We know the observations and remarks by the reviewer will help improve the quality of our manuscript. We have made effort to respond to the issues raised. Our responses are in blue. Additional responses are in the attached document on HESS Discussion platform

This is a study of importance. The water use of Miombo Woodland is poorly studied and this paper could make a significant contribution to addressing this gap in knowledge. However, in its current form, I do not believe that the paper is publishable. In particular, there are some major assumptions which introduce so much uncertainty in the analysis that the results produced cannot be assumed to be representative - leading to a lack of confidence in the interpretation and conclusions. For the the basin scale ET analysis:

- the assumptions made in producing annual estimates of rainfall and runoff need to be much better justified and the way in which the values used better explained. However, even if this is done, I believe that this would highlight a flawed approach, so I suggest that in teh cas eof the basin scale analysis that the author's need to "go back to the drawing board".

  We believe the approaches used are appropriate and good enough for the objective we intended to achieve. While we acknowledge that the approaches are not error free, we think that actually our approaches are meant to minimise errors.

  We explain below what we mean.

  Field observations of spatially distributed rainfall data are not available because the Luangwa Basin is sparsely gauged. It's practically impossible to have these data at basin scale. It is why satellite-based rainfall products have been used in this study. For rainfall, satellite-based rainfall products showed mixed performance of underestimating and/or overestimating rainfall. There was not a single satellite-based rainfall product that performed consistently, underestimating or overestimating, when compared to point scale field observations at four weather stations in the Luangwa Basin. We think the actual rainfall values are somewhere in between the two extremes of underestimation or overestimation. By getting an average of several rainfall products we observed that the magnitude of error of underestimation or overestimation was minimised.

For the run-off data we could not provide all the information on how the time series were extended because we were avoiding producing an extremely length manuscript. However, we believe the procedure we used to extend the time series gave us closer to field observations time series of run-off. The TerraClimate run-off data is used as a predictor to extend the run-off time series for the period 2009 - 2020 which did not have reliable field data. The TerraClimate run-off data showed significant correlation (Pearson r = 0.83, *p-value* < 0.005) with observed run-off data though it overestimated run-off (average 50 mm year^{-1}). However, estimates of run-off using the TerraClimate as a predictor are closer to field observations (RMSE = 27.9 mm year^{-1} and MBE = 21.89 mm year^{-1}). The regression equation using the TerraClimate run-off data as predictor is cross validated with field observations. This is the basis of its use as predictor in our study.

For better understanding of the regression procedure used in this study more information can easily be included in the manuscript. We have added the information in the revised manuscript.

The results of the comparison of water balance-based evaporation to satellite-based evaporation estimates are only indicative of the possible performance of satellite-based evaporation estimates. It is, however, true that more robust approaches are needed if the results are to be definitive. More field observations are needed. Nevertheless, for the purpose of this study and to the extent that this study in itself raises these questions, we think our approaches are sufficient.

- the process where an assumed uniformity of Miombo Woodland is assumed and how this influences basin level ET estimtes is quite unclear to me. In addition, the assumptions of extend of Miombo vs Mopane Woodland seem incorrect based on recent studies in the catchment.

This leads to an analysis where ET is derived from (I believe) flawed estimates of rainfall, runoff and land cover leaving little confidence in the annual values produced. To then use this as a basis for comparison with ET estimates from RS products seems a step to far to me.

We acknowledge that the Luangwa Basin has heterogenous woodland cover the largest being the miombo woodland and the mopane woodlands. Of these two the miombo woodland is by far the largest vegetation cover. It is true that the results at basin scale can not entirely be attributed to the difficulty of obtaining accurate evaporation estimates of the miombo woodland using satellite-based approaches. What our study intended to show was that satellite-based evaporation estimates potentially underestimate actually evaporation of the Luangwa Basin regardless of the vegetation type. Based on our approach, regardless the flaws, our study has

showed one possibility, that satellite-based evaporation estimates potentially underestimates evaporation of the Luangwa Basin. The result of our study is an invitation for the use of more robust approaches to prove otherwise.

However, what we think should be done is to provide more information on the approaches we used for rainfall and run-off data at basin scale. We have done this in the revised manuscript.

