Reviewer’s report

Title: A structural equation model for the effects of the doctor-patient encounter and expectancy in an open-label randomized trial of spinal manipulation for the care of low back pain

Version: 2
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Reviewer: Jarred W Younger

Reviewer’s report:

In this manuscript, the authors examine the impact of patient expectations, dosage, and the patient-doctor relationship on low back pain changes after spinal manipulation treatment. Issues of non-specific effects are of high concern in any clinical trial, especially where self-reported pain is involved. The authors have produced a technically-sound paper on an interesting topic. There are some minor issues of assumptions coloring the discussion. Specific comments follow . . .

Major Compulsory Revisions:
None

Minor Essential Revisions:
Have these data been previously analyzed and published? Should note explicitly in methods if this is a re-analysis.

Discretionary Revisions:
Would it be useful to present a zero-order bivariate correlation matrix?

It would be interesting to see if we can predict the dropouts using baseline predictors. Do the authors think baseline expectancy, or initially-reported DPE would predict attrition? But, as most dropouts as noted as "personal", we may not expect much interesting to come out of those analyses -- unless participants gave personal reasons to avoid being confrontational.

Once we leave the safety of the baseline measurement period, I’m sure the authors know interpretation of paths becomes difficult. The authors note issues of bi-directional relationships and feedback loops. The authors seem to be biased towards seeing DPE as a construct that changes pain, though the reverse path could be argued. I tend to like doctors who give me treatments that work, and dislike doctors that give me treatments that don’t work. DPE, measured at the same time as 6-week pain, may be difficult to interpret as a causal variable. A possible limitation.

The authors suggest their method creates equipoise among the clinicians, but we don’t know what the equipoise would have been had the authors not trained the
clinicians. It is possible that the participants would have seen no differences in clinicians’ attitudes even without the equipoise training. The concern is minor, though, because it is fairly well understood that clinicians in trials should be trained for homogeneity of the practices. A possible limitation.

My general understanding of path analyses is that betas give relative, but not absolute, importance of the paths. Could the authors provide R2 values in Figure 2 for each of the paths so we can more easily determine the variance explained? Of, if that is not possible, could we see R2 values inserted in the variables to see how much of the variance is predicted by the model at each step?

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

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare that I have no competing interests.