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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Improved SWAT vegetation growth module for tropical ecosystem" *by* Tadesse Alemayehu et al.

Anonymous Referee #2

Received and published: 27 March 2017

Review of hess-2017-104 by Alemayehu et al.: "Improved SWAT vegetation growth module for tropical ecosystem"

The authors develop a new vegetation growth module for tropical ecosystems in SWAT. In particular, they use a soil moisture index to initiate a new growing cycle within two pre-defined months. They evaluate the growth module with regard to LAI, ET, and river discharge with satisfactory results. The topic is of current scientific interest, as several authors have previously outlined that the default vegetation growth for e.g. forests in SWAT is not applicable in the tropics. The manuscript is mostly well prepared. However, the paper would benefit a lot if it was more structured according to the evaluation of the vegetation growth module. In particular, this applies to the results and discussion part. This focus should be set very clearly in a revised version. Moreover, some parts of the manuscript require further, more detailed, or more precise information. The pro-

vided comments should be addressed before accepting this manuscript for publication.

General comments:

1) The authors should take a decision on the aim of the manuscript. Do they aim at providing an improved plant growth module for SWAT in the tropics? Or do they aim at showing their adjustment of the model to a specific catchment? Currently, the title reads like the first is the case. However, in many parts the paper reads like the latter is the case. For the first aim the authors need a stronger focus on evaluation of the plant growth model and they need to program the module as flexible and transferable as possible. Right now there are parts that are not immediately transferable to other catchments. The authors are encouraged to sharpen the paper with regard to the first aim. However, if this was not their aim they may also go for the second aim and adjust the title accordingly.

2) Clear separation of calibration and validation period is required. This also applies to the calibration of plant parameters.

3) A clear calibration and validation strategy regarding the different parameters used for calibration and validation is needed. E.g. right now, it is not clear which parameter was calibrated first and why?

4) LAI is prescribed based on satellite data (l.256). Why is this necessary? This makes the model less flexible and non-transferable to other catchments without following a similar approach.

5) The third chapter "results and discussion" is sometimes hard to understand as discussion of model parameters and results is mixed with model validation. I strongly suggest to rework the structure and separate the "results" from the "discussion" part.

6) Why is it necessary to prescribe the two month in which the growing season starts?

7) It would be very good, if you could validate the modeled begin of the growing season using independent data. Is there any data that you have available to do this?

Line specific comments:

l.7: SWAT is a hydrologic model. The term “simulator” is not very common for SWAT in the literature. Suggest to replace this by “model” in the whole manuscript.

l.13: “uses of a simple...” Please improve the language.

l.15: Would be good to include information here, how the dry season is defined.

l.18: “flow” – The authors probably refer to stream flow. Should be more precise throughout the manuscript.

l.19: Please include information, which RS-ET was used.

l.20: “could be. . .” Please be more precise. In which situations is it useful?

l.44: Please be more precise, i.e. “dormancy, which is defined as a function of daylength and latitude”.

l.47: As I read it, they do not report a shift, but shifted the dormancy period to a prescribed dry season (see p.1786). Please improve the statement.

l.49-55: You are reviewing tropical regions. However the Kalahari has a subtropical climate. Please improve.

l.73: “phonological”

l.67-77: These lines include a lot of information on methodology. Please shift the methodological parts to the methodology section.

l.103: “SWAT uses a GIS based interface”. Not precise. You can use GIS to prepare input files for SWAT. Please improve.

l.126 following: Please add citations for formulas.

l.128: Grammar.

l.134: “endo”

l.170-173: This passage is not quiet to the point. SWAT does not offer heat unit scheduling to solve the issue of plant growth in the tropics. In fact, both scheduling options will not help, as long as the temperature dependant dormancy period is still activated. Please improve.

l.190-197: How are SOS1 and SOS2 defined? By a threshold, or by the increase of the SMI? Are they set by the user? If they are set by the user, the model is not as flexible – is this necessary? It should be highlighted in the other parts of the manuscript that the start of the growing season is not fully dynamic but triggered within a pre-defined period.

l.211: Please add a reference for the DEM (also in table 1 & add time period for river discharge in table 1).

l.217: Please add a reference for the SWAT land use codes, so that non-SWAT users can look these up.

l.222: Abbreviation “TMPA” is not explained at first mentioning. Please improve.

l.226-27: Please add some (short) reasoning for this adjustment, so that the reader can understand the idea of this approach without reading the referred paper.

l.237: Which forest biomes? What about others?

l.238: What do you mean by “land cover mix”?

l.238: Grammar.