The link to phenophase through the year and it's spatial variation is a stronger part of the paper, although the assumptions around extent of the Miombo Woodland and it's realtive homegeneity between different parts of the catchment need more careful analysis.

By its very nature the miombo woodland is heterogenous. No two locations of the miombo woodlands are the same. They differ in species composition and other environment factors such as soil moisture, nutrients and rainfall thereby facilitating the heterogeneity. What we think would help is by focusing on known miombo woodland sites at pixel level. Taking into account the dry miombo woodland and wet miombo woodland stratification. We can then observe if the trend and magnitude of satellite-based evaporation estimates substantially/or significantly differ in these two categories. We can also compare the results for the two known miombo woodland sites with the basin scale miombo woodland. At basin scale only the areas "officially" mapped as miombo woodland should be considered. This is what the study endeavored to do though it appears we need to provide more information and be clear on how the delineation of the miombo woodland was conducted.

I provide more detailed comments in the annoated attachment.

We have responded to the annotations in the attachment

RESPONSES TO REVIEWER 2

We have made effort to respond to each point that was raised by the reviewer. We find the observations and remarks very helpful. Our responses are indicated in blue below each remark by the reviewer. The issues raised by the reviewer have been address as indicated below and in the revised marked up manuscript.
This manuscript investigates different products of gridded evapotranspiration (ET) over an extensive but little gauged ecosystem type, the Miombo, in Africa. Little hydrological data is available in the region, but evapotranspiration estimates are required for management decisions. The manuscript compares different ET products most from

remote sensing and one from a water balance with each other and adds own qualitative observations on phenology to shed light on why they differ from each other. This is a valid way forward in my opinion although the manuscript remains rather descriptive when it comes to phenology. Local plausibility checks of existing global products are a fist way forward to improve information for management in dats scarce regions and important work. The manuscript is valuable in this respect. There are substantial critical points however, which I have pointed out below. The main point relates to the lack of information on methods, which is so substantial that currently I cannot confirm the validity of the conclusions. This may potentially be cured in a revision. In addition, I think the manuscript could provide much more (i) background on the type ET products (ii) known sources of uncertainty in (i) that could be linked to the unique phenological stages observed in the Miombo, and (iii) provide some discussion about how common this problem is either globally or in neighbouring ecosystem types in Africa. Finally, the manuscript really needs revision for structure and I strongly encourage the experienced co-authors to provide guidance to the first author.

We are grateful for the comprehensive review of our manuscript. The observations and insightful comments made will greatly help improve the quality of our manuscript.

Main comments

(1) I found it difficult to follow the methods, and specific information is required to allow assessment of the results and discussion.

☐ I do not understand the extrapolation in of the runoff data to be used for the water balance model based ET-estimate. This is highly relevant, since the latter is used later as a contrast to compare different ET products. The methodology on how the data was derived needs to be fully stated. Now it is arbitrary and renders this method useless.

The TerraClimate run-off data is used as a predictor to extend the run-off time series for the period 2009 - 2020 without reliable field data. The TerraClimate run-off data has significant correlation (Pearson r = 0.83, *p-value* < 0.005) with observed run-off data though it overestimates run-off (average 50 mm year-1). However, estimates of run-off using the TerraClimate as a predictor are closer to field observations (RMSE = 27.9 mm year-1 and MBE = 21.89 mm year-1). The regression equation using the TerraClimate run-off data as predictor is cross validated with field observations. This is the basis of its use as predictor in our study.
For better understanding of the regression procedure used in this study more information can easily be included in the manuscript.

☐ Standard deviation and coefficient of variations are used to evaluate difference within and between products. I did not follow how this was done. With ET varying both in space and in time and between datasets, I was unable to decipher in most instances, across which dimension the variance was calculated? Therefore, I am not able to follow the conclusions in several instances. Also, standard deviation and coefficient of variation are redundant, and I propose to use e.g. only the coefficient of variation.

When comparing variables with the same scale, i.e., satellite-based evaporation estimates, the use of the standard deviation is preferred. However, when comparing variables with different scales i.e., temperature, rainfall, NDVI and LAI the use of the

coefficient of variation is preferred. However, as observed by the reviewer, it is a matter of preference. We can use the coefficient of variation instead.

(2) The manuscripts main point is to compare different ET products in an underrepresented ecosystem type and to highlight where they are uncertain. The paper remains very descriptive in parts. Despite the very valuable information delivered at one location, it does not go much into depth. It would be really helpful, if the reasons for the uncertainty and similarity of the those products was discussed in more depth. Some information already appears in the discussion, e.g. mentioning the lack of ground water access in models like GLEAM, but even more background wold be desirable. For example, the methods section could give a short background on how the products are derived focussing on how inherent assumptions on phenology play in, or which LAI products are used.

We would be glad to include all this information. However, the reason for omission in the earlier manuscript was that readers can make reference to the cited documents. That's why the information only appears in the discussion. Also, part of this information is available in the companion paper (Zimba et al., 2023). Nonetheless, we can easily add this information to make the manuscript more informative as suggested by the reviewer.

(3) The manuscript is difficult to navigate. The text is partly fragmented (e.g. similar information is spread out over several sections). The order of presentation was somewhat confusing. There are several repetitions (I have pointed out some below in the detailed comments), and many sentences are complicated and long. It would be good to revise carefully for structure both at the manuscript as well as at the sentence level.

We appreciate the observation and can easily make the necessary revisions to make the presentation more organised.

General editorial comments

The Miombo is an extensive savannah ecosystem, but not all readers may be aware of it. It would be good to set the stage right from the start, including in the title, the abstract and introduction. Personally, I have not known this specific ecosystem before (and I am thankful I learnt it here). I propose adding a short introduction about savannah biomes in general, and how they are differentiated in Africa specifically. This would greatly help the reader to understand that this is not a site specific study, but represents an expansive ecosystem class on the African continent. Also, are there other equally underrepresented ecosystems that face similar problems with potential misinterpretation of satellite products due to insufficient representation of the local phenology?

We take note of this important observation. We agree on the need to avail more information concerning the uniqueness of the miombo woodland.

Detailed comments

I had a hard time counting the line numbers. I may be off one line sometimes in the comments. I hope the appropriate places can found found nevertheless.

We have made effort to respond to the issues raised throughout the comments.

Abstract

Lines 18-23: Would be good to give a short explanation about what the Miombo is, mentioning its extent and the complex phenology. Would be good to show that this is not a case study in the first sentences. Also, why is ET required.

We agree concerning the observation. The manuscript can be revised accordingly.

Lines 26, 29 „variation": I am unclear what variation is meant and therefore the message of the sentence is also unclear to me.

This is a valid observation. The variation in the trend and magnitude of evaporation estimates. We will make effort to explicitly state this throughout the manuscript.

Line 32-35: The wording of the sentence suggest that ET measurements were done. But those were not presented here. Is this a reference to the phenological information?

These are basically satellite-based estimates and not field based estimates. The statement clearly says "satellite-based evaporation estimates".

Line 36 „caused"

To be revised accordingly

Line 37 „leaf fall, leaf flush" - Many ecosystems have leaf fall and leaf flush, which aspects indicate the adaptation? Can you add a word to specify?

Unlike in other ecosystems the leaf fall and leaf flush in the miombo woodland occur simultaneously. This is one of the key distinguishing characteristics of the miombo woodland's plant species canopy display behaviour from other ecosystems.

Line 38: What is meant with „within vegetation water storage"?

Plants have developed mechanisms to store water to buffer the dry season conditions (Tian et al., 2018; Vinya et al., 2018)

Line 38: „heterogenous plant species response" I undestood only after reading the ms what was meant here. Can you rephrase?

We appreciate the observation. This will be rephrased.

Lines 39-43: I find this message unclear. Do those products exist? Should they be developed, existing ones adapted?

These satellite-based products already exists. We will revise the abstract to include the names of the satellite-based evaporation estimates used in this study.

Introduction

As mentioned in the main comment above, it would be good to give a bit of background on the ecosystem, its size, and how it differs from the surrounding ecosystem types.

We have taken note of this important suggestion. We will revise the manuscript to include these aspects.

Line 54: associated with variations

Line 56: which „conductance" is meant ?

Canopy conductance is a dimensionless quantity characterizing radiation distribution in tree canopy.

Line 56-63: Long and complicated section. Rephrase?

Will rephrase as suggested.

Line 67-68: This is an attribution of a very old hydrological concept to a recent reference. Can you rephrase, e.g. „here we are adopting the same definition as" and/or find a suitable text book reference.

