Reviewer's report

**Title:** Severe imported falciparum malaria among adults requiring Intensive Care: a cohort study.

**Version:** 3 **Date:** 26 November 2012

**Reviewer:** Michael Ramharter

**Reviewer's report:**

The validation of prognostic markers and scores for the management of severe malaria is of high clinical importance. Important differences in the clinical presentation and management in endemic versus non-endemic regions are the well justified rationale for this retrospective analysis of two previously published scoring tools. The study methodology is well conceived and the data are well presented. Please find my specific comments below:

**MAJOR:**

The title implies that this study is a cohort study. However, to my understanding this is not the case since participants were chosen based on the outcome under investigation (in this case severe malaria) and not prior to exposure. Please reconsider this classification.

**ABSTRACT:** The statement that HIV co-infection is common (8 cases) and bacterial co-infection uncommon (7 cases) in this patient population seems not very objective. Please reconsider this statement.

Besides CAM and MSA another internationally established scoring tool (Lambarene-Organ Dysfunction Score; Helbok et al. J Infect Dis.) has been proposed and the manuscript would greatly benefit from inclusion of this score in the analysis.

The clinical presentation of patients is very well described and highly informative.

**MINOR:**

The use of abbreviations is at some points confusing. For example "AKI" is used in the abstract without introducing this abbreviation. Personally I advise to refrain from all abbreviations if possible.

Given the wealth of data and the detailed clinical and laboratory description of patients, the authors could set out to identify risk factors for adverse outcome in this cohort. Several studies have shown that age is associated with malaria related deaths in returning travelers and similar associations may be considered for pre-existing co-morbidities, initial parasitaemia, schizontaemia, etc. Such an analysis would help to identify prognostic markers for this patient populations.

Discussion “Previous studies particularly from France … have reported a mortality rate between 7 and 25%. It is possible that the lower mortality at this
hospital could be attributed to considerable experience in the management of severe malaria acquired over many years."

The excellence of the HTD as an institution and its individual staff members in the management of tropical diseases is beyond doubt, however other reasons than “considerable experience” – a statement implying superior clinical management – for lower mortality in this patient population than in other cohorts (including the French cohort) may also play an important role and should be discussed in more detail (differences in disease severity, differences in classification of severity or in decisions to transfer patients to ICU units between countries, etc). The mortality in this HTD cohort is comparable to what we observe at our centre in Vienna, Austria (Auer-Hackenberg et 2012 Malaria Journal). However, I would disagree to imply from this observation that the lower mortality at our institution is due to “considerable experience” compared to the highly experienced French colleagues.

**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:**

'I declare that I have no competing interests