Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/hess-2022-114-RC2, 2022

Comment on hess-2022-114

Anonymous Referee #2

Referee comment on "On the importance of phenology in the Miombo ecosystem: Evaluation of open-source satellite evaporation models" by Henry Zimba et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-114-RC2, 2022

Review of "On the importance of phenology in the Miombo ecosystem: Evaluation of open-source satellite evaporation models"

The paper presents a very interesting study on evaluating different remote sensing products for a specific ecosystem found across Africa. The paper contains a wealth of data and information but the presentation of the study and its results require significant improvement as currently it is not very clear to me how the methods used for the evaluation results in the overall conclusion. In addition to a critical review of the English language required, I have provided detailed annotated comments on the manuscript and below the general observations on the manuscript:

Title and abstract

The title in my opinion is not correct, shouldn't open-source be open access?; and you are evaluating evaporation products, not the models itself (eg ETLook; SeBAL)? Also from the abstract it is not very clear that you are not only evaluating RS evaporation products, but also two (hydrological?) models. If you evaluated all 6 products, then the title is incorrect, but if you evaluated the 4 RS evaporation products, and used the other two models for validation, then the abstract needs to be re-written (line 21-25). Also I am not sure that the importance of phenology is reflected in the study and results for the evaluation of the evaporation products.

Methodology for evaluating RS evaporation products

Generally there is a bit of unclarity which products are being evaluated and against which data. For the annual comparison you compared the evaporation products (all 6) to an

estimation of evaporation based on a water balance (Ewb), to which I have the following comments:

- Ewb was calculated for calendar years, not hydrological years which may influence the over year storage variations, better to reduce this additional source of error by comparing the hydrological year
- The selection of precipitation product used for the Ewb calculation seems to be based on 8 observations in three locations (Figure A1), this is a very limited validation and the data should therefore be considered with certain uncertainty and not seen as an absolute reference to which the other products need to adhere to (you already indicate that the absence of the over-year storage is reducing the value of Ewb as a validation product
- The explanation of the Ewb validation data (incl selection of the precipitation product) can be presented fully in the methodology (and not have two sentences at the start of the results sections, which seem a bit out of place/ duplication "sensitivity of the precipitation product")

Regarding the trend analyses of the evaporation products:

- To me it is not clear what the purpose of the trend analyses and correlation analyses is, the reference data set (Ewb) has more variation than the evaporation products being evaluated, what does this mean? The fact that there is no significant trends (for 12 years of data, which is a too small data set) for the different products, what does this mean?
- Trend analyses at monthly timescales seems to be done for the entire dataset (144 months) instead of comparing similar data (jan alone), if you do take the full timeseries, the trend analyses is influenced by the seasonality of the data, how do you account for that?

Regarding the correlation analyses and inter-comparison of the different products:

- TopoFlex and TMC seem to be used as standard to compare the other products against (eg figure 6 & 10) however later in the paper it becomes clear that these products also have their limitation to model evaporation.
- For example figure 7 shows the mismatch between the NDVI and the spatial patterns of the TopoFlex and TMC models. The higher variability observed by SSEBop and WaPOR shows the basins is most likely due to the higher resolution of the data, including being able to identify water bodies with high evaporation
- Not clear to me how the NDVI spatial pattern is used to evaluate the spatial patterns of the different evaporation products? The text now seems to be descriptive.

Comparing to phenophases at basin level

The first analyses done are to compare the entire basin evaporation data against the Miombo phenophases, however we only find out in the discussion that only 60% of the basin is covered by Miombo woodlands (at least if we assume open forest classification is all Miombo woodlands). What is the justification for this assumption and would you assume that the entire basin would respond in a similar way as the Miombo woodlands?

Comparing the five selected locations (Figure 8)

The six selected locations vary in size as can be seen from the number of pixels used from the WaPOR data (256-2304), however for the low resolution data, each time only one pixel is considered in the analyses. It can be assumed that for the smaller areas, these pixels overlap with the surrounding areas, which could have different land cover types which may have influenced the temporal signature. How has this been taken care of. In table A5 you indicate the same number of observations for each of the locations, how did you aggregate the WaPOR data (average?) to compare it as one value against the other datasets?

Discussion

In the discussion section a lot of new data is presented, this is not normally a good place to present new data and analyses. For example figure 11 presents new data, but to me it is not very clear how this contributes to understanding how well the different evaporation data products are able to monitor the Miombo woodlands. Similarly the sections with the explanations on how SSEBop and WaPOR perform contain a lot of new information on how the Miombo woodlands work and which is used to confirm that the evaporation observed by SSEBop and WaPOR at the end of the dry season are not unrealistic. In my opinion it would have been helpful if this information was presented upfront, including figure 13 with the land cover classes observed in the Luangwa basin.

Specific comments:

You categorise the remote sensing evaporation products into energy balance models (EBM), however the WaPOR methodology is not a surface energy balance model, instead it uses Penman-Monteith (ETLook) for estimating evaporation.

Update graphs to remove the digits (eg 100.0 should be 100)

Figure 5 title of 5B monthly average (year 2009-2020) and not 2019-2020

Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2022-114/hess-2022-114-RC2-supplement.pdf