Reviewer's report

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

Version: 1 Date: 11 July 2006

Reviewer: Gregory Lehman

Reviewer's report:

General

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

Reviewer: Greg Lehman

General Comments (Accept with Major Revisions or comments/justification)

Good paper. Straight forward, easy to read with a good review of the literature. More time could have been spent discussing the debate in the field regarding the biomechanics of local vs global muscles and their relative importance.

I may appear to be unduly harsh in some of my criticism and I have repeated myself consistently. My main concern is that many assumptions are stated as facts and some comments are made that were not studied or found in the experiment but rather come from what are common clinical beliefs and are still highly debatable.

I urge more caution and hesitancy in some of your statements.

Good luck.

Abstract:

-it is assumed by some that the ratio between so called local and global muscles is essential. The abstract should not state as fact as an assumption. This debate is discussed well in the introduction so it should not be simplified and have sides chosen in the abstract.

In the conclusion of the abstract authors need to be hesitant in commenting on the need for stability since stability was not measured. This statement can stay but it should be hedged with a qualifier like possible need for stability or something like that.

Background

creating optimal movement is essential again this is assumed. What tests exist to document non optimal movement?

2nd paragraph, last two sentences: therefore, it seemed useful to compare the activation patterns of muscles in relation to local and global

I don't see how this sentence follows from the preceding sentences which discuss the idea that there is considerable debate regarding local and global muscles. If you believe that no one muscle is more important than another than why is finding ratios important? It is important if you believe that the rectus abdominis activity should be minimized and you should activate the local muscles preferentially. This paper seems to assume of what McGill is arguing in the papers you cite. Would it not be more appropriate to cite
Hodges, Jull and Richardson? The concept for the paper is still valid but the rationale you have put forth (McGill’s work) does not make sense. Could you elaborate more on why finding these ratios is important and how it adds to the debate or our knowledge in the area.

Last paragraph: Marshall and Murphy (2005) have calculated these ratios during front bridge exercises—could you comment on this.

The paper you cite by Kavcic (2004) you state that the amount of activation and the contribution of both local and global muscles was not analyzed. These authors did look at the contribution of local and global muscles they looked at all muscles and determined the contribution to spinal stability. Calculating muscle activation levels was just one step in finding stability and the contribution of different muscles. You can’t say that they didn’t look at the contribution of the different muscles. You can say that they didn’t calculate an EMG ratio that you wish to look at.

Methods

You probably did not measure Multifidus surface activity. I believe this has been disproven (Stokes IA Clinical Biomechanics 2003).

Data Analysis

-what was the overlap window of the RMS?

Results

Well presented

Discussion

6th sentence: this is a debatable sentence. You have referenced researchers and clinicians that either co-author papers or cite each other’s work. Please state this statement less as an assertion of fact and more as an assumption of how the spine might optimally work.

7th sentence: still a contentious statement considering that your literature review focused on McGill’s work which concluded the opposite with a far more robust biomechanical analysis than simple surface EMG. This statement is still an assumption and not at all supported by what you found or the papers you cited in your background.

You can only make comments on the amount of muscle activity you did not measure stability nor did you measure any clinical efficacy. Your factual comments should stay focused on this. Your assumptions and beliefs when discussing the literature should be stated as just that—assumptions and beliefs of other researchers.

3rd paragraph: This sounds a little self-serving but we published a paper last year (Lehman GJ 2005) that assessed the trunk muscle activity during prone and supine bridging on and off an exercise ball and found similar and also slightly different results (We found higher IO > EO activity in both the ball and floor bridging with the ball having little effect non significant unless it was a prone bridge). I think this should be commented on considering the obvious similarities between the studies (we did not measure the ratios). This is similar finding to the work of Marshall and Murphy.

The Unilateral Bridging Exercise

You have not considered (or perhaps not said it plainly enough) a simple rationale for the finding that the ipsilateral IO should higher activity than the ipsilateral EO and vice versa for the contralateral IO and EO.

The ipsilateral IO causes an ipsilateral rotational moment about the spine and the ipsilateral EO causes a moment in the opposite direction. When you raise the contralateral leg a rotational moment about the spine occurs. The IO and EO are becoming active in a textbook fashion to counter this moment. The required
muscle activity may generate stability but is also necessary to stop the spine from twisting.

2nd last paragraph: the idea that a ball automatically increases the muscle activity has been disproven (please see Lehman GJ or Marshall & Murphy)

Conclusion

The following statement was not supported in your findings:

The results of the present study show that evaluation of the relative contribution of local segmental stabilizing to global torque producing muscle activity, using ratios, is more important than focusing on minimal or maximal activity of specific muscles often used in the past.

I do not see where our findings let you make value judgements (i.e. what is more important). We don’t know what ratios are good or even if there are any. You could have an exercise with “excellent” ratios with very high muscle activity and possible high compressive and shear loading.

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

Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests