Author's response to reviews

Title: A new brace treatment similar for adolescent scoliosis and kyphosis based on restoration of thoracolumbar lordosis. Radiological and subjective clinical results after at least one year of treatment.

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Author's response to reviews: see over
Dear prof. Grivas, dear Editors

In the now submitted version of our manuscript the changes and in our opinion needed comments on the reviewers remarks are highlighted.

In the last cover letter of September, hereunder copied, we could answer or comment the most of the given remarks.

It is not of interest of the reader in our opinion to get involved in controversies, but in this submission we tried to diminish this.

In our opinion the ms. does comply to journal style. The language is checked again.

The chance of focus we made from Dickson’s point of view (the thoracic curve) is only different from that of HR. Weiss in that we stress the existence of a thoracolumbar kyphosis as a first sign of deteriorations of the upright spine.

The remark on the difference we gave TLI and the Physiologic brace in sagittal forces in table 3, we can explain that in TLI progressive forces are delivered in a rigid ring structure, whereas the Physiologic cannot give too much forces because of the loose connection by straps between the two parts.

We cannot stress enough, that the period of treatment described here is short, but that also the combination of different biomechanical pathogenesis and a different and more dynamic design (seen in time) makes a true comparison with results of other techniques difficult.

As soon as you put some etiologic factors like external or environmental factors in front in otherwise idiopathic deformations, you get in conflict with contemporary idea’s on discussing brace treatment as in the SRS criteria.

We do hope the changes and corrections can be accepted for publishing,

Many thanks for the patience and guidance,

Sincerely yours,

Piet van Loon

Erik Thunnissen

Joop Kuik

Monique Roukens.
Dear Editorial team,

In a very last attempt to get our manuscript published in Scoliosis, some addresses will be made by us to the remarks of the last remaining and still in some aspects opposing reviewer.

We ask the Editorial team kindly to intervene if they are also of the opinion, that the last reviewers report brings too much new remarks, that gives the impression, that we are opposing the way of brace-techniques, in different forms brought by the reviewer himself, and he takes the opportunity to defend himself. This was never the goal of our study, because the history of our technique is based partly on serendipity in the field of clinical behavior of the thoracolumbar spine and a thorough search in literature (even historical or neglected), how the found results in the first period could be explained. We brought insight in these processes on different occasions and in other articles than only in this ms, as well as in abstracts and a book chapter.

Or you understand and accept the backgrounds and accept the given results, or you don’t. We stressed several times, that the drawback to face the impossibility of follow-up our cohort completely to the end of growth, while leaving his institution of the main –author, makes this research only valuable as “preliminary”. The upraising local conflict on medical scientific matters will soon have further official comments of Governmental institutions and a civil Court in the Netherlands. Even the complete care for Scoliosis is in decline, what worried many people. Blowing the whistle by a physician cannot always encounter direct emphasis.

It is with our best knowledge, that the reviewers earlier remarks were addressed properly, but it looks we end up in a more semantic discussion or a discussion were only differences in design have an important role. A discussion where the origin of our technique, the etiologic complex of neuro-osseous growth relations and the specific role of the thoracolumbar joint which are strongly supportive in our process of “reversed engineering” and have a prominent place, seems no part of the reviewers comments.

At the other hand cultural differences between countries and institutions in the prevailing way of dealing with spinal deformities is not to be discussed by us in an article. The SOSORT studies, who deal with this are under the references. There is more in a child with scoliosis or kyphosis, than the magnitude of curves. There we and the reviewer come very close to each other.

1. We cannot agree with the remark no point to point response to his earlier questions were given. There were three more reviewers, who had right on answers and did accept our changes.

   In the review of Weiss, there were only few questions, more on making more humble statements and rename the study as case series, what was done. There was an argue in his review of March in which the reviewer stated, that “So nothing is principally new or even better than state of art” to which we still has to answer, that a profound change in etiologic factors and switch to the original thoracolumbar kyphosis, precluding the idiopathic scoliosis cannot be seen other than new and even in some parts truly innovative, but based on forgotten research. Nature doesn’t change itself, we change our insights in it.

2. In this last text only a few changes were made and marked.

3. Although we carefully followed the instructions in the first submission, no other reviewer had comments on the style not to be according the Journal style,. We checked it again and we will kindly respond to specific omissions brought by the reviewer.

4. Another check on language and spelling was done.
5. We stated very precisely, that Dickson’s focus was on the thoracic spine and that we were able to get it back at the thoracolumbar spine, the area in which in all deformities the proper development in function and morphology have its start. The study of Ni et al. recognized many of this, just by relating the magnitude of the thoracolumbar kyphosis with the coronal curves.

6. Coming from a firmly stated plea in this ms. to correct exclusively the TL spine towards lordosis in any deformity in the same way, it’s not to us to comment on different types of braces and areas of indication of the reviewer.

7. Our study has a firm drawback in the for statistic reasons chosen period of only “at least one year” of wearing the brace. That is accepted or not. We cannot change the way the case series were started. But the achieved changes in many other curves in both planes and the intercorrelation is clear and in a way unique. The fact the reviewer accepted “lordosis” as not “harmful” as Dickson and followers stated, is a great support for our work.

8. In the ms. we describe the way the TLI braced is adapted at every control to an even more extended(lordotic) position. That declares the difference between a + and a ++ in table 3. It represent the dynamic character of this technique over time. According to the VolkmannHueter principle you get more correction as you can change the functional working zone of the deformable facetjoints and bring the TL discs under more optimal or original loading conditions. In early youth and in healthy adult spines, the TL spine is the most three dimensional functioning part of the spine. In the used references one can find the scientific evidence of this.

We do hope this letter and the changes undertaken will bring this to a good end.

On behalf of Erik Thunnissen, Monique Roukens and our statistics Kuit I do send greetings,

sincerely yours,

Piet J.M. van Loon