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:** 26 November 2012

**Reviewer:** Lars Kessing

**Reviewer’s report:**

This is a clinically important and interesting study on the naturalistic outcome of bipolar patients. The study seems well conducted and the paper well written. Nevertheless, the paper may be improved in a number of ways.

Major Compulsory Revisions:

The title is a biased version of the findings, as there are other explanations to the results, such as the selection criteria of stabilization, as also acknowledged by the authors.

Similarly, regarding the Abstract:

The Conclusion should be revised in accordance with the above mentioned point. It should be stated in the Method part that patients were included if they obtained remission.

It is not correct as stated in the Results part that there was “no notable differences” in retention rates for different drugs. The authors found (as presented at p. 15) that patients “treated with OMS and AM were more likely to discontinue their medication than in OM. Patients treated with LM had about a 2-fold chance to continue their treatment compared to patients treated with OM”.

It is indicated that “The primary objective of our study was to assess the time on different mood stabilizing medications and retention rates in standard clinical care. Further objectives addressed relapse rates, patient adherence, and tolerability”. It is not entirely clear what is the difference between adherence, time on different mood stabilizing medications, and retention rates? Please revise and give more precise and separate definitions of each term.

Under Statistics, reasons for censoring should be presented in relation to survival statistics. It is not clear whether censoring was done at the time of change to another mood stabilizer? (add on or/and switch?), death, emigration, end of study, etc. ?

The authors adjusted their Cox-regression model for a number of important variables except the most important: duration of prior mood stabilizing treatment.
on the given drug. The authors discuss duration of prior mood stabilizing treatment as an important factor but do not adjust for it. These data must be available and I suggest adding data from such analyses.

In the Background section, more updated studies on adherence should be provided such as a nationwide study on adherence to lithium (Kessing LV et al (2007). Adherence to lithium in naturalistic settings – results from a nation-wide pharmacoepidemiological study. Bipolar Disorders, 9, 730-6).


In the limitation section it should acknowledged that authors were employed by Lilly except one. Did this in any way influence the design and findings of the study?

Level of interest: An article of outstanding merit and interest in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

Lars Vedel Kessing has been a consultant for Bristol-Myers Squibb, Eli Lilly, Lundbeck, AstraZeneca, Pfizer, Wyeth, Servier, Janssen-Cilag.