Author's response to reviews

Title: Discontinuation of hypnotics during cognitive behavioral therapy for insomnia

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Author's response to reviews: see over
Dear Dr. Furukawa,

We appreciate very much your positive opinion on the revised version of our manuscript. All of your comments, as well as the comments of primary reviewers, have been of extraordinary value to us.

Regarding the individual points in your review:

1. Language. We have asked San Francisco Edit, a professional company that edits and proofreads scientific manuscripts for submission to peer-reviewed journals, to review the manuscript.

2. Possible major depression. There was no major depression among the patients. We have realised that the formulation of the text was unclear, and have described the exclusion criteria more precisely.

3. The terminology: not “hypnotic dependent“. Thank you for this comment! In our own language, the meaning of the expressions “abuse”, “addiction” and “dependency” are somewhat obscure. We agree that it is necessary to use well-defined terms in a scientific paper. According to the reports of the National Institute on Drug Abuse at NIH, the term “abuse” is most appropriate in this case. The text has been corrected to reflect this.

4. Benzodiazepine successfully withdrawn in all patients? The intention of the study was to investigate effect of a single drug withdrawal, uncomplicated by other drugs or medications. As a rule, there is a waiting time of several months for treatment at our unit. We used the policlinic contacts during this period to encourage patients not to use any other medications other than, if necessary, zopiclone. We completed the text and added a sentence on this to the Discussion section.

5. Table 3 needs to show all variables. We have completed the table in its new version.

6. CGI is scored 1 to 6 but the median in the paper is higher. Many thanks for this comment! We applied our own (and, as we realised later, non-standard) version of CGI. We do agree that a conventional application of internationally standardized scale must be used in a peer-reviewed paper. Therefore, we have recalculated the data so that it now corresponds to the conventional application of the CGI scale.

7. Only one number of IQR. Again, we realise that we originally used a less conventional way of presentation (which we have seen in some papers) that shows only one value corresponding to the “length of the interquartile range” instead of two numbers describing the value of first and third quartile. We have changed it in this revised manuscript.

In addition to your points above, we have also completed the Abstract according to the instruction of the Editorial Office.
Let me thank you once more for your efforts in reading the manuscript and to providing valuable comments. We appreciated them, and we have revised the manuscript accordingly.

Yours, with best regards,

Lucie Zavesicka