Author's response to reviews

**Title:** "Do European psychiatry trainees base clinical decisions on evidence?"
Decision-making and antipsychotic choice in European trainees - a
cross-sectional survey of the European Federation of Psychiatric Trainees’
Research Group

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**Author's response to reviews:** see over
Dear Editor,

Please find attached the revised manuscript, with amendments. We have addressed each reviewer's comment point by point, as requested.

We would like to thank Dr Kawouhl for his clear and helpful review.

1. “Parts of the manuscript are hard to understand...it seems they have not been thoroughly corrected by a native speaker.”

Drs Sameer Jauhar and Neil Masson, native English speakers, educated in the United Kingdom, reviewed the manuscript with regard to English usage. We hope the manuscript now reads in a more clear fashion.

2. “It is unclear how the recruitment of the participants has been conducted.”

We have addressed this, adding to the Methods section, indicating that this was an opportunistic sample, taken at the discretion of the international representative.

3. “it is not comprehensible why 50 participants per country had to participate given the different sizes of psychiatric trainees in those countries.”

Fifty was set as an arbitrary number in light of the difficulty in recruiting trainees from each country, for an unfunded study. We readily accept that a sample of 50 from Portugal would have more validity than a sample of 50 from England, which has a significantly higher number of trainees. We have added this to the limitations of the study.

4. “Why were different countries not included? Please explain.”

We have added in the Methods section the reasons why other countries that originally participated were unable to provide samples for the overall analysis. This was broadly due to the poor sample collected within the country.

5. “It would be interesting to know whether there are specific differences between the countries concerning the choice of antipsychotic.”

We have conducted exploratory chi square tests to ascertain if this was the case, and found significant differences for trainees form Holland, which have been added to the Results section.

6. “The statistics part must be improved. In the statistics, more influencing factors should be included. Multivariate statistics, such as factor analysis may be more helpful than the Chi square tests used.”

The use of multivariate analysis to improve the statistical analysis, were considered for further analysis of the data (addressed in point number 5).

Given that the variables of interest – both dependent (type of medication) and independent (countries) were dichotomous categories (that were mutually exclusive), it was thought to be inappropriate to use factor
analytic techniques like a principle components analysis or a discriminant function analysis. It was also considered inappropriate to use logistic regression, as membership of a country (independent variable) was mutually exclusive.

Considering the dichotomous nature of the data and exploratory nature of the analysis, it was thought that Chi square tests would seem the most judicious and simplest form of analysis. As this was an exploratory analysis we did not think that controlling for multiple comparisons was necessary (nevertheless, the countries that differed significantly in their choice of antipsychotic use, survived the bonferoni family wise error – multiple testing correction for 12 variables at 0.0042). We feel that, to address our research question, these techniques were justified.

Minor essential revisions

1. “Were the participants asked or screened for mental disorders they had or have themselves? Were they asked about mental disorders in first-degree relatives?”

This is an important point that we neglected to ask about, as it was felt that this would adversely affect the completion rate of the questionnaire. We fully acknowledge that this may well have had effects on the answers given, particularly in regard to the question pertaining to psychotherapy.

2. “ECT is still used and should not be equated with insulin coma therapy.”

We have changed the introduction to reflect this.

3. “The tables should be adapted to the new statistics.”

We would hope that our explanation of the statistical techniques used, and explained above, is satisfactory.

We would like to thank Professor Priebe for his clear, insightful and helpful comments.

1. “I am not sure whether the use of the term ‘decision-making’ is appropriate.”

We fully agree with this point and have changed the title, and use of this term within the manuscript. We have made reference to previous work on “decision-making”, as we feel this is relevant; although we have been careful to make a distinction between our work, and other work that is more focused on the decision-making process.

We have also added the questionnaire itself, in an appendix.
2. “In my version Table 5 is missing”

We have ensured that Table 5 is provided, and apologise for its original omission.

3. “the sample size would be sufficient to explore whether treatment preferences vary across countries, and are associated with gender.”

We have conducted Chi-square tests, and found that trainees in Holland were statistically more likely to both prescribe typical antipsychotics for their patients, and request them for their own treatment, than trainees from the rest of the sample. Possible reasons for this are discussed. No difference with gender was found.

4. “the description of the methodology is inconsistent....how was the question (on influence/awareness of trials) exactly worded”

We have added the question as it appeared in the questionnaire in Appendix 1.

5. “the qualitative analysis looks like a simple content analysis to me, but not enough information is provided to assess this. not enough information is provided to assess this. The analysis is unlikely to have anything to do with a grounded theory approach as claimed in the description of the analysis. I suggest the authors consult an expert in qualitative methodology, describe properly what they have done, use an appropriate terminology and provide more data to interpret the findings. The latter will probably include the frequencies of how often different categories have been mentioned.”

We have amended the analysis to reflect this. One of the authors (NM) was recruited to the study on account of his experience in qualitative analysis. The term “grounded theory” has been taken out, and replaced with content analysis. The original responses were not significant enough to analyse in greater depth. Frequencies are provided.

6. The selection of “a representative group of trainees” in each country remains unclear. Representative for what? What do “trainees from a similar institution” mean? If there was really an attempt to random sample, this
should be described clearly. If however this was just an opportunistic sampling (which it looks to the reader, it should also be stated”.

We have amended the manuscript to reflect that this was an opportunistic sample, taken from trainees that the national representative had access to.

7. “Whatever the precise interpretation of the results I feel the conclusions can be sharpened. It is true, as the authors say in the discussion, that European trainees appear to base their treatment preferences on factors other than evidence-based medicine. However, the discussion should go a bit further. The study did indicate some of the reasons the preferences of trainees many of which seem to be pure myth (depending on where one stands on evidence-based medicine). Trainees simply argue with irrational or false assumptions which are an important finding that the conclusions should acknowledge and consider.”

We welcome this advice, and have re-worked the discussion to take this into account, including a number of publications that have dealt with this issue. We feel that this is an important issue, and thank Professor Priebe for highlighting this.

Dr Sameer Jauhar, Dr Rajeev Krishnadas on behalf of the EFPT research group.