

Dear reviewer,

first we would like to thank you for the review, the comments and the remarks you have made. Through your constructive comments, the quality of our brief report could be substantially increased. The corresponding answers and comments are listed below. Please let us know, if there is anything else we can do.

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How bias are reduced?

Thank you for pointing this out. In the context of the present study, this is an absolutely justified question. The investigation with pilot study design should first give us an overview before aiming for a high-quality RC study. In order to reduce bias, a clear and unambiguous language was used. Only people who had been recommended a SMI by an expert in the field were included. Based on your request, we would like to add a further limitation to the work:

- Selection bias: Since only people with insole recommendations were the subject of the study, it can of course also be assumed that the sample in general has a rather positive attitude towards insoles, which may influence the success of the therapy.
- Questionnaire: A validated questionnaire should be added to the next study.

Why the authors choose a one-arm design? How the authors can separate a simple placebo or temporal effect with the insole effect?

→ The choice of design is based on the approach to conduct a first pilot study in the context of the effect of sensorimotor insoles on subjective pain perception. In any case, the authors agree with your opinion that in the following randomized control study placebo effects must be controlled by means of a control group. Both points mentioned are limitations of the study, which were also evaluated as such (e.g. lines 272 – 273) and reduce the significance of the results.

More information: What about the other results? (e.g. indication, age, gender effects)

→ The authors also discussed that point. As described in the results section, there were no differences between the different indications. Concerning age and gender effect no differences on the reduction of subjective pain perception could be identified during the data analysis and statistical calculations, the authors decided not to emphasize these aspects further. Also due to the already existing limitations of the study, in our opinion the data analysis should not be extended. The following RC Study should hopefully be able to show a more representative and detailed overview.

Thanks to your question we added this point in the corresponding paragraph (lines: 259-260).

Last, the discussion section is very rich but weakly connected to the results; it tends to be more like a bibliographical discussion. I suggest synthesizing more. In any case, the authors ought to comment on the limitations of the study (including of course the choice of a one-arm design).

→ Thank you for the comment, which we appreciate very much. We tried to adjust the connection to the actual results and synthesized a few paragraphs. Nevertheless, due to the fact that scientific studies on SMI are lagging and we found that many studies missed to precisely explain the effects of the insole spots in the context of the medical/therapeutical target, we saw an added value in discussing the possible known effects from different studies on disease patterns in more detail. We rather want to use these results to point out the missing investigations in the sector and to hint at

the potentials in this orthopedic/scientific sector. Please let us know if you don't agree and if there is anything else we can do.

page 2 line 62: "directional pressure". This vocabulary is not suitable as the pressure is a scalar and thus directionless

→ Thank you for pointing out this biomechanical language mistake. We clarified this sentence (see: lines 62 -63).

page 2 line 71-72: [23] suggests the mechanism in the discussion part, but the level of proof is very weak considering (1) the number of patients (2) the study design that is not able to separate proprioceptive and biomechanical effects. May the authors be more cautious in this assessment.

→ Thank you for this valuable comment. The underlying study by Mabuchi and colleagues (2012) shows a good approach with a small number of subjects, but definitely its limitations. However, it is one of the few studies that looked at the effect of insoles on the rotational behavior of the lower extremity. Therefore, the statement has now been correctly qualified to a certain extent (see: line 72)

page 4 line 143: Seabor should be Seaborn instead?

→ Thank you for revealing the error. It has been corrected.

page 5 line 175-176: the question of cause/consequence between pain reduction and joint kinematics is unclear. Even though the authors write "As a result, the corrected foot position and a better foot pressure distribution can lead to a reduction in foot pain", this could also be the case in the other direction.

→ The authors agree with your comment. The fabrication and fitting were always accompanied by a trained orthopedic shoe technician, whose task is to work along medical guidelines and to effect a change in foot position and joint kinematics only to the extent that a reduction in pain can be achieved. Nevertheless, a change can of course also lead to an increase in discomfort or even pain. This has now been taken into account (lines: 182-185).