Response to Anonymous Referee #2

We thank the referee for this thorough review and for the numerous constructive suggestions. We agree with the need of further improvements in the manuscript organization. We therefore have addressed all the comments below point-by-point (italics). We have attached as supplementary material the revised manuscript with changes in blue.

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

Response: We thank the anonymous referee for his/her time and the valuable comments and suggestions. We have addressed properly all the comments and suggestions in the revised manuscript (in blue). Below we provided our point-by-point response (in italics).

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.

Response: This is an interesting point, thank you! The main goal of this manuscript is to present and demonstrate a methodology on an improved SWAT vegetation growth module for tropical condition. The modified growth module can be applied anywhere in the tropics and a user can also add region specific information such as the transition months (SOS_1 and SOS_2), the number of days for rainfall (P) and reference evapotranspiration (PET) aggregation to compute the soil moisture index (SMI= P/PET) and the minimum SMI threshold for triggering new growth cycle within a predefined period. We therefore have sharpened our revised manuscript in this regard.

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?

Response: For the sake of convenience we combined the responses for comments 2 and 3. As noted by the referee, the calibration and evaluation approach was not stated clearly in the manuscript. The revised manuscript has now included a dedicated section (section 2.8.2 lines 290-305) that elaborates on the calibration and evaluation approach.
"…The main purpose of this study is to explore the potential of the SMI to trigger new vegetation growth cycle for the tropical ecosystem within a predefined period annually. We initially evaluated the effects of the vegetation growth module modification by comparing against the standard SWAT model growth module with varying management settings. This analysis involved uncalibrated simulations of the SWAT models with the default SWAT model parameters, meaning the models differs only with how vegetation growth is simulated. It is worth noting that the aim of these simulations is mainly to expose the inconsistencies in the vegetation growth module structure. Afterwards, we calibrated the parameters related to the simulation of the LAI, the evapotranspiration and the streamflow manually by trial-and-error and expert knowledge for the SWAT-T model. Firstly, SWAT parameters that control the shape, the magnitude and the temporal dynamics of LAI were adjusted to reproduce the MODIS LAI at 8-day for each land cover classes. Then, we adjusted parameters that mainly control streamflow and evapotranspiration (ET) simulation simultaneously using the daily observed streamflow and 8-day ET-RS. Perhaps, the manual adjustment may not be as robust as an automatic calibration as the latter explores a larger parameters space. However, the manual calibration is sufficient to illustrate the impact of the modification on the vegetation growth cycle and its effect on the water balance components. The SWAT-T model calibration and validation was done for 2002-2005 and 2006-2009, respectively."

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.

Response: The results from LAI prescription is not shown in the manuscript. Therefore, we have removed in the revised manuscript.

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 reworking the structure and separating the "results" from the "discussion" part.

Response: We accepted this suggestion. As a result, we substantially restructured the "results and discussion"part in the revised manuscript (line 314-483). Briefly, we have presented uncalibrated LAI simulation results from SWAT growth module with and without modification. The purpose of this section is to highlight the limitations of LAI simulation with the existing SWAT vegetation growth module and the added value of the new SMI based modifications. Afterwards, we have presented calibration and evaluation results for LAI, ET and streamflow are presented back-to back using SWAT-T model.

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

Response: This is interesting question, thank you! The two months are assumed to represent the transition from the end of the dry season to the beginning of the rainy season. These months are determined based the climatological rainfall (P) and reference evapotranspiration (PET) data. The main purpose of these months is to avoid false starts during the dry season short rainfall episodes as well as during the rainy season short dry spells. These months are in fact defined a priori and varies geographically depending on the climate.

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?

Response: In fact, the SOS dates are mainly controlled by the SMI variations and the effect of setting the transition months a priori is rather minimal given the season change is not immediately occurring with the start of the first transition month (.i.e. SOS_1). This dates can be verified with SOS dates extracted from remote sensing-based NDVI timeseries. However, we believe it is sufficient to show the simulated inter-annual dynamics of the SOS dates for two reasons: i) Since the SWAT-T model is calibrated against MODIS LAI, using the NDVI derived SOS dates may not be considered as independent data and ii) our study area is a typical data basin and hence such detailed verification would be more interesting in a basins with better forcing data. Therefore, we acknowledge the need of further research in this regard. We have added in the revised manuscript the need of verification of the SOS dates (line 427-428) that reads as"…..Yet, we acknowledge the need of further verification studies in basins with sufficient forcing data and field measurements."

