Reviewer's report

Title: Broad autism phenotype status of parents of children with autism in China and its correlation with severity of social impairment in probands

Version: 2 Date: 1 February 2015

Reviewer: Dorothy Bishop

Reviewer's report:

Overview:
This is an account of a study of the Broader Autism Phenotype in a Chinese sample of families with an autistic child compared with a control sample. Parents were assessed using the BAPQ (Broader Autism Phenotype Questionnaire) and their children were assessed using the SRS. The study has many positive features. The sample size is impressive and the study is clearly described. I felt the study would be publishable as it stands, but it could be improved by adopting a more appropriate statistical analysis, and inclusion of some additional analyses would increase its value.

Major compulsory revisions
1. P 7, para 2. I would recommend that the self- and informant-estimated scores are kept separate for two reasons. First, in the original account of the BAPQ by Hurley et al, on the pragmatic scale and the total scale, the sensitivity and specificity of the informant report was better than the self-report, suggesting that some individuals may lack insight into their BAP characteristics. Second, there is potential for rater bias affecting the correlation between BAPQ scores and SRS scores. The SRS in the current study was completed by mothers. Correlations between parental BAPQ and SRS were generally found for mothers but not fathers. The concern, then, is that some bias in the rater might affect both ratings. To avoid such contamination, you could look at the informant-rated BAPQ of the mother and the SRS of the child, so that different people were making the two ratings. This seems important because a priori it does seem surprising that there should be a correlation between BAP features in parents and autistic symptoms in children just within the autistic group. It would be more convincing if the correlation were present even if different people did the ratings.

2. P 9. I did not understand the logic of doing the regression analysis separately for the two groups. It would help if the authors could explain the reasoning behind this, in terms of possible genetic models that might predict different results for the two subgroups.

3. P 9. Results. I was rather unclear about the statistics conducted on the BAPQ scores. The F ratios suggest Anova was used, but were separate Anovas done for each scale? It would be more informative to do a 2 x 2 repeated measures Anova with parent as one factor and scale as the other. That way one could see
whether there was any interaction between parent and scale, as well as testing for main effects of each of these, with a method that incorporates correction for number of levels of the factor. Indeed, this could be extended to have a 2 x 2 x 2 Anova with rater as a third factor. It might be worth splitting Table 1 into two, with the Parental BAPQ data separate (thought I treat this suggestions as Discretionary)

4. The multiple statistical tests for the data in Figure 1 are also not optimal. And indeed, some of the results are surprising, with statistical differences reported for differences that look very small (lower panel of the Figure). We do need some control for multiple contrasts here, and it might be more effective to present the data in a Table, where means, N and SDs can be reported, as well as significance levels. The graph is not so effective for representing such a wide range of scores.

5. Discussion: Somewhere in the discussion it would be worth at least mentioning that there may be an effect on the BAPQ of having an autistic child. Thus if parents of children with autism are more stressed, or have their lives more constrained, this could affect their responses. It is not possible to control for this except by having a control group with another disorder, but I think it should not be forgotten as a potential source of variation. The issue of rater bias should also be touched upon.

Discretionary revisions

6. P 7, para 1. Personally, I would not have excluded children with high SRS scores from the control group; it seems unlikely that these children would be autistic, and it would be better not to restrict the range of SRS scores. The concern is both statistical (difficulty seeing relationships with a restricted range of scores) and methodological – what is needed here is a typically-developing group, not an unusually symptom-free group. It is noteworthy that the SD of SRS score for controls is much lower than that of the ASD group. I do not regard this as crucial and I can see the argument in favour of the authors' decision to exclude these cases so I do not insist on this change, but I think it might be worth more justification if this criterion is maintained.


Minor issues not for publications (Line-by-line comments)
The standard of written English is impressive and I just have one or two minor suggestions for amendment.
P 3, Line 10: add "the" before "Social"
P 4, line 4, add 'a' before 'different'
P 5, line 4, add comma after 'influence'
P 5, sentence starting line 9 would be clearer if it started with "To better understand the genetic etiology of autism" 
P 5, line 15 represent -> reflect
P 5, line 20, suggest
P 6, line 2 full stop after }
P 6, line 4, full stop after autism
P 6, line 5 'the West' -> Western
P 11, line 1: "the two groups" is ambiguous here – could mean autistic/control or mother/father

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests