Reviewer's report

Title: The effectiveness of pelvic floor muscle training alone and with other physical therapies for the treatment of stress urinary incontinence in women: a systematic review

Version: 2 Date: 16 November 2005

Reviewer: Peter Herbison

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)

I have a general feeling of uneasiness about this review without finding anything too seriously wrong. I am not sure why, and I am sure that the review comes up with approximately the right answer. I will list my concerns.

I am uncertain about the use of levels of evidence, that are supposed to apply to all the evidence, to individual studies. For example no study can have level I evidence as that is evidence from a systematic review. I am not sure how much this matters, as it could be worked around, by saying that this study would contribute to level x evidence. I would remove the levels from the abstract at least and be clear that these are the Australian ones, as there are many others.

The authors say this is a study looking at "effectiveness" rather than "efficacy" but then say that time scale is not being considered. I would find it hard to consider effectiveness without regard to the time frame over which the treatment may work. Just about all of the studies measure the outcome at the end of the treatment. For effectiveness you would have to assume that the treatment would continue or that the effect would remain. I am not convinced that either of these will happen.

I find it hard to be sure that they have a comprehensive group of trials. They have not followed the QUORUM statment in this, as that says you must say how many trials were screened, how many excluded, and the reasons for this. For example they exclude Glazener CM, Herbison GP, Wilson PD, MacArthur C, Lang GD, Gee H, et al. Conservative management of persistent postnatal urinary and faecal incontinence: randomised controlled trial. BMJ 2001;323(7313):593-6, a study which would appear to meet their inclusion criteria, and is interesting in that it has a follow up:
I am sure there would be others that people might wonder why they are not included.

There is an over emphasis on the quality score of the studies. The one thing that is certain about quality scores is that they can give almost any answer when used in a systematic review (see Juni P Witschi A, Bloch R, Egger M. The hazards of scoring the quality of clinical trials for meta-analysis. JAMA 1999;282(11):1054-60). The particular quality score chosen has some elements that are highly unlikely to be associated with bias in the outcome, or the difference in outcomes between the groups in the studies. For example, whether a study reports "ethical processes", "Consideration of sample size" or "Lit review relevant" or not will not affect the internal or external validity. This is not
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say that these things are not worth doing or reporting, just that they will not introduce bias. Thus there is the potential for a study that has a high quality score to still produce a more biassed result than a study with a lower quality score. Studies in a meta-analysis should be assessed for quality, but this should not be by way of a score.

In the introduction the authors say they want peer-reviewed reports, but this is not mentioned in the inclusion/exclusion criteria. Some of the abstracts will have undergone a cursory peer review.

In the methods of the review they say about assessment of quality but then don't say how they use this.

There is an element of study counting when they report number of outcomes reported and number significant. Many studies selectively report outcomes that are statistically significant and this should be discussed.

I have some issues with the section on compliance. Of course PFMT is unlikely to be effective if people don't do it, but the effect may well be different in those who do it out of choice and those who are "encouraged" to do it, so it is not just a simple matter of saying that if they did all the training then this would be the effect.

In the implications for practice section, second bullet point it should be made clear that they are not saying that BF, ES etc may still have a place in clinical practice as stand alone therapies, but on the evidence in this review only as adjuncts to PFMT.

Implications for research, bullet point 2. Do they mean effectiveness, or efficacy?

In Table 1 why is age presented as the only participant information. For stress incontinence it is likely that parity is even more important - and I am sure there are other patient characteristics related to incontinence that could be presented (e.g. BMI).

Figure one should be replaced by a 2-D plot. These are much easier for the reader to interpret.

In the appendix with the quality score the abbreviation OM is not explained.

While most of the provisions in the QUORUM statement are met there are some that could be met without making large changes (e.g. including the words "systematic review" in the title.

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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)

There are some typos. For example, in the first paragraph of the results there is a p>0.895 which is surely p=0.895.

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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.

I think I should say that I am involved in the Cochrane Collaboration, and think that as many systematic reviews as possible should be published in the Cochrane Library. I think that this review would require some tidying up before being acceptable for the Cochrane Library.