Reviewer's report

Title: Comparably high retention and low relapse-rates in different subpopulations of bipolar patients in a German non-interventional study: how physicians optimize outcomes by adjusting medication regimens to individual patients' needs

Version: 1 Date: 10 December 2012

Reviewer: Amy Price

Reviewer's report:

1. Major Compulsory Revisions:
   The basic premise of the article is that this observational study shows how in a naturalistic setting patients are doing better than what is reported in RCTs because "physicians optimize outcomes by adjusting medication regimens to individual patients' needs." Patients who experienced medication regimen changes were considered censored from the analysis, so the physician optimization part of this article is not apparent to this reader. Why were the patients who changed mood stabilizer therapy in the course of the study censored, just as those who dropped out or died were? That seems to contradict the concept of the naturalistic setting in which clinicians must change therapy in the event of an adverse drug event.

   Olanzapine is well known to cause weight gain, hyperglycemia, and alterations in lipid profiles. There is no discussion of the latter adverse events for this or any of the other cohorts which at this point in the literature appears to this reader to be inadequate.

   The cohorts in the study are all generally compared to olanzapine monotherapy as if it were a gold standard. The use of the OM group as an active comparator in many ways (such as Fig. 2a) should compel the authors to consider a reference to this in both the title and the abstract.

2. Minor Essential Revisions

3. Discretionary Revisions

   Were the patients in the study enrolled continuously or could there be selection bias based on more motivated patients seeking to enter the study? Were patients with characteristics more likely to succeed being included? This was not clear in the methods section. Some discussion later in the paper done, but a further delineation of the selection of patients is warranted.

   Language suggestions: Second paragraph under safety and tolerability in the methods section: tick list? Would probably suggest "check list" but this wording may be more appropriate in the British audience. Baseline characteristics paragraph: sentence starts with 36.4% spelled out. This is confusing, but
perhaps a style suggestion by the journal.

Other methodological concerns:

Can you report the clinicians' assessment of patients adherence to treatment (as described in the methods section)?

Can you include the comorbid medical and psychiatric demographics in the baseline characteristics (since these often guide medication selection.)

Can you relate the use of antidepressant medication to medication discontinuation?

The reporting of data in Tables 3 and 4 using odds ratios is confusing; some are less than one and yet still purport to favor a variable, such as the CGI-BP < 3 with an OR of 0.118- it would seem more relevant to supply the inverse such that the OR is greater than 1. This reader would recommend independent expert statistician review of the data to screen for potentially inflated claims.

**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, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.