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.

Response: Modified accordingly throughout the revised manuscript.

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

Response: We have improved the language in the revised manuscript that reads as (line 12-13) "…..we present a modified SWAT version for the tropics (SWAT-T) that uses a straightforward but robust soil moisture index (SMI)…".

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

Response: Given the word limit in the abstract, we could not include extra information on how the transition months are defined. However, we have further elaborated the rationale on how to determine the transition months (SOS_1 and SOS_2) using long-term climatological P and PET data in the revised manuscript (line 180-205). In short, the dry season is defined based on climatological P and PET data, whereby the PET is considerably higher than the P (shown in Figure 2 page 8).

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

Response: Updated accordingly throughout in the revised manuscript.

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

Response: We have slightly modified the text to reflect the type of ET source in the revised manuscript (line 18)”…. a thermal-based evapotranspiration (ET-RS) estimate……”. Nevertheless, *We have provided a brief description in the data in section 2.5 (line 254-264) about the ET-RS data based on Alemayehu, T., Griensven, A. van, Senay, G. B. and Bauwens, W.: Evapotranspiration Mapping in a Heterogeneous Landscape Using Remote Sensing and Global Weather Datasets: Application to the Mara Basin, East Africa, Remote Sens., 9(4), 390, doi:10.3390/rs9040390, 2017.*

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

Response: We agree with the referee suggestion. We have updated this in the revised manuscript with information about the applicability of the tool (line 19-20) that reads as “The SWAT-T model with the proposed improved vegetation growth module for tropical ecosystem can be a robust tool for simulating the vegetation growth dynamics consistently in hydrologic model applications including land use and climate change impact studies.”

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

Response: Thank you. We have updated this in the revised manuscript.

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.

Response: Thank you for spotting this. We have corrected this in the revised manuscript (line 50-52) that reads “Likewise, Wagner et al. (2011) reported a mismatch between the growth cycle of deciduous forest in the Western Ghats (India) and the SWAT dormancy period, and they subsequently shifted the dormancy period to the dry season. ”

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

Response: We disagree on this comment with the referee. Jolly and Running (2004) used two site Maun and Tshane sites, respectively located at -19.93^0 and 24.17^0 latitude to evaluate the BIOME-BGC simulated phenological development. The authors reported tropical climate for the study area (P.307 reference in the manuscript).

l.73: “phonological”

Response: Corrected in the revised manuscript.

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

Response: We agree with this suggestion, thank you. We have moved that to the methodology section in the modified manuscript.

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

Response: We have rephrased this in the revised manuscript.

l.126 following: Please add citations for formulas.

Response: We have added Neitsch et al. (2011) in the revised manuscript.
l.128: Grammar.

Response: We have corrected this in the revised manuscript.

l.134: "endo"

Response: Thank you for spotting this. We have corrected this.

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.

Response: We agree with the referee suggestion. We have further improved this in the revised manuscript (line 173-176). This reads as "…..SWAT offers several management settings for the start and the end of growing season based on either heat units (the default) or calendar date scheduling. The default management setting in SWAT is scheduled using heat unit fractions, whereby planting (start of growing season) and kill (end of growing season) occur at FR_{PHU} values of 0.15 and 1.2, respectively. In fact, the limitation with plant growth dynamics cannot be solved using SWAT management settings as far as the latitude and daylength dependent dormancy is activated…… "

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.

