

Authors' response in BLACK.

The manuscript has definitely been improved by the valuable comments of the reviewer. We have made effort to accommodate all observations and recommendations of the reviewer in the revised manuscript. Below we provide brief responses to the major concerns/suggestions of the reviewer.

The argumentation relies strongly on correlations and coefficients of variations without stating whether the considered variation is spatial or temporal. Oftentimes both options are possible, and hence it is not possible to follow the conclusions. I had stated this previously, but the problem remains. In the current form, the conclusions are not supported by the presented information.

Throughout the revised manuscript we have made effort to indicate the nature of the variations the study focuses on. The study mainly focuses on the temporal variations both in terms of temporal trend and magnitude. This is demonstrated in terms of the analyses based on phenophases and climatic seasons which are temporal in nature. While we acknowledge that no detailed spatial analyses have been performed, we however, made effort to show the differences in the spatial distribution of evaporation across phenophases (i.e., temporal comparison) (example, please refer to section 3.6, Figs. A7 & A8 of the revised manuscript). For the objectives of this study, we are of the opinion that this is sufficient. For future studies, we would appreciate recommendations from the reviewer as regards the detailed spatial-temporal analyses.

The discussion does not link the results well to the conclusions.

We appreciate the observation and have addressed this in the revised manuscript

There are repeated excursions to the literature that do not lead to synthesis. At the same time, the discussion of the ET products makes little reference to the own observations of canopy structure, especially the camera observations.

We appreciate the observation. In the revised manuscript we have address this issue. We have linked the camera observations to the temporal trends in satellite-based evaporation estimates.

The manuscript is still poorly structured, starting at the sentence level to the order of paragraphs and subsections.

We took note and revised some sections of the manuscript, especially the results section.

The text is wordy, there are frequent repetitions (I have not pointed them out this time), and several sections could be shortened to 50% without losing information.

We appreciate the observation though to achieve 50% reduction has been a huge challenge. We, however, have revised some sections and components of the manuscript. Additionally, some components, like in the materials and methods sections, were only lengthened as a response to reviewers' earlier comments.

The order of the sections is sometimes confusing, the headings do not correspond well to the content of the subsections (e.g., headings in subsection 3.8x).

We appreciate the observation. However, it is difficult to follow the comment that subsection headings and the contents do not correspond well. The contents under each subheading are specifically speaking to that particular subheading. Once again, this section was added in response to reviewers' comments. The initial manuscript did not have this section as we only made reference to the discussions in Zimba et al. (2023). Nonetheless, we have revised some parts of section 3.8 (now section 3.5).

The red thread is difficult to find, and there are many unnecessary excursions. It is difficult to follow.

In our opinion, the red thread is the fact that this study appears to be the first to focus entirely on the miombo woodland eco-hydrological interactions and the potential to influence the performance of a number of satellite-based evaporation estimates. This in itself is extremely important especially in the miombo region where there is a scarcity of field observations and satellite-based data is seen as the best available alternative, especially in the management of water resources. An inter-comparison of satellite-based evaporation estimates, like this one, is important in providing information on which satellite-based estimate(s) is/are potentially close to the field conditions of the miombo woodland. We believe this study has made effort towards highlighting the uniqueness of the miombo woodland and providing some information to bridge the gap in understanding the evaporation of the region.

Zimba et al. (2023) (which was actually written after the earlier versions of this manuscript) was a point-based study and did not cover all phenophases of the miombo woodland. This manuscript covers all phenophases of the miombo woodland at the Luangwa Basin scale and took into account multi-year variations in the satellite-based evaporation estimates.

We also believe that the results of the comparison of satellite-based estimates with the water balance-based evaporation are insightful. The results appear to agree with what has been generally observed in other basins in Africa. We now have an indication of which satellite-based evaporation estimates are potentially close to miombo woodland conditions i.e., the WaPOR.