l.238: Please add the sizes of the homogenous sites.

l.242: Please provide reasoning for selecting the threshold value 1.5.

l.242: Are these gaps resulting from the previous masking?

l.243-245: Sentence not clear. Please improve.

l.266: Is this measured NDVI or remote sensing based? If it is remote sensing derived,

are the two products independent from each other?

l.271-273: Please include how large the gauged headwater area is. Also, add information on when the gaps happen, e.g., at similar times in the year or mainly in one year?

l.275: This is not precise and could be misleading. As I understand it, the SMI triggers the growing season within a predefined period of 2 months. Please improve.

l.275-281: The model calibration and validation strategy is not clear. The authors use stream flow, LAI and ET. However, it is not clear which parameter is used first – or are they combined? Please improve this section.

l.287: Peaks in April and August are not shown in Figure 4. Please clarify.

l.291: Please add the "drier months" you are referring to in brackets.

l.285-299: Why are you showing tea in Fig. 4? It is shown but not mentioned here.

l.302: Again, this is not precise and could be misleading. As I understand it, the SMI triggers the growing season within a predefined period of 2 months. Please improve.

l.308-310: The authors use long-term MODIS LAI to parameterize the model. It would be much better if these values were derived from the calibration period, so that calibration and validation data are strictly independent. Even though, I do not expect a pronounced change in LAI values in calibration and validation period, I would recommend to only use data from the calibration period for model setup.

l.313: Not clear why the authors express the amplitude of simulated LAI as a percentage of the average annual MODIS LAI. Please clarify your validation strategy.

l.316: Why are simulated and remote sensing based LAI not directly compared and shown as later presented in figure 10? That is what the reader would expect at this point. A scatter plot is also useful in this context.

l.320-323: This passage can be shifted to a discussion part.

l.323-325: This is only an effect of your modeling. Can you use this for validation? E.g. by comparing to independent data (e.g. satellite derived) on the beginning of the growing season?

l.337: But with a correct definition of ALAI_MIN it would not be 0, right? Please make clear, what you are validating. This improvement is due to satellite-based improvement of the parameter ALAI_MIN and not because of the improvement of the plant growth module.

l.332-341: You need to explain your different model setups in more detail. E.g. IGRO=1, will not be understandable for non-SWAT users. Moreover, please use only meaningful model parameterizations. It does not make sense to compare SWAT-T to a model that does not work properly. Many different model setups are irritating. I would suggest to compare SWAT-T to the best possible model parameterization achieved without code changes.

l.342-347: Not sure, why this paragraph is provided here. Figure 5 was already discussed before. Moreover, the comparison to an uncalibrated model is not a fair evaluation (see comment above).

l.352: "standard SWAT" Please define this and provide a model evaluation for this setup. If this model does not work it is not useful for comparison.

l.353-354: "better realism". Please improve the language.

l.354: Did you test for significance?

l.368: Quantify where possible.

l.363-369: This short paragraph presents some of the most valuable results. Please provide further details here. E.g. in which periods and why do MODIS LAI and simulated LAI not match well, as shown in Fig. 10?

l.374-375: It is hard to see from Fig. 11 whether ET values match well as the lines overlap. Please add a scatter plot.

l.384: Contradiction to the previous sentence. Do you mean “grassland” instead of “forest”?

l.385: But Figure 5 shows a seasonality for both? This is contradictory to the previous sentences. Please clarify.

l.387-395: Again the structure is not clear. The authors evaluate parameter values at this point. Why is this needed and presented here? Please provide some justification or remove the paragraph.

l.404: Sentence not clear. Please improve.

l.405: Please quantify the spatial variability.

l.396-406: Why do you not compare the spatial distribution of simulated ET and LAI to the spatial distribution of MODIS based ET and LAI? That could be another valuable comparison that might be more useful than a presentation of modeled values.

l.417-452: The conclusion should be shortened so that it only includes the most important conclusion drawn from your study.

l.432: This sentence is misleading (“default parameters”). As I understand your setup SWAT-T parameters were calibrated. Please clarify.

l.434: What do you mean by “potential transpiration”? Potential evapotranspiration?

l.435: The results with the other PET method were not shown. Please focus on what you have shown in this paper. If it is important, please include it, if not please remove.

l.438: Misleading statement regarding SMI initiation of growing season, see also earlier comments. Please improve.

l.441: “conformed”

l.447-452: This is more suitable for a discussions section. Please shorten or remove.

l.451: “could be. . .” Please be more precise. In which situations is it useful?

l.451: “carbon fluxes” Not shown. Please remove.

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