Reviewer's report

Title: Predictors of outcome of multidisciplinary treatment in Chronic Widespread Pain: an observational study

Version: 1 Date: 13 December 2012

Reviewer: Manuela Glattacker

Reviewer's report:

This paper aims to identify predictors of multidisciplinary treatment outcomes in patients with chronic widespread pain (CWP). Identifying (changeable) predictors of outcome is a relevant issue with a practical impact – particularly in diseases of unknown aetiology such as fibromyalgia or CWP, where the treatment effects on subgroups of patients are often limited. The paper has several strengths. The authors address a range of theoretically relevant constructs. They specify concrete hypotheses, and the paper is generally well structured. However, there are several issues which should be addressed before publication.

Minor issues not for publication

1. Introduction section, 4. paragraph: I would suggest adding what patients with severe depression are typically excluded from (“Although patients with severe depression are typically excluded, symptoms of depression and anxiety are common (...)").

2. The same applies to a sentence in the introduction section, 7. paragraph (“Both factors are typically not a reason to exclude patients”) – please add, from what patients are not excluded.

3. Discussion section, first paragraph: The authors should repeat what “higher baseline values” (which were associated with greater improvement in outcome) mean: better or worse health states?

4. Discussion section, third paragraph: “These results are congruent with those of Glattacker et al. (...), who demonstrated that (...) are associated with a greater improvement in treatment outcome” - please add “in women with fibromyalgia”.

Minor Essential Revisions

1. In the patients and procedures section, the authors state that patients were excluded from the study if they were not motivated for behavioural change. I would suggest adding some information regarding the operationalization of limited motivation.

2. In the introduction section, the authors argue that “(...) reduction of symptoms is not the focus of a multidisciplinary pain treatment, in contrast to disability and interference of pain in daily life”. However, “pain” is named as the “first” outcome indicator in the outcome measurement section. This may be somewhat confusing, and I therefore suggest explaining the choice of outcome variables a bit more thoroughly.
3. Illness and self-efficacy beliefs section: Are the authors sure that “(…) low scores on illness coherence demonstrate positive beliefs about the controllability (…)”?

4. The “emotional representations” scale (IPQ-R) is not self-explanatory and should therefore be explained.

5. Discussion section, 6. paragraph: The authors state that, as their study population only included 5 men, their findings regarding the socio-demographic factors require further investigation. However, I think these results should be interpreted in more detail, i.e., how they were achieved despite the low power of the variable “gender”.

6. Discussion section, 6. paragraph: In their discussion concerning the level of education, the authors could perhaps complement their clinical experience with some considerations with respect to the construct of “health literacy”.

7. Discussion section, 7. paragraph: The authors explain the association between higher baseline values of outcome measurements with greater effects of multidisciplinary treatment with regards to content. However, this could also be a methodological effect (regression to the mean).

8. I would encourage the authors to display the correlations between the predictor variables within a table.

9. Table 1: I suggest adding the ranges of the variables within the table.

10. Table 1: I suggest adding the “N” within the table.

11. Table 1: In the manuscript text, the range of catastrophizing is indicated with 0-10, however, in table 1 the mean for catastrophizing is 23.85 – please double-check this result.

12. Table 2: please replace p=.00 by p<.001.

Major Compulsory Revisions

1. In their analyses, the authors consider a range of relevant constructs which are for the most part well introduced in the introduction section. However, there is a considerable overlap between these constructs, and I think the introduction would benefit from adding the results of an earlier study conducted by the research group (“Overlap of cognitive concepts in chronic widespread pain: an exploratory study”).

2. Furthermore, I would encourage the authors to provide more theoretical background to all constructs considered as potential predictors in the analyses (e.g. Catastrophizing, Psychoneurotism), and to expand the hypotheses to these constructs as well.

3. It is a good idea to deduce the hypotheses relating to the considerations in Figure 1. However, I think some differentiations should be explained more precisely. For example, as the treatment included cognitive behaviourial therapy and the acquisition of pain management skills (see “Intervention”), it also could be argued that variables such as illness beliefs or self-efficacy beliefs are not expected to predict treatment outcome (as done for fear-avoidance beliefs and
behaviour). Furthermore it could be beneficial for readers if the hypotheses (and their results) were summarized within a table.

4. The authors state as a limitation of their study that there was a large number of predictors – particularly if one takes into account the sample size. However, what was the reasoning behind the sample size, and did the authors conduct any power analyses?

5. I would suggest adding the effect sizes (in table 2).

6. As table 2 shows, the differences between the two measurement time points were rather small. This should be taken into account when interpreting the results of the regression analyses.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests