Reviewer's report

Title: A case control study of five factor model personality traits in opioid dependence

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Reviewer: Morten Hesse

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General

The study compares a sample of opioid dependent patients to a non-clinical sample on a widely used measure of the five-factor model (FFM), the NEO-PI-R. The method is generally adequately described. The only reported results are the mean differences in t-scores between the two groups. The meaning of these differences is discussed, along with differences to North-American studies.

However, the study is very limited in its purpose and data analyses. I have listed all my comments in the "Major Compulsory Revisions" section below.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Introduction

The relatively brief introduction covers a lot of ground in discussing both the etiology of opioid dependence and a number of concepts related to personality (sensation seeking, personality disorder), as well as the FFM. However, it might be desirable to focus the discussion more on the relationship between the FFM and opioid dependence. What do the authors believe about the relationship between the FFM and opioid dependence? Also, rather than going into concepts and issues that have little or no relationship to the data in the study, the authors would be well advised to give a brief overview of the relationship of the FFM to other psychiatric conditions, including for instance alcohol dependence and other illicit drug dependence. This would provide some context for the findings.

Method

While the methods section is well-written, the methods used are quite limited. The study is referred to as a case-control study. However, cases and controls are likely to differ in a number of other ways, apart from opioid dependence, and no control is made for such differences. To name a few, employment history, socio-economic status, criminal history, psychiatric co-morbidity. Therefore, whether the study measures differences between opioid dependent populations and non-opioid dependent populations, or between clinical samples, or simply disadvantaged groups, and normal groups is not persuasively shown.

The normal group clearly has representative scores on the NEO-PI-R, and as such the inclusion of the normal group tells us little that cannot be inferred from the t-scores of the clinical sample. Where the normal group might be useful was in fact if confounders, such as socio-economic status, employment history or psychiatric co-morbidity could be controlled for in multivariate analyses. Also, while mean differences is one way to look at a clinical group such as opioid dependent patients, there may be other interesting things to say about it. Several studies have found that there is substantial heterogeneity in the personalities of substance abusers. One way to illustrate this heterogeneity is to give the percentage of opioid dependent patients scoring above or below the 95th percentile of the normal population. This would give readers an impression of the "prevalence" of extreme traits in the opioid dependent sample. Another option would be to do a cluster analysis of the opioid dependent sample to get an impression of the material.

The opioid dependent sample is described as coming from a trial of pharmacotherapy for opioid dependence. But what were the exclusion and inclusion criteria? This may have impact on the findings. And the description of the sample is very limited in terms of other data than the fact that they were seeking opioid substitution treatment. How much other substance use did they report at intake? How many had been in prison? How many were employed, married, cohabiting?

Discussion
The discussion is, like the introduction, very broad in its focus. To me, one issue seems to be under-valued: the difference between Norwegian and North American findings. Why were the Norwegian opioid dependent sample so different? Was it something about the inclusion/exclusion criteria, or are Norwegian (possibly European) drug misusers simply different from North American? Maybe the North American samples included a lot of court ordered patients? This could explain why the North American drug abusers were more extroverted and antagonistic?

On the other hand, the authors discuss life-time trajectories of personality traits, and how these could be affected by a drug-using lifestyle. This discussion appears highly speculative, and should be omitted, since there are no data in the study to shed light on these issues.

Whatever the authors choose to do, I would like to see a bit more in-depth discussion of the specific findings of the study, and less speculation.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

None - the manuscript is well-structured.

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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.