Author's response to reviews

Title: Association of the sense of coherence with physical and psychosocial health in the rehabilitation of osteoarthritis of the hip and knee: a prospective cohort study.

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Author's response to reviews: see over
Dear Editor

I am pleased to resubmit the revision of our paper entitled: “Association of the sense of coherence with physical and psychosocial health in the rehabilitation of osteoarthritis of the hip and knee: a prospective cohort study.” for publication as a original / full-length article.

The revision has been carefully done following point by point to the comments of the two reviewers (see pages 2 - 4). If additional changes have still to be done to fulfill the formal requirements please let me know it immediately.

All authors have contributed to the manuscript and agreed to be co-authors. The same is true for the persons named in the acknowledgements. Further, consent forms had be obtained from each patient who participated in the study.

There are no competing interests to declare.

Yours sincerely,

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Association of the sense of coherence with physical and psychosocial health in the rehabilitation of osteoarthritis of the hip and knee: a prospective cohort study.
Thomas Benz, Felix Angst, Susanne Lehmann and André Aeschlimann

Comments to the revision
Report of reviewer 1:

1. The exclusion criteria are not all clearly defined. What is 'severe illness' at inclusion? Same as the definition for follow-up? If so, please mention at first occurrence. What is the rational for excluding patients with medication non-compliance? And how was this medication non-compliance defined?
   Comment: We have revised the exclusion criteria in Methods, Patients on page 6. The exclusion criteria were specified more exactly. The "severe illness" was defined at first occurrence. The same definition was valid for baseline and follow-up.

2. The definition of the interventions deserves further attention. From the first paragraph, it seems that all inpatients have been outpatients before, i.e. have participated in the outpatient program before. Is that really correct?
   Comment: The complete content of Methods, Intervention on page 7/8 has been revised. The structure of this chapter has been improved. The description of what the patients did before the program has been outlined in greater detail.

3. It is stated that rehabilitation was patient-tailored. However, some more details would be helpful for the reader to get an idea of the general course of each program, e.g. How many sessions did the patients attend? Is it up to 36 for outpatients? How many sessions per day for the inpatients?
   Comment: The qualities and modalities of the rehabilitation programs were outlined more detailed in the revised chapter Methods, Interventions, page 7/8 and in the revised 2nd paragraph in Results, Patients on page 12 starting with "Socio-demographic variables...". Furthermore, the therapy quantities were reanalyzed and presented in the revised table 2 (page 26) in the strata active – passive, land-based – aquatic, and individual – group.

4. It may come up as a surprise to include patient education in the group of passive therapies. Please elaborate on this point.
   Comment: In the revised chapter Methods, Intervention (page 7/8) we described in detail active and passive therapies. We defined active therapies as all therapies that include somatic exercise intervention provided by physiotherapists for improving physical capacities and performances. Education was rather information than cognitive behavioral therapy and was therefore, classified as passive therapy.

5. In the Results section, the reader is somehow left wondering about the role of the independent variables/potential confounders. No results are provided. Does this mean that these variables (in/out patient program, length of treatment, comorbidities,...) played no role? This issue would also be worth commenting in the discussion section.
Comment: More information about the role of the confounders in the multivariate regression models and some results of them were added in Results, Correlation and regression analysis, 3rd paragraph (new), starting with: "Confounders included in 3 or more..." (page 13). More detailed results of the not significant confounders were not provided in the tables to save space. If the editor wishes to report these data in detail, we will add that.

6. Regarding the length and type of treatment, Table 2 suggests e.g. a mean of 23 hours of active and passive therapies for a mean duration of 22 days inpatient hip program. Although it has been stated that treatment is patient-tailored, the numbers in this Table need to be explained more in detail in the text.

Comment: We reanalyzed the therapies and completely revised the resulting data in the table 2 (page 26). See also item 3.

7. The discussion is interesting. It should, however, be noted that it is not self-evident how SF-36 is conceptually related to health promoting behaviors or to appropriate bodily resources. Indeed, the SF does not measure health promoting behaviors; in the same line, it is not obvious that 'appropriate bodily resources' are equivalent to physical dimensions of health-related quality of life.

Comment: It is true that we didn't directly measure bodily resources and health promoting behavior. According to Antonovsky, "a strong SOC should be associated with health promoting behavior and should mobilize the appropriate bodily resources. Therefore, perceived health should become better which will positively influence the physical dimensions of health-related quality of life [21]. This can be measured by the SF-36 and the WOMAC, whereas health promoting behavior cannot be directly measured by these instruments." See revised Discussion on page 14, 2nd paragraph.

Report of reviewer 2:

1. Abstract/Background: An additional sentence is needed to provide a stronger clinical implication for the study. Why is it important to know whether SOC is related to rehabilitation outcomes?

Comment: An additional sentence in the Abstract, Background was added (page 2).

2. Background (page 5, paragraph 1): Although this paragraph provides a good summary of the Antonovsky Sense of Coherence model, how might this model influence the way rehabilitation programs are developed or delivered? Without this explanation, the evaluation of the SOC in this study measure seems a bit arbitrary. What was the compelling reason to collect these data and perform these analyses?

Comment: The possible importance of the sense of coherence and the possible influence of it to adapt rehabilitation programs have been outlined in greater detail. See revised/added paragraph 6 in Background on page 5, starting with "Therefore, the SOC may...".
3. **Background (page 5):** A paragraph should be added to summarize the existing empirical support for the Sense of Coherence model in other disease categories and clinical settings. Although some existing studies are reviewed in the Discussion, the Background section should provide some indication of level of support.

   Comment: This has been done in the revised/added paragraph 5 in Background starting with "Osteoarthritis was associated with..." on page 5. Further studies are discussed in Discussion, 4th paragraph, starting with "Our findings are in line with ..." and in 5th paragraph starting with "A systematic review found..." on page 15.

4. **Methods (page 6, Intervention):** The rehabilitation program is well described, but can anything be said about the potential relevance of SOC with regard to program content?

   Comment: "The rehabilitation program was not based on a possible influence of the SOC on the effects of the rehabilitation." This has been added in Methods, Intervention, 3rd paragraph on page 7/8 (completely revised) starting with "Inpatients as well as outpatients...". See also item 2.

5. **Results (page 11, paragraph 4):** Given the very high correlation of the SOC-13 with the mental component score from the SF-36, it begs the question of whether these two measures are actually measuring unique traits or whether SOC is not distinguishable from a general measure of health-related psychological distress. There should be some Discussion of the potential overlap in these two scale measures (and their underlying construct domains).

   Comment: We agree to the hypothesis that the constructs of the SOC-13 and the psychosocial scales of the SF-36 overlap to a certain extent. Discussion about that has been added in the new 6th paragraph in the Discussion starting with "The high correlation of the SOC..." on page 16.