Reviewer’s report

Title: Compliance, persistence, costs and quality of life in young patients treated with antipsychotic drugs: results from the COMETA study

Version: 1 Date: 16 October 2012

Reviewer: Jan Volavka

Reviewer's report:

Major compulsory revisions

1/ In the Subjects and Procedures section, the authors state that patients who were “drug addicted” were ineligible. It is necessary to explain what criteria were used to define “drug addiction”. Furthermore, it is not clear if this includes alcohol use disorders.

2/ The project used 86 participating centers, and an unknown (but obviously large) number of raters administering various assessment instruments. Was there any attempt to determine interrater reliability, for example, for the PANSS scale, and for other instruments? How were the raters trained?

3/ “Persistence with the antipsychotic drug treatment was estimated by means of Kaplan-Meier curves, by comparing the mean number of patient-days (i.e. number of days each patient persisted in each treatment) of permanence in the same drug class (i.e. class of atypical, typical, combination of both classes, or no antipsychotic drug)” (Stat section). This is an unusual method of defining persistence. With this method, it was possible for a patient to switch for example from risperidone to olanzapine and then to clozapine and he would still be considered persistent since all 3 switches occurred within the same class (atypicals). This method may explain, in part, the unusually high persistence rate for the atypicals depicted in Fig 3. Such results are of course not comparable with those obtained using in other studies (e.g. the CATIE) that counted each switch as a discontinuation, no matter what the patient was switched to. This problem should be explained in the Discussion section.

4/ In the Background section, we read that “The aim of this study was to assess compliance and attitude toward antipsychotic drug treatment, persistence and clinical status and Health-Related-Quality-of-Life (HRQoL)…”.

In view of that aim, it is puzzling that the authors limit the data presentation for these variables to displays of averages in Figures 4-6. Measures of dispersion are missing.

Without appropriate testing, we do not know which, if any, of these “trends” achieve statistical significance. These tests should be done, and the presence or absence of stat. significance should be reported in the text and reflected in the Abstract. The current Abstract states that “the attitude toward treatment
improved”. This statement which implies an unequivocal effect is misleading in view of the minimal change observed.
In any event, measures of dispersion around the means must be provided

Discretionary revisions
1/ I would replace Figures 4-6 with a table that would give means, standard deviations, and significance levels for each of the time effects (“trends”) (after appropriate corrections for multiple testing).

2/ I would omit the Conclusions section. As it is, it consists of general statements such as “How patients with schizophrenia are treated can have important repercussions…”, These statements are true, but not new. Furthermore, they do not follow in any clear way from the currently presented data.

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

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interest