Reviewer’s report

Title: Prospective cohort study of the relationship between neuro-cognition, social cognition and violence in forensic patients with schizophrenia and other psychiatric disorders

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Reviewer: Benjamin Buck

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MAJOR COMPULSORY REVISIONS

1. The authors should explain in greater detail the impact of the fact that there was a subset of participants who were under supervision in the community – this seems to have an entirely different set of circumstances and questions related to this sample.

2. The claim that the current paper demonstrates a prospective relationship between cognitive ability and violence should be revisited. To demonstrate the ability to predict an outcome prospectively, it seems to me that the most rigorous way to approach this would be to regress the follow-up levels outcome on the predictor controlling for some kind of baseline level of the outcome. Did the authors consider predicting violent behavior from cognition while controlling for violence risk or past violence? Without something like this, it seems to me that the primary contribution is that the article demonstrates that individuals that commit violence have more cognitive impairment and greater psychopathology as well. I do think it’s important to compare the effectiveness of various domains in predicting dangerousness (i.e. if MATRICS were more effective than measures of violence proneness in predicting dangerousness, that would be very informative). But given the potential for confounds and third variables, without controlling for baseline of the outcome in one way or another, the present study cannot claim satisfying “temporal and association criteria for causal inference” as the paper claims (p. 23).

3. This paper has a huge number of analyses. I appreciate the use of a Bonferroni correction, but authors should be more clear about specifically how this was done and the specific values that were calculated to adjust the criterion for significance testing. Also, is there a theoretically-justified reasons for examining the subscales of the MATRICS? It appears that there is no justification why any specific subscales would differentially predict violence based on the literature.

4. Was a correction applied to the number of mediation models examined? This number of analyses massively increases Type I error, not to mention the fact that only a subset are conceptually justified by the introduction and previous literature.
5. It’s not clear to me that based on the study design (89 individuals with schizophrenia or schizoaffective, and 15 individuals with a range of other disorders) that this paper is examining a cross-diagnostic model of predicting violence from cognitive variables. This isn’t well justified in the introduction or conclusion (e.g. “to ensure generalizability?”). I also find it concerning that some of the proposed mediation models held in the large sample and not in the schizophrenia/schizoaffective subsample alone. What does this mean for the model of what’s happening here? It confuses the analysis to be conducting both of these analyses together.

In order to achieve this goal, one would need to ensure that other conditions were better represented. Of course, this is limited by the fact that inpatient forensic units (or at least those to which the authors have access) might be overly populated by individuals with schizophrenia or schizoaffective disorder. Nonetheless, running all analyses on a large sample, and then repeating them on a subset that only removes around 15% of the sample seems redundant and unjustified by the aims and background presented in the paper’s introduction.

MINOR ESSENTIAL REVISIONS

1. Generally speaking, some folks from the anti-stigma movement have suggested a linguistic shift from “schizophrenic patient” to “person with schizophrenia.” While I think the authors take a nice de-stigmatizing approach in much of this article (and am by no means suggesting the authors are presenting attitudes of stigma), it might strengthen this even more for the authors to take more person-centered language.

2. p. 6, line 103 – Because of the quality of this evidence, this paper should be explained in greater detail. I’m not entirely sure how to make sense of the authors’ description of this paper. This is a meta-analysis with no support of the relationship between which two variables? And what theoretical reasons caution us from interpreting this paper?

3. I’m wondering about the selection of social cognition variables. There are significant relationships between attributional style and functioning – but a much smaller, and much less consistently demonstrated – relationship between skill-based measures of social cognition and outcome. The introduction could be strengthened by a bit more reflection on the kind of social cognition captured by the MSCEIT-ME, whether this demonstrates a skill or a bias?

4. More should be said about the timing of the assessments – how frequently were these completed? Was the battery collected all together at one time point? Also, who administered the MATRICS and who scored the informant-rated measures. Was inter-rater reliability established, or has it been demonstrated elsewhere?

5. p. 14, line 300 – Is there a reason this was approached as a binary outcome – violent or non-violent?
6. The contribution of the coding of each violent episode as reactive or instrumental is not clearly explained in the discussion. What might this mean for the conclusions of the paper and future studies in this area?

DISCRETIONARY REVISIONS

1. p. 3, line 41 – Just a small observation, but authors should temper language – it’s not clear based on this paper that cognition “determines” violence, but predicts it.

2. p. 5, line 66 – It would be helpful if the authors provided some citations demonstrating the low rates of violence among individuals with schizophrenia.

3. p. 5, lines 80-83 – This point doesn’t build the argument for the current paper, in my opinion; if homelessness, substance misuse, and unemployment might mediate the relationship between cognitive decline and violence, then a study on an inpatient unit wouldn’t be testing this relationship very effectively.

4. p. 6, lines 113-116 – More could be said about what the authors mean by mankind’s increased ability to reason. I’m not clear on how this connects to the themes of the paper specifically.

5. Is there any conceptual overlap between the SOFAS and the measure of violence – e.g. are there items related to violence on it?

6. I’m confused about the correlations with actual violence in the follow-up period. Wasn’t violence in the follow-up period collected as a binary outcome? And if so, could the authors explain to me a bit more the justification of using non-parametric correlations – rather than something like a logistic regression?

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.