Response: We appreciate these important questions from the referee. The transition months (i.e. SOS_1 and SOS_2) indicate the end of the dry season and the beginning of the rainy season. These months should be determined using the climatological monthly P and PET ratio (i.e. the SMI). In principle during the dry season months the SMI values are low since the PET exceeds the P considerably. In contrast, during the rainy months the SMI values are relatively higher compared to the dry months. Therefore, the user should select the transition months guided by the climatological SMI values. We acknowledge some degree of subjectivity in fixing the months and yet, the climatological transition months from the dry to the rainy season are often known. The aim of fixing the SOS_1 and SOS_2 is to avoid false starts during the dry season due to short spell rainfall episodes. The new growth cycle is triggered dynamically when the SMI exceeded and/or equaled a user defined threshold within these pre-defined months. We have further clarified this in the revised manuscript.

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

Response. We have provided this information in the revised manuscript.

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

Response: We have provided reference in the revised manuscript.

l.222: Abbreviation "TMPA" is not explained at first mentioning. Please improve.
Response: Updated in the revised manuscript.

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.

Reponse: Thank you for the suggestions. We have provided information on the adjustment in the revised manuscript. The new text read as (line 286-289) "….To improve the spatially and temporally consistency of the PET estimates, we adjusted at sub-basin level the solar radiation for each month by comparing against observation based, long-term (1950-2000) seasonal PET (Trabucco and Zomer, 2009), which is similar to the method suggested in Alemayehu et al. (2017)."

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

Response: This is referring to the validation study on MOD15A2 LAI at Budongo Forest (Uganda) and Kakamega Forest (Kenya) by Kraus (2008). This study did not include other biome types.

l.238: What do you mean by "land cover mix"? l.238: Grammar. l.238: Please add the sizes of the homogenous sites.

Response: land cover mix is referring to the mix of different land cover classes (i.e. forest, grassland…) within 1000m grid resolution of MODIS LAI. We have further improved this part and provided more

information in the revised manuscript (line 228-231). This read as "…We selected a representative relatively homogenous sample sites (i.e. polygons) for evergreen forest (174 km^2), tea (123 km^2), savanna grassland (136 km^2) and shrubland (130 km^2) (see Figure 1b) using the Africover classes and Google Earth images. This is useful to reduce the effect of land cover mix while averaging coarse scale (i.e. 1 km) LAI and hence improve the reliability of the LAI timeseries. "…

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

Response: This is based on the long term LAI timeseries during the rainy season (i.e. the peak growing season), whereby the LAI values are above 2.0 m^2/m^2. Therefore, LAI values during the rainy season below 1.5 are replace with interpolated values. We have provided this information in the revised manuscript (line 233).

l.242: Are these gaps resulting from the previous masking?
Response: The gaps are mainly due to cloud contaminations during April mostly.

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

Response: We have further improved this part in the revised manuscript (line 235-237).

l.266: Is this measured NDVI or remote sensing based? If it is remote sensing derived, are the two products independent from each other?

Response: The NDVI is referring the MODIS product. The thermal-based ET from Alemayehu et al. (2017) is independent of the MODIS NDVI data. Alemayehu et al. (2017) estimated ET using mainly MODIS land surface temperature based on the Operational Simplified Surface Energy Balance (SSEBop) (Senay et al. 2013) algorithm.

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?

Response: We have provided this information in the revised manuscript (line 266).

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.

Response: Right, the SMI is used to indicate (i.e. trigger) the start of the new growing cycle within a predefined period. We have further clarified this throughout the revised manuscript.

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.

Response: We have addressed this properly and we provided the response for major comments 2 and 3.

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

Response: We agree with the referee. Even though the LAI magnitudes are relatively high in April and August, they are not considerably high compared to the rest of the rainly months. This is mainly due to ample rainfall distribution throughout the year. We therefore further improved this in the revised manuscript.

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

Response: We have modified this accordingly in the revised manuscript.

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

Response: The tea and the forest are located in the humid and mountainous part of the basin. Therefore, we note comparable seasonal LAI dynamics and hence presented results for forest only. We have updated this in the revised manuscript.

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.

Response: We have updated this accordingly in the revised manuscript.

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.

