Reviewer’s report

Title: Relaxation training for anxiety: a ten-years systematic review with meta-analysis

Version: 1  Date: 11 February 2008

Reviewer: Victor Sierpina

Reviewer’s report:

Comments: This is an excellent systematic/meta-analytic review and offers support for the use of relaxation therapies for anxiety. It also helpfully finds certain variations in populations and methods for with which to target these therapies.

I believe it should be published with minor modifications.

Regarding points of assessment:
1. Both question posed and hypotheses are well defined
2. Methods are appropriate and very well described
3. Data sound? Yes
4. Yes
5. Yes, in fact, discussion and conclusions are really excellent and help the reader extract the most useful information from the article
6. Limitations clearly stated and in fact boldly admit to going beyond the RCT model in their review to gather a larger number of very relevant studies.
7. Yes. This is an excellent bibliography
8. Yes
9. Mostly. Given that these authors are Italian, their use of the English language is very good. At times it needs some work, itemized below, including elimination of contractions, a few awkward constructions.

Minor Essential Revisions recommended:

P 2: Jacobson’s progressive relaxation is never defined nor referenced. Despite years practing and researching Mind-Body approaches, I don’t clearly recall this method and perhaps other readers won’t either. It needs to be defined, referenced, and clarified wherever it appears

Results: Rather than “Consistent with prediction”… use “As hypothesized” Or “Supporting our hypotheses”

P 3 Do you really need 3 references to same article (1) in first paragraph. Consolidate to one overall ref

Anxiolytic drugs should be lower case
We chose this span (not land) of time..

**Methods:**

While these are appropriate databases, I think the use of the CINAHL database would have added significantly to this study and ought be done in future analyses. The Nursing and Allied Health literature has abundant studies in Mind-Body therapies and leaving it out in this review, is one of the only weak points of the study. Perhaps this could be pointed out in limitations. Actually, CINAHL studies would probably increase the effect size, the n of participants and studies, and the number of techniques of relaxation employed.

**Data coding:**

What is provenience ??

Study characteristics:

Jacobson needs definition as above as well as citation

**P 9: para 2**

Almost half of the papers was realized in North American [awkward construct]

Change to: Half the papers were NA publications.

**P 9: Overall efficacy of relaxation training**

Please eliminate all contractions in the paper. This should be: as it is.

**P 12 Discussion:**

It's colloquial. Replace with Both meta-analyses. Last line in facts.

Replace with In fact...

**P 14**

Revised to:

people often improve just by being in a waiting list.

para 2 change It's

para 2 Further research is needed. Instead of Further researches are
needed --
Jacobson again

Last para:

More lasting training is awkward and unclear.
Perhaps you mean: Repetitive training over several months or something like that??
Last line: change coherently to consistent with.

P 15: Limitations,
Para 3: In order to improve their number. Awkward construct
Replace with In order to increase the of studies and participants
(actually, this was a wonderful idea!)

P 16
Conclusions,
Para 3
 Relaxation training still has good efficacy instead of relaxation training has still a good efficacy.
It replace with .
Young people and old people (define age range here), older than 50 or 60, younger than
Can’t and doesn’t in last paragraph eliminate contraction