This is an important observation. We will revise the statement and provide the appropriate citation.

Line 78: influences

To be revised accordingly

Line 92: also

To be revised accordingly

Line 92-93: „Leaf flushing .. „ I m not sure in which context this is meant?

The plants acquire new leaves in the dry season. The formation of new biomas requires water but this process is experienced during the driest part of the year.

We will make effort to be more elaborate and clear.

Line 89-110: If I understand correctly, this section implies that remote sensing products are validated in regions, where ground truth is available. This may cause a bias as it puts a focus on ecosystems other than those located in the global South? If yes, und assuming there are some more examples, it would be good to formulate this paragraph more generally. Otherwise it sounds like a case study.

This is understood and will be revised.

Material and Methods

Line 111: replace „formulated" by „conducted" ?

Either word can be used

Lines 138-141: Sounds much more like introduction. Move up?

We agree that the manuscript requires reorganising. This will be done.

Lines 145-147: It is not entirely clear what it was „sufficient". Can you specify.

We used the word "sufficient" because in the study area a 12 year period is likely to experience a wide range of cycles of climate conditions such as low and high rainfall years. Looking at the historical time series the cycles in the period 2009 - 2020 were assumed not to likely be different from the historical cycles (30 year cycles) especially rainfall.

Section 2.2

I was unclear whether the phenophases were defined as part of this study? The word „field observations" suggest that. If yes, I propose moving them to the results section. Also explain how they were determined / observed. Alternatively, move content of now section 2.11 up here and merge. There is a lot of overlap in content between those sections.

The "observations" involved taking images to document the changes in the colour changes of the leaves of the canopy that represents different phenophases.

We agree that the manuscript requires reorganising. This can easily be done.

Figure 1: I found it hard to read this figure, which includes a time axis, definitions and the phases. Would be good to move the definitions out of the figure and into the main text. The different shades of green in the top line (phenophase) and third from top (Period of the hydrological year) do not match and it is therefore unclear what they mean. I propose to leave out the third line and turn it into a simple time axis.

Figure 1 is important for understanding the dominant activity in the different phenophases. However, we agree that it needs refining for easy understanding. We will rework the figure and improve on it.

Line 154: Not sure what is meant with „dominant activity".

There are several activities taking place simulataneously such as the leaf fall, leaf flush and greening up. Depending on the phenophase, especially in the dry season, there is a more prevalent activity than the others.

Line 170: I do not understand what is meant with the header, also the first sentences do not explain what the delineation does and for what purpose it is required. Please add some more information.

The observation is noted and more information will be provided. It basically shows how the area for the study was determined. The Luangwa Basin is composed of other woodland types such as the mopane woodland. The study is focused on the miombo woodland component of the basin. "Delineation" in this case is simply how the miombo woodland part of the basin was determined. Note that, despite having other woodlands the miombo woodland is the largest vegetation cover of the Luangwa Basin.

Line 181: I do not understand what is meant with „put the basin boundary Miombo Woodland generalisation into context"

As explained above the Luangwa Basin has other woodlands apart from the miombo woodland. Additionally, we wanted to focus on an area with the highest canopy closure throughout the year so that the actual evaporation can be attributed largely to the miombo woodland and not other vegetation types such as grass. For the dry season the actual evaporation can largely, if not entirely, be attributed to the transpiration of the miombo woodland plants.

We believe, in agreement with the reviewer, this section of the study requires more information. We will clearly show the differences in the trend and magnitude of evaporation in the two strata of the miombo woodland in the Luangwa Basin.

Lines 183-185 and 188-89 and Line 197-198: are repetitions. Also, not sure why this comparison is mentioned, but maybe it is the motivation why the delineation is required. Please give more information and move up to the beginning of the section.

This can easily be done

Lines 192-194: Better move to introduction.

The manuscript will be reorganised

Line 213: I do not understand „This was because these products are normally applied as is, in their original resolutions." Can you be more specific?

