Reviewer's report

Title: Exploring the relationships between International Classification of Functioning, Disability and Health (ICF) constructs in people with osteoarthritis.

Version: 3 Date: 8 December 2010

Reviewer: Elizabeth Badley

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Major compulsory revisions

This paper reports on a structural equation modeling analysis of data on individuals about to have a total joint replacement (TJR) to explore the relationships between latent variables representing impairment (I), activity limitation (A) and participation restriction (P).

The title states the paper is about OA but the methods show that OA is only part (albeit the largest part) of the sample. The paper variously (and somewhat arbitrarily) presents findings for the total sample and for the OA subsample. I would suggest the authors need to decide to focus on one of these groups. Either keep the title as is, and report only on the OA sample (perhaps indicating that findings for the total group of all arthritis were very similar – but not showing the data or perhaps only in supplementary tables), or change the title to arthritis and report on the full sample.

‘prior to joint replacement surgery’ needs to be added to the title.

This is a cross-sectional study. The limitations of this needs to be explicitly acknowledged, not just hinted at, as in the present version. The authors need to be careful in their wording when it implies causality. For example, the final sentence in the abstract needs to be more carefully worded. In the discussion (para 2), I have issues with the statement ‘... ICF constructs have evidence of independence….and interventions to reduce restriction in participation are likely to be achieved by reducing activity limitations rather than directly altering impairment’. Similarly in the conclusion there is the bald statement that ‘treatment and interventions that reduce impairment only improve participation if activity limitation is improved’. While these statements are likely true, they do not fully reflect the potential (full) mediation of activity limitation between impairment and participation restriction. What this paper showed is that there were relationships between I and A and between A and P, but not between I and P. This suggests that A might mediate the relationship between I and P although longitudinal data would be needed to establish this. The wording needs some attention and the limitations of a cross-sectional analysis need to be more explicitly dealt with in the discussion.

Background, para 2. There has been (relatively speaking) lots written about the limitations of the ICIDH model, and this does not just relate to the reverse
pathways and psychological factors (which are listed here as the only example of the omission of contextual factors, with reference to the authors own work). I suggest the authors confine the discussion to the reverse pathways only, as these important to the biomedical model. The omission of contextual factors is not relevant here (except to acknowledge this as a limitation in the discussion).

I had some problems with separating out the biomedical model and focusing this paper on that. Raising the biomedical model raises old questions of the virtue of biomedical versus biopsychosocial models, and I’m not sure that this is productive. The I-A-P relationships are dominant pathways in the ICF (biopsychosocial) model and therefore worth investigating in their own right. I would suggest that the authors perhaps take this route. As indicated above a limitation of this paper (which then needs to be acknowledged) is the lack of consideration of contextual factors.

The paper spends a lot of time on the measurement model. I suggest the authors cut to the chase and present the final model with less description of the steps that were taken to arrive there. I would be content to know that after testing one and two factor models, the three factor model was the best fit. Then there were some further modifications to improve model fit (and that only suggested modifications within individual constructs were considered appropriate here). It the authors feel that readers need a blow by blow account this should go in supplementary material, not in the main paper.

I was very confused by the figures of the model – which all appear to be very similar. Figure 2 in the paper is the measurement model (and this appears from the coefficients to be the measurement model for the OA sample (or is it?). The final structural models are given in the supplementary materials. The coefficients are very similar in all models. I suggest for the paper the final SEM model is the one that is included for either OA or all arthritis (see above) – omitting Figures 3 and 4. I would be happy to take the details of the final measurement model(s) on trust – merely reporting that it was satisfactory with appropriate fit statistics (and once again, details could go in supplementary materials).

Background, para 3. Reference is made to core sets for clinical conditions and ‘few studies have empirically explored the relationships between I,A and P.’ These two concepts need to be dealt with separately. The fact that few studies have looked at I,A,P relationships is irrelevant to core sets which do not distinguish between A and P. This needs to be raised here as the fact that core sets do not distinguish between A and P comes up later in the discussion. It would be nice if other studies looking at IAP relationships were mentioned (there are some).

Measures. Need more details. Need response options for the items in the measure. Table 1 needs a more informative title. At a minimum it should include the name of the measures.

The response rate 43% is relatively low. This needs a comment in the discussion. It could be that non-responders had more severe disease; this does
not seem to be the case.

Minor Essential Revisions

Background, para 1. Need a reference to ICIDH.

Top of page 4. Refers to ICIDH constructs – for those who don’t know ICIH need to say what they are (perhaps where ICIDH is mentioned in para 1)

Background, para 5. This is confusing – it seems to mix up two papers in the order A-B-A. Needs clarification.

Participants. Omit ‘on that particular joint’ (it’s redundant).

Results. There seem to be two versions of Table 2.

Labeling of figures. Supplementary figures do not have figure numbers. The SEM models are labeled ‘for all patients’ and ‘patients with confirmed OA’. More descriptive titles are needed mentioning that these were patients about to have TJR. The figures for the final models also need to have footnotes explaining what all the boxes are.

Background, para 3, line 4. Who are ‘they’? – this needs to be reworded.

Discretionary Revisions

Figure 1: I suggest colour is not used (perhaps a broken line instead) in consideration for those who want a copy of the paper but don’t have a colour printer/copier.

Figures of the model. This is a suggestion to improve clarity. If the authors flipped the section of the model that relates to I (i.e. take the mirror image (in the vertical plane)) then the lines joining the circles containing I.INDEP to A.INDEP and P.INDEP would not cross over the boxes for the variables and the error terms etc. It would nicely put the I-A-P relationship in a triangle in the middle of the figure.

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

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.