Reviewer's report

Title: Tridimensional Personality Questionnaire data on alcoholic violent offenders: specific connections to severe cluster B personality disorders and violent criminality

Version: 2 Date: 31 January 2007

Reviewer: Simone Ullrich

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Re: Tri-dimensional Personality Questionnaire data on alcoholic violent offenders: specific connections to severe cluster B personality disorders and violent criminality

31 January 2007

Dear Sir / Madam,

I apologize for the delayed response to your request for a re-review of the above-mentioned paper. However, your request came in when I was on annual leave and thereafter I had to finish some important work before being able to review the paper again.

I have carefully read the revised version of the paper as well as the remarks of the authors regarding my concerns. My comments are as follows:

Minor Essential Revisions
Most of the suggested minor essential revisions have been addressed by the authors either in the revised version of the paper or in the comments. However, regarding some aspects I would have wished a more in-depth discussion of my objections.

• I still believe that the main focus of the paper is on Antisocial PD which represents only one of four Cluster B PDs (1).
• There might be a limitation of space, but for a better understanding the background of the study could provide more detailed information on Cloninger's model of personality and the rationale of the study (3).
• Administration of SCID-II by an experienced psychiatrist and double-checking does not replace analysis of inter-rater agreement (6).

Major Essential Revisions
• In the first version of the paper it was not clear that the control sample had undergone the same assessment procedure (SCID I and II) as the forensic sample. From a methodological point of view, this should be made explicit. However, it is rather surprising that in 170 controls no participant met the criteria for any Axis I or Axis II mental disorder (7).
• The issue of co-morbidity of Axis I/Axis II mental disorders and demographic variables as potential confounders in statistical analyses has not been addressed by the authors (9).
  ? It is well know that static demographic factors such as socioeconomic status, ethnicity or marital status are significantly correlated with violent behaviour. Although the authors mention that their sample is rather homogeneous, it cannot be ruled out, that differences between high and low HA offenders are due to differences in demographics unless these variables are controlled for.
  ? The co-morbidity of PDs is rather high in both the ASPD (65%) and Non-ASPD offender group (14%). How can the authors guarantee that the reported differences in the TPQ scales reflect real differences between ASPD, Non-ASPD offenders and healthy controls, and that they are not confounded by other co-morbid PDs?

I hope that my comments are helpful for you in reaching a decision about the above mentioned paper.

Sincerely,

Simone Ullrich

What next?: Reject because too small an advance to publish

Level of interest: An article of limited interest
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.