We did not resample the satellite-based evaporation estimates because this would have introduced errors. Additionally, we wanted to observe if there was correlation between spatial resolution of the satellite-based evaporation estimates and the spatial resolution. Normally, in water resources management applications, the satellite-based evaporation estimates are not resampled. However, those with fine spatial resolution are normally preferred because it is assumed they perform better. However, it is not known if actually this is a valid assumption. For instance, according to the results of our study there is no significant correlation between spatial resolution and the evaporation estimates. This is because, especially in the dry season, satellite-based evaporation estimates with coarser resolution (i.e. FLEX-Topo-based estimates) appear to have higher estimates of actual evaporation compared to the satellite-based evaporation estimates with finer spatial resolution (i..e., those based on MODIS and SSEBop).

Line 220: „acceptable": Acceptable or not, depends a on the situation. Better state that this is the choice you made, as there were no other option. Also the water balance technically also requires distributed data, please be more specific.

Well noted. We will revise the manuscript accordingly.

Line 222: What is meant with: „ver-year storage change is neglected"

Over year soil moisture storage is not considered in the equation

Sections 2.6 and 2.7: I understand that the precipitation and runoff details given in sections 2.6 and 2.7 are mainly related to the water balance in section 2.5? If yes, it would avoid confusion, if those section were subsections of 2.6.

Valid suggestion. We will revise the manuscript accordingly

Line 232: What is meant with: „geographically biased"?

The biases are dependent on the location due to the heterogeneity in the landscapes

Lines 231-233: The message of this section is unclear. Some are bad, some have some good correlations, but than all are used anyway? Also, this section is a repetition of Lines 236-239. Would be good to merge.

Well noted. The sections will be revised accordingly

Line 241: Do you mean „to this end"?

Yes. This will be revised

Line 245: „that they had desirable spatial and temporal resolutions" sounds a bit arbitrary. Can you be more specific on what was desirable?

Well noted. This will be revised

Line 248-249: This extrapolation seems really important. The method is completely unclear. How were the data extrapolated by linear regression? What as the dependent and independent variable in this extrapolation? Furthermore, shortly state why it was necessary.

This has been explained above. We initially felt that including this information would simply lengthen the paper. However, it appears it is important information that should be added. We will revise the manuscript accordingly.

Line 258: „Denver camera": Unclear description of methods. Please make sure manufacturers are clearly stated. It sounds like Denver camera is an established name, but what you probably mean is you installed a camera (Product name, Denver A/S) ?

This was an oversight. The appropriate information will be provided.

Line 262: Similar comment as above for the fisheye lens - was it added to the same camera?

This was an oversight. The appropriate information will be provided.

Eq 2: Mu and sigma are typically used for theoretical distributions, not descriptive stats. I propose using bar over x and s .

Noted. Will be revised accordingly

Eq. (3) Maybe just use B here

The suggestion is noted and will be considered.

Section 2.10

The header „Data sources". Can you be me more specific here? Data sources were already previously disussed, e.g. in sections 2.4, 2.6, 2.7, 2.8 ..

This will be revised.

The section describes mainly the FLEXTopo model, which is good information to have to interpret the results. It would be good to give background information about the other products here too.

Our initial thought was that the manuscript would be too lengthy if we included the information for other satellite-based evaporation estimates especially that there is sufficient document in public domain. We feel references to these publications in public domain should be sufficient other than describing the various satellite-based

evaporation estimates in this paper. The FLEX-Topo background information was added because the estimates were simulated based on data and identified processes specifically for the Luangwa Basin. We feel references to other documents in public domain is sufficient for these other products.

Line 287-288: „FLEXTopo" used without previous introduction

This will be revised

Lines 293-301: Description of the model is difficult to follow, as it starts with the groundwater cells before stating this is one of several storages modelled, some distributed some apparently not.

We felt the information was well presented. However, we can revise this section so that it is easier to understand.

Line 292-293: „within a grid cell to which all HRUs are connected." To me this is unclear. Are there different HRUs within a grid cell? A bit mit information would help.

We felt the information was well presented. However, we can revise this section so that it is easier to understand.

Lines 295-296 „Furthermore, each storage component has been structured as a reservoir with matching water balance equations." and Lines 301-302: „However, the limitation with the FLEXTopo was the 27.7 km spatial resolution" - unclear. I do not understand what is meant.

We felt the information was well presented. However, we can revise this section so that it is easier to understand.

Section 2.11 would be good to have this information much further up. Also as mentions above, there is a great deal of overlap with section 2.2. Would be good to move up and merge.