Response: This suggestion seems a misunderstanding. We used the MODIS LAI timeseries from 2002-2005 for the calibration and the rest (2006-2009) for validation. Only the minimum LAI and the Potential Heat Unit (PHU) are adjusted based on long-term values and the remain SWAT parameters related to vegetation growth are adjusted by comparing the with the 8-day LAI time series. As mentioned earlier, we substantially restructured the results and discussion part in the revised manuscript.

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.

Response: We think the way the manuscript structured created this confusion. As we stated already, the results and discussion has been restructured substantially. We have clarified in the revised manuscript (line 407-419) that the average seasonal pattern of LAI was computed using the calibrated SWAT-T simulation and MODIS LAI from 2002-2009. The average seasonal LAI amplitude is the difference

between the peak and the trough LAI values (i.e. the range) and we normalize this value with the mean annual MODIS LAI.

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.

Response: We agreed with the referee suggestion. We have plotted together the seasonal LAI for from MODIS and SWAT together. We have also included a pooled scatter plot of MODIS and SWAT-T simulated seasonal LAI for FRSE,Tea, RNGE and RNGB in the revised manuscript.

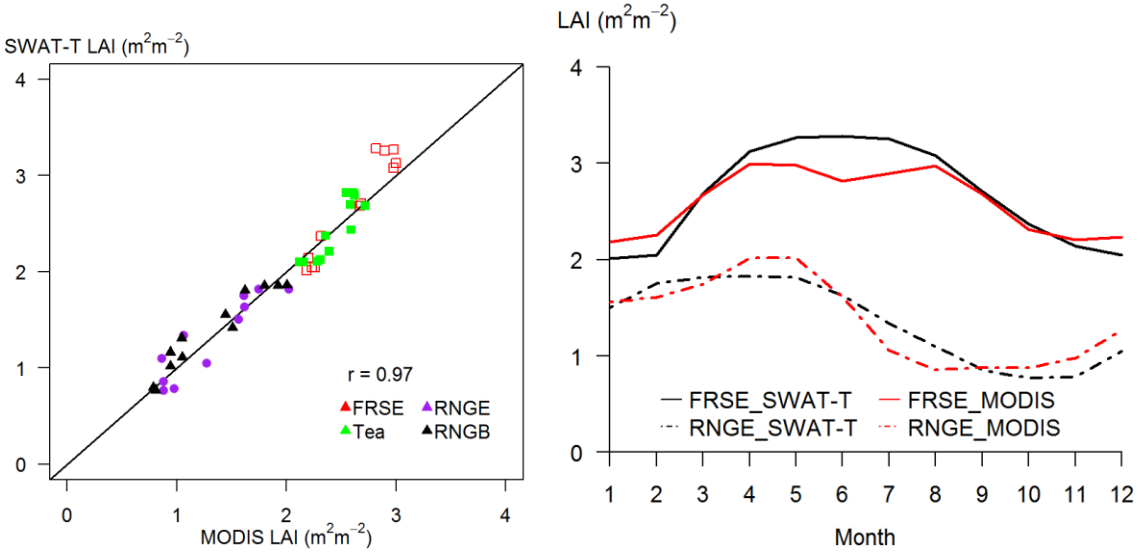

Figure 8 The long-term (2002-2009) average seasonal LAI pooled scatter plot (left) and temporal dynamics (right). FRSE: evergreen forest; RNGE: grassland; RNGB: shrubland.

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

Response: We think discussing the season change dates after LAI seasonality is appropriate.

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?

Response: Addressed in the general comment 7 response.

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.

Response: Yes, correct ALAI_MIN value to the SWAT plant database would not solve this problem. Yet, the zero LAI values, for instance the forest (FRSE), as simulated in SWAT is not due to wrong prescription of ALAI_MIN in the plant database, rather due to the management settings. The ALAI_MIN is used only when dormancy occurs.

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.

