Author’s response to reviews

Title: Habitual Prospective Memory in Schizophrenia

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To the Editor,

Thank you for the helpful reviews of our paper titled "Habitual prospective memory in schizophrenia" (ID-1727825597130689). We detail below our responses to all questions:

Reviewer: Brebion

Introduction: In the introduction on p.5 (and elsewhere), we now make a clear distinction between 'action' and 'evaluation' errors. Also, the study’s predictions are now explicitly outlined on p.6.

Method: The reviewer suggests examining the relationship of our experimental measures to the presence of symptoms, the assumption presumably being that there might be a positive correlation with the presence of positive symptoms (e.g., hallucinations or delusions), since these include behavior that might be considered to be a consequence of a distortion of events or misattribution of events that never occurred. Of the 20 patients in our habitual prospective memory study, we have PANSS scores (Positive and Negative Syndrome Scale; PANSS - Kay, Fiszbein & Opler, 1987) for 16 of these patients. We note that the primary goal of our study was to explore habitual prospective memory in schizophrenia in general, and thus our analysis of the relationship of symptoms to prospective memory measures is of a post-hoc nature since we did not recruit specifically patients with high levels of positive and/or negative scores. Not surprisingly therefore, the correlations between symptoms and the tendency to make prospective memory errors were not significant (see below). Naturally, we are happy to include these results in the manuscript if it is deemed appropriate, although we believe that reporting such a negative result may give the impression that the relationship to symptoms is non-existent, when in fact the limited range of symptoms in our patient sample considerably reduces our power to detect such a subtle difference (if present). In response to the reviewer's request, we have correlated the total number of 'Action-no/Report-yes' errors and 'Action-yes/Report-no' with positive symptoms (mean = 13.31, SD = 5.04; Spearman's R=.07, p=.8 and Spearman's R=.15, p=.6 respectively), negative symptoms (mean = 19.63, SD = 10.23; Spearman's R=-.11, p=.7 and Spearman's R=.32, p=.2 respectively) and with the general psychopathology score (mean = 26.82, SD = 9.71; Spearman's R=.28, p=.3 and Spearman's R=.27, p=.3).

Results: We appreciate the reviewer's concern over the interpretation of evaluation errors but disagree that this renders the results uninteresting. We would argue that an increase in absolute numbers of such errors in patients is of practical importance and have therefore followed the same analysis procedure as reported by Einstein et al. (1998) who similarly analysed absolute numbers of evaluation errors, despite overall group differences in omissions. As acknowledged in the Discussion, we agree that it would also be useful to study numbers of evaluation errors conditionalized on the numbers of opportunities for such errors, but in order to produce sufficient
numbers of errors in controls, the PM task would probably produce floor effects in patients.

Additional analyses in response to the second reviewer (see below) address the reviewer's query about the elimination of group differences after controlling for backward digit span or current IQ. It turns out that the group difference in omissions remains significant after controlling for digit span or IQ whereas the group difference in evaluation errors does not.

We agree with the reviewer that it would be interesting (and clinically important) to explore the relationship of 'Action-no/Report-yes' errors to the presence of hallucinations. However, as mentioned above, we do not think that we have an adequate range of positive symptoms (including the severity range in terms of hallucinatory behavior specifically) to merit a post-hoc analysis of this. However, we do believe that a future study should examine this specifically, and thus recruit patients accordingly.

Reviewer: Craik

As suggested by this reviewer, we first examined the correlations between the number of omission errors and IQ/digit span in patients. There were no significant correlations. Rather than reporting these correlations, we decided instead to conduct more powerful tests of the reviewer's alternative explanation by performing ANCOVAs on the numbers of omissions, with IQ/digit span as covariates. As we now report in the Results, the group differences remained significant and therefore the PM deficit observed in the patients with schizophrenia cannot be attributed to their low IQ or impaired digit span. These additional analyses provide a neat contrast with the results of the ANCOVAs conducted on the evaluation errors (see Results and Discussion).

In response to the reviewer's reservations regarding our interpretation of different types of errors, we have deleted this section of the Discussion.

We have slightly expanded the sections on PM/RM differences and time- vs. event-based PM tasks in the Introduction and have also referred the reader to an edited book on PM that discusses these distinctions in detail.

We are grateful to the reviewer for his suggested changes to terminology which we have made accordingly.

Rather than presenting a 2x2 table of action/report data, we have added a table of means of action and evaluation errors for the two groups (see Table 2).

We have cut a paragraph from the Discussion.

Reviewer: Miles

Discretionary Revisions

1. We have adjusted our references to the format required by this journal.
2. As requested, we now report exact p values in Table 1.
3. We now report that the game (Kongman) was manufactured in 1982.

Compulsory Revisions

1-2. The reviewer is correct in pointing out that ANOVA was inappropriate for our original analyses with trial as a factor with 10 levels. We therefore consulted the Einstein et al. (1998) article on which the current study was based. They had 11 trials and compared performance early in the task (trials 1-5) with late in the task (trials 6-11) by combining scores appropriately. Our analyses were
therefore repeated with trial (2 levels: 1-5 vs. 6-10) as the within-subjects factor. Thus data were no longer binary but were scores out 5. Also, Table 2 follows Einstein et al.’s Table 1 in splitting early and late trials.

3. As noted in response to the second reviewer, the new terminology should help the reader in understanding the different error types.

4. Table 2 has been added in response to the reviewer’s request.

5. The ANCOVA results have been amended to make it clear that the p-values refer to the significance of the group effects (i.e., patients vs. controls) after inclusion of the covariate in question.

Once again, we are grateful for the helpful reviews that we received of our manuscript, and we very much hope that this revision is suitable for publication in BMC Psychiatry.

We look forward to hearing from you.

Yours sincerely,

Brita Elvevag, Ph.D.