Reviewer's report

Title: Toe spreading ability in men with chronic pelvic pain syndrome

Version: Date: 2 September 2004

Reviewer: Ahmed N Ghanem

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General

Re: Toe spreading ability in men with chronic pelvic pain syndrome (CPPS):

Article appraisal

Despite the extensive list of exclusion criteria mentioned in the article, there is one that has been overlooked that may screw the results. This concerns failure to exclude subjects with meta-carpo-phalangel joints disorder that limit toe mobility and toe spreading ability at a local level such as arthritis and hallux valgus deformity for example. The photograph in (Figure 1) shows the big toe over-riding the second toe of the left foot. Halux valgus and toe over-riding are feet conditions that may limit ability to spread the big toe or move it towards midline of the body. However, this does not necessarily reject the hypothesis as it may be argued that such local toe disability result from the same pathology suggested by the authors as "subtle deficits in the most caudal part of the spinal segments causing muscle spasm". It is possible that a similar spasm of the inner foot muscle innervated by the same spinal cord segment may cause stiffness and toe deformity later. The conclusion made on pelvic floor muscle spasm in CPPS patients was reported from the same research centre group or team 5, plus or minus a couple of authors.

The hypothesis has been proved by authors based on demonstrable statistical significance (p=0.007) that CPPS patients were less able to spread all toes than men without CPPS. I trust authors have input correct data, selected the statistical methods that best support the proposed hypothesis. They sincerely believe the conclusion and hypothesis are correct, and must be well prepared as surely equipped to defend it. These facts, irrespective of any different opinion, are in favor of accepting the article and reporting the hypothesis based on the currently accepted standard methods of article evaluation and peer review process.

The reported data also show as indeed mentioned by the authors that there is statistically significant difference between the controls and patients in age, education and employment. This means that the controls were not matching. The authors' self audit stated: "Age could result in subtle changes in toe spreading ability". Also the authors mentioned that the association between toe spreading and NIH-CPSI scale of CPPS are statistically insignificant. These two issues may arguably refute the conclusion and hypothesis on toe spreading ability and CPPS. I wish to read a convincing explanation from the authors on these issues in defense of their hypothesis.
The knock out blow to this report comes from analyzing the data given in table 1. The authors built their conclusion on the statistical significance found on comparing the subjects of both groups who scored 4 and thus had normal toe spreading ability. This is only one matched small subgroup of both patients and controls, however. On comparing the figures of all the remaining 4 sub-groups scoring (0-3), separately, based on matched numbers shown on the shaded diagonals of rectangles of Table 1, statistical insignificance was "smelt" here!

The same conclusion was reached independently by a friend who calculated p-value but did not want his name mentioned. He is a Professor of Urology and an expert peer reviewer to most known urological journals; his opinion was to reject the article- based on this point alone. I initially intended to reject the article, and certainly would have done so if I am reviewing it for a paper journal with tight space and no chance of adequate justification of reviewer's recommendation. However, after long, hard and serious deliberation I decided to recommend acceptance of the article and give the authors a second chance of defense but final acceptance depends on their reply to all numbered points here.

Hence, I would ask the authors to re-consult with their own statistician on the validity of analysis and possibility of detecting any known statistical error. If it has been affirmed by the authors that there are no known statistical error in the analysis then the article should be accepted as it is for publication. If they returned a reply affirming detectable known error, that I much doubt, the article may be rejected. However, the story should not end here as nothing will be learned by the quick burial of rejection. The real truth is revealed by post-mortem examination. The article may be reported with appropriate comments and authors' reply.

In summary, the main conclusive evidence proving the hypothesis correct is based on statistical analysis, if correct the article is acceptable, and if wrong it may be rejected. However, I personally do not recommend rejection of this unique article for many reasons. As it would be regrettable to me and a great loss to the scientific and medical community and readers, if the article is rejected and a subtle important issue is buried. The article may be sent to and get accepted by another journal without comments. The reader will miss the an education value as well as being left in the dark uninformed about a higher cause and most important issue, concerning a subtle statistics bias. I look forward to the authors' reply so one can finalize his recommendation.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

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Discretionary Revisions (which the author can choose to ignore)