Response: We agree on the fact that comparing different SWAT model LAI simulation with varying management setting and vegetation growth module is not appealing. Nevertheless, such uncalibrated comparisons of LAI simulation is important in shedding light on the inconsistencies of SWAT growth module for trees and perennials in the tropics. To clarify further, the uncalibrated LAI simulations are outputs from the same SWAT model with four different settings: 1) standard SWAT vegetation growth module with the default management settings based on heat unit scheduling

2) standard SWAT vegetation growth module with the default management settings based of calendar date scheduling

3) standard SWAT vegetation growth module with no management setting

4) with the modified vegetation growth module with no management setting

The first thee option, of course, do not require a code change. Note that there is no difference in the SWAT model parameters for this comparison, their differences are mainly due to the management setting and the modification in the vegetation growth module. We have further sharpened this in the revised manuscript.

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).

Response: The aim of this paragraph is to provide supportive information on rainfall distribution in the basin, which is the dominant factor in vegetation phenology. The redundancy with Figure 5 has been improved with the new rearrangement of the results and discussion part in the revised manuscript.

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.

Response: The "standard SWAT" model is referring to SWAT 2012 revision 627 as stated in line 272 in the revised manuscript. As discussed in the earlier comments, the uncalibrated model comparison is to minimize the effect of model parameters in highlighting the inherent limitations with the vegetation growth cycle in tropics.

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

Response: We have improved the language in the revised manuscript.

l.354: Did you test for significance?

Response: No significance test but that is to show the considerable reduction in zero potential transpiration. We have changed the wording in the revised manuscript.

l.368: Quantify where possible.

Response: We have improved the result presentations in this part using bias and correlation values in the revised manuscript.

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?

Response: As pointed by the referee, this section is indeed the important part of the manuscript. We have further provided information on the results in the revised manuscript (line 368-3888).

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.

Response: Thank you for the suggestion. We have improved the figures in the revised manuscript (shown below).

