

Reviewer's report

Title: Anthroposophic medical therapy in chronic disease: a four-year prospective cohort study

Version: 2 Date: 20 December 2006

Reviewer: Harald Walach

Reviewer's report:

General

In general this is a good study and the MS is in a very good shape. I will make a few suggestions for improvement, which I feel the authors should at least respond to, preferably incorporate.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. The authors should indicate how they calculated effect sizes. Was it by using a pooled standard deviation, other methods, or what?
2. Effect sizes of the KINDL questionnaire are zero, while quite large percentages of improvement from baseline are given. This can mean two things: the questionnaire is crap and invalid, because it does not have any re-test validity; or kids change very rapidly. I think the authors need to discuss this puzzling finding
3. Effect sizes of the SF36 scales are rather small, while the effect sizes of the symptom specific measures and clinical measures are larger. For their interpretation the authors seem to rely more or less on the disease and symptom scores. This discrepancy would warrant some discussion and interpretation. One way of addressing this would be to go into the more recent literature around the model validity of the SF36, which is poor, or discuss why the disease scores/symptom scores should be better indicators of therapy success.
4. I find the reporting of the data a bit confusing. Some of the data are reported for 12 month (Table 3 clinical outcomes), while some are reported for 48 months (Figures two and three). My understanding of the design is that all outcomes were measured after 48 months, except the symptom score. It is not clear, why different reporting schemes are used here. The critical reader could surmise that the authors would wish to go for the best moment in time to report effect size improvements. I find the authors should determine whether they want to report mid term (12 months) or long term (48 months) effects and then stick to it throughout all presentations.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

p 3, top: I am not sure "median" should be used as an adjective; it would probably better to say 3.. consultations in the median or something the like

I find it hard to believe that none of the authors would have any competing interests or conflict of interest, as, to my knowledge, two of the authors are more or less dependent on the flourishing of the anthroposophical pharmaceutical industry. This in itself is not at all a problem, but it should be mentioned, if it is true.

Discretionary Revisions (which the author can choose to ignore)

I find the evaluation of the data, considering the large expertise of the group, a little bit dry. They have collected excellent data, with a high degree of completeness and auxiliary data of a rare complexity. Why not slot the data into a regression model and find out how much of, say the 48 months improvement in main symptom score can be explained by demographic variables, by initial scores, by the usage of other therapies, etc.... This is not a hugely complex analysis and would explain a lot in one shot, especially as they document that their patient group is selected and has a favourable sociodemographic profile. If they

have data on duration of first consultation, even that could be addressed. This would give immediate estimates of how important these factors are in terms of explanation of variance. I am sure they have thought about that and they may even have good reasons for not doing it, being bored by their own data being a potential and very understandable reason. But I would hate to see such a great effort wasted in some blunt descriptions only.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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.

Declaration of competing interests:

I declare that I have no competing interests