Lines 396-397: Those references are also the sources for the information in Fig 2, correct? If yes, add them there (too).

Yes they are. Will revise accordingly.

Lines 403-414: Specifically here there seems to be a great deal of overlap with Section 2.2 and does not need be mentioned twice.

This part will be revised

Results and Discussion

Fig 3: I was really unclear what is plotted here? Does „aggregated" mean „climatology", only one year is shown but the year is 2019. Alternatively, is this a spatial aggreagation? Also which precipitation and temperature are shown (Table 1 gives 4 options)? Where is the specific humidity taken from? I think relative humidity would be more intuitive and give a better indication of water deficit. Finally, satellite based soil water content refers to which depth in this case? Does it add information?

There were errors in the labels and the level at which the soil moisture was taken was not included. This can easily be revised. The necessary information as suggested above will be provided.

Line 451: „climate" would be better to say „atmospheric"

A matter of preference. We take note and will revise accordingly

Line 464-465: unclear „The peak climate and phenological variables values were generally observed in the rain season, green-up and maturity/peak phenophases." and „lowest values" can you specifiy.

Yes we can specify. This will be revised accordingly.

Lines 471-483: This is about methodology and should be moved to the Methods section, Lines 476-478 are a repetition of Lines 176-179.

Will be revised accordingly

Line 489: Coefficients of variations are very difficult to see in Fig 5, would be good to have a table.

The suggestion is noted and will be revised accordingly.

Line 490: Actually, the correlations are rather poor, which points to the opposite? Would be good to discuss those poor correlations as well and potentially move table to main text?

The correlations are poor at both basin scale and point scale. This is the similarity talked about here and not that the correlations are high. It was the basis for only focusing on pixel based comparison. The possible explanation for the poor correlations has been discussed but possibly not adequate. We will discuss these correlations a bit more.

Line 491: I am not sure what is meant with: „variations within each satellite evaporation product"? Spatial or temporal variation? Intra or inter annual?

It will be best to revise this part and be more specific. We will revise accordingly

Lines 495-496: This is a change of topic and interruptive. Phenophases have been discussed in 3.1, merge there.

Well noted. This will be rearranged

Line 496: „relatively higher correlation" - This is difficult to see from the table. Also it is an important message and would be good to have some aggregated information in the main text. For example, you could add a figure showing the correlation coefficients in a box plot (one box per season) and add this to the main text.

This is a very good suggestion. We will add this information.

Line 436: „(Fig. 5)" Do you mean Fig. 6?

Yes. It was an error

Lines 538-540: This discussion of Fig 3 should be moved up or a better connection be made, why we need to remember this here.

Suggestion noted and revision will be done

Line 543: „significant differences" - I believe you mean "substantial". Would be good to replace to avoid confusion with the statistical nomenclature requiring significance tests.

Good observation. We will replace the word

Line 549-550: „effectively account for rainfall interception at the commencement of the rain season." - It find it difficult to understand the logic of this statement without further information. Can you explore? Do you mean that the interception part of actual ET depends on leaf area index and therefore misrepresentation of LAI may reflect on estimates of actual ET?

Yes. This will be be revised to reflect the importance of correct representation of vegetation type/ LAI in estimating evaporation.

Figure 5: Please add y-axis labels. Please state clearly what is shown on left and right panel? What is meant with „aggregated" and „mean" ?

The mean for each month for the period 2009-2020

Line 580: Please make sure abbreviations / symbols for standard deviation are the same throughout the manuscript, including the equations. Eq. 2 used different ones.

Valid observation. We will revise accordingly

Line 580-581: This information is required earlier, maybe already in the Methods section. Also be please be more specific, e.g. what is the sample when you say between phenological phases? Finally, why is is necessary to use two different measure of variations? Why not just use coefficient of variation throughout?

This will be reorganised. The use of the standard deviation and coefficient of variation has been explained above.

Lines 582-585: I am left unclear what the percentages in the text refer to? Is this coefficient of variation? Of what? For example, what is meant with „showed the lowest differences (7.83 %)"?

Yes it is the coefficients of variation. We will make it clear.

Lines 596-598: Difficult to understand, can you rephrase?

