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We thank the reviewers for their comments and suggestions. These are specifically addressed below:

Reviewer 1 (02445281)

Non-significant trends usually do not merit a discussion, since there were non-significant differences between groups.

Although statistically significant differences were not found between groups, a trend was identified with a strict alpha value that was set to account for multiple comparisons. This trend was discussed as it is relevant to the illness, and may prove to be statistically significant in larger studies utilising the same measures. This is described in par. 3 of the discussion section of the revised manuscript.

The experimental group, albeit receiving medication sustained the eating disorder. This aspect requires a more detailed description in methods section (for instance, doses and duration of treatments) and a wider discussion, because of the possibility that drug treatment may influence the results of the test, but not the cardinal symptomatology of anorexia nervosa.

Patients were on a number of different medications, all with different prescribed doses. The potential effects of medication have been discussed in par. 3 of the discussion as a potential limitation of the study.

Reviewer 2 (00547906)

I would suggest to include additional discussion on the limitation due to the small sample size of the study. It would have been useful to include more potential confounding factors in the analysis model, but this would not be feasible given the small Ns.

Limitations regarding the sample size and other confounding factors have been added to par. 3 of the discussion.

Reviewer 3 (02445294)

You wrote that you investigated females with AN and controls matched for age and premorbid intelligence quotient. How have you been able to do so with different sample size for AN and controls? To whom did you match the one more control subject?

This was not a case-controlled study. Participants were not matched on an individual basis, but the two groups were matched based on age and premorbid intelligence. This has been made clearer in the methods section of the manuscript.

What was the rationale for the small sample size used? Did you calculate a power analysis before?

A power analysis was conducted which indicated that a total sample size of 54 is required to indicate a large effect size of 0.6 with an alpha of 0.01

Aren't your first and second references a bit outdated (from '95 and '98)?

The first reference has been replaced by a more recent meta-analysis, though we have retained the second reference as it remains relevant.

On p. 12, 2nd paragr. you refer to expectations in a sense of hypotheses. I would suggest that these hypotheses should already be introduced at the end of your introduction combined with giving some rationale for these hypotheses.

The hypothesis referred to in the discussion ("Contrary to expectations, individuals with AN did not differ from healthy individuals in performance on any other task") was described in the last paragraph of the introduction ("We hypothesised that individuals with AN would show poorer performance on tasks assessing each of the cognitive domains, except the tasks assessing speed of processing and visual learning, where the literature has reported intact performance to date.").

On p. 13, 12th row you stated that the patients were "medically stable". Would you please explain what this should mean and how you have been able to draw this conclusion based on the data available (cross-sectional study).

Medically stable patients were identified as those who did not require medical attention due to their physical state. Patients were screened to ensure medical stability prior to inclusion in the study. These details have been added to the methods section of the revised manuscript.