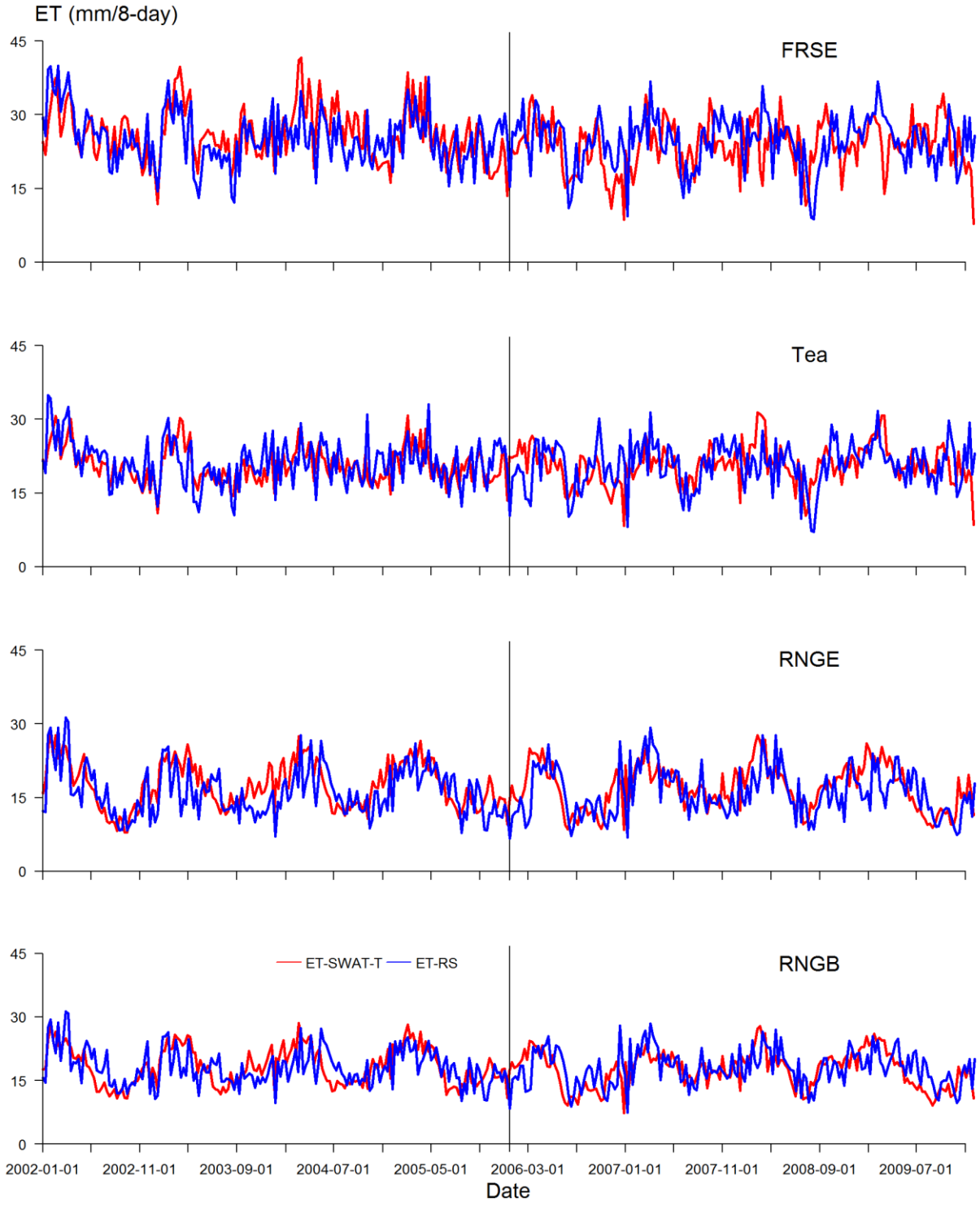

Figure 10 The comparison of remote sensing-based evapotranspiration (ET-RS) and SWAT-T simulated ET (ET-SWAT-T) aggregated per land cover classes. Note that for SWAT-T HRU level ET is aggregated per landcover. The vertical black line marks the end of the calibration period and the beginning of the validation period.

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

Response: Thank you for spotting this. This has been corrected in the revised manuscript.

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

Response: Thank you again for pointing this out. We have clarified this in the revised manuscript (line 443-444).

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.

Response: The purpose of this paragraph is to provide information on the calibrated SWAT parameters and the higher water use (i.e. ET) by FRSE compared to the other land cover classes. We have moved up this to the first part of section 3.2.3. (Page 20, line 431-436) in the revised manuscript.

l.404: Sentence not clear. Please improve.

Response: We have rephrased this in the revised manuscript.

l.405: Please quantify the spatial variability.

Response: Since SWAT assumes a uniform, single plant species community per land cover class, thus we do not expect substantial spatial heterogeneity. Therefore, we have provided a qualitative visual information using spatial maps on LAI and ET as shown in Figure 11 (page 22).

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.

Response: This is a good point. However, since SWAT is not a fully distributed model and the HRUs are not square grids direct comparison will not be effective. Furthermore, getting cloud free MODIS LAI for the whole study area is not feasible. Therefore, the aim of the graphs two show LAI and ET spatial variation (qualitatively) for one rainy month and one dry month.

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

Response: Thank you for the suggestion. We have shortened the conclusion in the revised manuscript.

l.432: This sentence is misleading (“default parameters”). As I understand your setup

SWAT-T parameters were calibrated. Please clarify.

Response: To demonstrate the modification in the plant growth module, the comparison were shown for simulations with default SWAT parameters (i.e. uncalibrated SWAT and SWAT-T results). Then the SWAT-T were calibrated and evaluated using MODIS LAI, RS-ET and observed streamflow. We noted in general that our manuscript need improvement in structures. Therefore, the revised manuscript will address clarity issues.

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

Response: Potential transpiration is referring to the maximum transpiration rate by plants at optimal conditions, i.e. no water, temperature and nutrient stress. On the other hand, the potential evapotranspiration is defined as "the amount of water transpired by alfalfa, completely shading the ground, of uniform 40cm height and never short of water.

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.

Response: We have excluded this from the conclusion of the revised manuscript.

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

Response: Improved.

l.441: "conformed"

Response: We have improved the language.

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

Response: We removed this from the conclusion.

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

Response: Improved.

l.451: "carbon fluxes" Not shown. Please remove.

Response: *Removed.*