural disorders. However, in Wu's study, it was found that PF might become structurally weaker in the plantar foot during ESWT treatment.[5] If patients return to their previous activity level, symptoms may recur quickly if too much pressure is applied to the PF without adequate protection. Therefore, we recommend that patients should be instructed to wear insoles following ESWT and to avoid applying excessive pressure on the heel for at least 1 month.

4. Both the VAS and Foot Function Index (FFI) assessments rely mostly on patient-reported outcomes or clinical assessments. This subjectivity adds to the possibility of bias and heterogeneity in

symptom interpretation. It is worth noting that imaging is generally more reliable than health-related pain intensity. Plantar fascia thickness is one of the indicators of plantar fasciitis, with thicknesses greater than 4 mm considered a sign of this condition. In a 12-month longitudinal follow-up study, PF thickness and stiffness were assessed using B-mode ultrasound and strain hyperelastography. It was found that patients with PF experienced a progressive decrease in plantar fascia thickness after ESWT.[5] In a study of the same type, where ultrasound-measured fascia thickness was introduced as an indicator for assessing the efficacy of ESWT, statistically significant differences in final fascia thickness were found.[6] Therefore, it might be better to evaluate the therapeutic efficacy of ESWT in combination with changes in plantar fascia thickness.

In conclusion, we again thank the authors for their contribution, since this study provides a more comprehensive theoretical basis for the treatment of PF. However, a more nuanced discussion and cautious conclusions are required when assessing the efficacy of ESWT or HILT, combined with an in-depth analysis of various potential confounders to enhance the clinical applicability of this study. We hope that this letter will assist researchers in improving the design of subsequent studies, as well as readers in better understanding the findings of this research.

Statement from IJMS: The response letter from Dr. Zare Bidoki and colleagues will be published whenever received by the Journal.

Authors' Contribution

Yuhan Gong: Writing – original draft. Xinjie Wang: Writing – original draft, Writing review & editing. All authors read and approved the final version of the manuscript.

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