We will rephrase as suggested

Figure 6: The Figure description does not match the figure content. Please re-organize and make sure to mention the content of panel (c) separately from (b). Also, I found it confusing that (b) shows standard deviation and (c) shows coefficient of variation (CV). I am also not sure, which sample CV is calculated for? Is this inter annual variation?

We will reorganise the information and make clear the aspects that could be confusing.

Line 636-638: Can you explain how you see this from your data?

The legend shows which phenophases each line represents. The data points in the plot corresponds to each satellite-based evaporation estimate. The phenophases with high coefficients of variations of evaporation estimates are clearly shown.

Line 639: „significant variations in precipitation" - Which variation is meant? Variation within the same year, between years?

Within the same year in different phenophases.

Section 3.2.4 Great to see mentioned which variation is meant (spatial). I found it hard to understand what "variation" meant in the previous section. Now I am assuming it probably meant temporal variation. Correct? Please specify.

Yes, variations in the temporal trend and magnitude of evaporation.

Line 647: „forested upland Miombo" - Can you add basic delineation here, e.g. where is the forested upland region roughly?

This is based on the digital elevation model (DEM) in Fig. 2. However, for better presentation we will revise the map so that the extent of the miombo woodland in the Luangwa Basin will be overlaid on the DEM.

Line 679-680: What is meant with „user accuracy"?

User accuracy refers to extent what is classified on the map agrees with the real world land surface characteristics (i.e., forestland). For a given class, how many of the pixels on the map are actually what they say they are? More information can be found in various land cover classification documentations including Lillesand et al. (2015) (Remote Sensing and Image Interpretation, 7th Edition)

Line 678-679: The mentioned drivers of transpiration are textbook knowledge. Please cite a textbook here.

This was an oversight. There is adequate information in public domain on this aspect. The appropriate citations will be provided.

Line 680-681: I think this would be a place to give more background information. For example, can you show those differences and/or have a reference showing in a similar region that (a) that land cover classification can differ substantially between products, and (b) have a substantial influence on the ET estimate. Also, what are probably causes for those different land cover classifications?

We felt the information provided was adequate. However, realising the importance of the differences in the landcover products on the evaporation estimates we acknowledge that more information and citations are needed. This will be provided.

Section 3.3 I would have liked to read at least the first part of this section (up to line 718) at the beginning of the results, as it gives a background on the small scale spatial heterogeneity of the canopy over different phenophases. Also, any quantitative information would improve this section, e.g. information changes of greenness or heterogeneity thereof in the fotos in Fig 9 left. The information would help explain the spatial variation and differences between products in sections 3.2. The second part, discussing consequences for ET estimation (e.g. starting at Line 751) could remain here or become a last part of section 3.2.

Suggestion is well noted. The section will be revised accordingly.

Line 696-701: This is a repetition to section 3.1 and would be another reason to move this section up.

Well noted and will be revised accordingly.

Lines 749-751: This statement has to be accompanied by a reference or phrased like an assumption.

Well noted. Appropriate reference will be provided.

Fig 9: Please differentiate between left and right panel. Also, what is the sample for the standard deviation in LAI and NDVI? Is this across pixels in the Miombo Forest?

Yes this is across pixels at the wet miombo woodland site in Mpika, Zambia. We agree this requires more detail and better presentation. This will be done.

Section 3.4 I would have liked to see the first part (up to Line 893) of this section before section 3.2. It would be much easier to appreciate the discussion on the lack of groundwater access in GLEAM.

Well noted. This can easily be done. The need to improve on the general organisation of the manuscript is noted.

Line 828: I do not understand what is meant with „below average aggregated annual mean bias".

Basically, the aggregated mean is the mean of all satellite-based evaporation estimates. The mean of each satellite-based evaporation estimate is compared to the average of all the satellite-based evaporation estimates.

Fig 10: Please ass variables to the y-axis labels. I think you could remove precipitation from panel (b) and thus only show bias there. Please make sure to separately state what is shown in panel (b) and (c).

Well noted and will be revised accordingly

Conclusions

Line 923-924: I still do not know how the coefficient of variation shows this.
It has been explained above. Coefficients of variation in the trend and magnitude of the evaporation estimates.
Line 961-964: This part is discussion and not conclusion. Wold be goo to move up.
Well noted and will be revised accordingly.
Reply