Reviewer's report

Title: Trunk Muscle Activity in Healthy Subjects during Bridging Stabilization Exercises.

Version: 1 Date: 18 July 2006

Reviewer: Nina Vollestad

Reviewer's report:

General
This paper addresses an important topic related to low back pain and exercises used in treatment. The paper aims at determining and comparing the degree of activation of various trunk muscles during tasks requiring different degrees of stabilisation. It is also an aim to examine if so-called global and local muscles are activated differently for the three tasks.

The study is carefully designed and conducted and it provides some interesting and important data. It has a potential for providing even more data that may enhance the value of the study. Hence, some additional analysis and clarifications are needed.

I strongly disagree that the data can be used as a normative database for comparison with patients (as stated in the abstract, at the end of the discussion, and in the conclusion). For this purpose, a much broader group of subjects is needed, being comparable with patients in for instance age, gender, sociodemographic factors, training status and background. The subjects included are between 19 and 23 years and there are equal proportions of men and women. Apart from that, we do not know anything about them. However, I believe that this paper can serve as a pilot study, indicating that it is worthwhile to generate a valid database in the future.

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

1. In several parts of the manuscript the authors claim that the ratios are more important than using relative EMG values in comparisons (e.g. conclusion of abstract, in the discussion and in the conclusion. It is however, difficult to understand how the present data contribute to this. Furthermore, one may wonder if these statements are too broad, since ratios and relative values reflect different aspects of muscle activation. I believe that relative EMG can be important in itself, when you what to get information about the degree of activation of a specific muscle, or compare the degree of activation of muscles. Ratios may be used for other purposes. The statements regarding this should be carefully revised.

2. All statements regarding the normative databases should be omitted (for the reasons given above).

3. The abstract is incomplete, in that it does not present the data on relative EMG, only on ratios.

4. It is one important aspects that the authors have not discussed. That is that all their data are not only dependent upon the activation during the different tasks, but also during the maximal contractions used to normalize EMG. These contractions are in nature "global", and it is a possibility that the local muscles are less close to maximal activation than the global muscles. If this is true, both ratios and relative EMG are difficult to interpret. However, you may come around this to some extent by analysing the differences between the tasks, by for instance using Exercise 1 as a kind of baseline. Hence, you will obtain data on the actual differences when you add complexity. This analysis will help a lot in interpretation of your data (and actually, several places the data are discussed in a way that should be based on these data).

5. No data from the repeated ANOVA is given. This should be done, unless the suggestions above removes the need for this analysis. The statistics of the differences in ratios should also be given.

6. Figure 4 and 5 show relatively large SDs for those ratios with mean values clearly above 1. Assuming that the data normally distributed, one may thus expect a substantial proportion of subjects with ratios at 1 or even lower also for IO/RA and IO/EO (ipsilateral). This indicates that the typical patterns of activation that you discuss, is not universal, even for young healthy subjects. This finding should be discussed.

7. The paragraph at page 12-13 (starting with "During the ball..." is difficult to follow. Probably you need to
clarify what you mean by "stability". Is it segmental stability or trunk stability? You also base your discussion here on a higher EO activation than IO in Exercise 2, but this is not in accordance with your data. Furthermore, this discussion will benefit from the analysis suggested above.

8. Also in the following paragraph, there are problems both with clarity of the points you try make, and in the data referred to. I believe the data of ipsilateral and contralateral are exchanged.

9. I cannot see that the present study provide data on the discussion in the paragraph at page 14-15 (starting with "It remains unclear ..."). In addition, you have overlooked a clinical study of the efficacy of stabilization exercises focusing on both global and local muscles (Stuge et al 2004).

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

10. Some sentences are incomplete and lack words.

11. Provide more data the subjects, where they are recruited from, their training activity level, etc.

12. Is it correct that you used a low-pass filter of EMG of 1 kHz and sampled at 1 kHz? If so, you may have problems with getting erroneous data (confer literature or exercise on these issues). You need to provide some evidence that this has not created a problem for you.

13. Due to the importance of the maximal values for proper interpretation, these should be given in a table.

Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interest.