Reviewer's report

Title: Who will increase their physical activity? An examination of the predictors of change in objectively measured physical activity over 12 months in the ProActive cohort

Version: 1 Date: 29 September 2009

Reviewer: Deborah Weiss

Reviewer’s report:

General comments: Overall, the manuscript presents interesting work, and is well-written. As stated by the authors, the availability of objectively assessed PAEE as an outcome variable, a rarity in the literature, is an important strength of this analysis. A second important strength is the large sample, which is also noted by the authors. That being said, I think the analyses are a bit superficial, and given the data available, more should be done (see below).

Major compulsory revisions- these pertain to the analysis. If there are reasons why these analyses were not done, or if these analyses were done but the data are not presented, the authors should mention this in the manuscript. Based on their description of the data, however, I think more analyses are possible, and warranted. As it's currently presented, it’s not clear why they used the method they did.

1. In the earlier publication, where the results of the trial are reported (Kinmonth et al. 2008) the authors reported that there was clustering in the sample due to the inclusion of family members. Since this new publication uses the same data it’s not clear to me why a mixed effects model wasn’t used for this analysis.
2. I’m puzzled as to the lack of any multivariable analysis. If baseline PAEE is the only statistically significant predictor in a multivariable model then this should be stated. If the multivariable results are the same, then one line to say it was checked would suffice.
3. A paragraph in the discussion is devoted to the hypothesis that when physical activity is assessed by questionnaire, this increases the likelihood of finding predictors. Very interesting, but why then not use the questionnaire data to run the same analyses? There is a mention of this in the discussion, but no data are presented, and it’s not discussed anywhere else in the manuscript (no mention in the methods). There are only two tables presented in this manuscript, I think it would be fair to include another with this analysis. It is rare to have access to both types of data, and given that this is presented in the discussion, it should be addressed in the analysis.

Minor compulsory revisions-
1. on page 4, 2 lines up from “methods” section heading, missing word, may not be causally...
2. on page 11, paragraph and sentence beginning “fourth”, whether this factor was an important predictor…(remove the s).

Discretionary-
1. I’m not convinced that the model best represents change as referred to by the authors. What about using the difference in PAEE between baseline and follow-up as the dependent variable? The results may be the same, they might not be, but it would strengthen the analysis in my mind if this was checked.
2. The authors could also consider using structural equation modeling for this analysis, it might be more informative.
3. Literature review- my impression is that the lit review should be updated. In looking at the dates of the various papers, in particular those relating to the predictors of physical activity behavior, some important recent literature has been missed. I will admit I’m less familiar with the TPB literature than the social-cognitive, but I find it hard to believe that so little has been published in the last 5 years.
4. Given the findings of the study, I find the title to be misleading.

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