

Reply to referee 2

Overview: We would like to thank the reviewer for his/her dedication in reviewing the manuscript. We are also thankful for his/her detailed and constructive suggestions and comments. We have addressed all the comments raised by the reviewer and the manuscript has improved from the proposed changes.

The authors evaluate a method to depict seasonal land use dynamics with SWAT+. Moreover, they evaluate blue and green ET for the study area. The results with regard to the implementation of seasonal land use dynamics evaluate by using satellite ET are promising. However, more details on the model and the implementation need to be provided, before this manuscript can be considered for publication.

General comments:

1) There are two topics in the manuscript that are not very well related. E.g. the State-of-the art focuses on the implementation of seasonal land use dynamics. However, also blue and green ET is also one of the study aims and not well represented in the introduction section. Most of the paper is about seasonal land use dynamics. The manuscript part on blue and green water consumption is not very well connected to this. In parts, it reads like a different paper. Particularly as in the last part of the results section a new method is presented that was not introduced in the methods section. I would suggest that the authors either focus on the topic of seasonal land use change implementation and its impacts (which might include blue and green ET as one -but not the only-example), or they provide more motivation why blue and green ET is important in this context and why these two topics should be dealt with in one manuscript. In this case, please also include blue and green water in the state-of-the-art.

Authors Response: We agree with the reviewer, after re-reading our previous manuscript, we realize we have two topics as stated by the reviewer and less details are provided in the introduction on blue and green ET. We will focus on blue and green ET, the state of art will be included and more details on the introduction will be included in the revised manuscript.

Our innovation is that we use an agro-hydrological model (SWAT+) to represent blue and green ET for different cropping seasons (represented by trajectory with time and space) and we further used validated remote sensing ET to evaluate the simulated ET from SWAT+.

2) The model calibration and validation approach are not clear. Details need to be provided to judge on the validity of the results.

Authors Response: We did not calibrate the SWAT+ model, our aim was to improve the spatial distribution of blue and green water consumption and not discharge simulation. We evaluated the simulated ET by comparing with the remote sensing ET. We will include a clear explanation in the revised manuscript under the methodology section.

3) Model setup for static and dynamic model needs to be explained in detail. Inconsistencies in model outputs, e.g. static does not equal dynamic ET for areas that are static in both model implementations, should be explained.

Authors Response: We agree with the reviewer, the revised manuscript will include more explanation of the model setup under the methodology section.

4) Land use data (e.g. land use classes, trajectories, accuracies) need to be shown in more detail.

Authors Response: We will provide enough details on the land use classes, trajectories and accuracies in the revised manuscript.

5) Innovative aspects of your research should be highlighted and presented against the state-of-the-art.

Authors Response: We agree with the reviewer that the state of art of the manuscript is not clear, we will include more explanation to our specific innovation. For example we will explain in detail our innovation as the use of agro-hydrological model such as SWAT+ to representing blue and green ET for different cropping seasons (represented by trajectory with time and space) and the use of remote sensing ET to evaluate the simulated ET from SWAT+.

6) Proof-reading by a native speaker would be helpful. I suggested some changes, but there are certainly more sentences that need to be improved.

Authors Response: Reviewer's comment is taken; we will have a native English speaker to help read and edit the revised manuscript.

Line specific comments:

l.9-10: Please clarify and unify terms: cropping cycle, cropping seasons

Authors Response: Cropping cycle and cropping seasons were used interchangeably. We will stick with the cropping seasons instead of cropping cycle.

l.11: 'In most agro-hydrological model applications such as SWAT+ in Africa, only one cropping season per year is represented.' This is indeed surprising. Please see also my comment on l. 56 and l. 72-73.

Authors Response:

l.14: Better focus on the topic of this paper in the abstract: 'This study builds upon earlier research that proposed an approach on how to incorporate seasonal land use dynamics in the SWAT+ model but mainly focused on the temporal pattern of LAI and tested the approach in a small catchment (240 km²).'

Authors Response

l.20: suggest to change to: , remote sensing estimates, resulting in a higher performance' remove ,than default'

Authors Response: Thank you the comment is taken we will revise and change in the revised manuscript.

l.22-23: Please improve the language and strengthen conclusion

Authors Response: We agree with the reviewer, the conclusion will be improved.

1.30 suggest ,at the‘ instead of ,per‘

Authors Response: We will change from “per” to “at the”

1.36: I believe these are studies that have implemented land-use dynamics. In this case, ,few‘ is misleading, suggest to say ,A few...‘

Authors Response: Comment is well taken. We will change from “Few” to “A few”

1.44-45: Please clarify, what you mean with ,implemented seasonal land-use dynamic in SWAT and SWAT+ through land-use trajectories, and not land-cover classes.“ As I understand it, a trajectory is also a change of land-use and land-cover classes. So that the meaning of the sentence is not clear to me.

Authors Response: After re- reading the sentence yes, it is misleading. We will revise and provide a better explanation in the revised manuscript. We mean to say that, a study by Nkwasa et al. 2020 and our study, use the land use trajectories as input map that shows changes in land use per time and space, unlike other studies where the input map is a land use map then the seasonal changes in the land use is implemented later by crop rotation.

1.56: AfricaN basins

Authors Response: Africa will be changed to African

1.56 and 72-73:,...typically not represent different cropping seasons‘ and 'Although the SWAT (+) model is capable of representing multiple cropping seasons, this is rarely implemented.'

I agree with you, that it is important to represent different cropping seasons. But please reflect that seasonal crop rotations can be depicted with SWAT and that has been done in the past in study areas with a strong seasonality, e.g. typically in India. Please find 3 example studies below. For these implementations the seasonal changes within one year is however always the same. Would it be possible to go beyond that with your methodology? Do you account for all possible combinations of seasonal crop rotations in space? Please highlight the innovation in your research.

Garg, K.K., Bharati, L., Gaur, A., George, B., Acharya, S., Jella, K. and Narasimhan, B. (2012), Spatial mapping of agricultural water productivity using the swat model in the Upper Bhima catchment, India. *Irrig. and Drain.*, 61: 60-79. <https://doi.org/10.1002/ird.618>

Narsimlu, B., Gosain, A.K. & Chahar, B.R. Assessment of Future Climate Change Impacts on Water Resources of Upper Sind River Basin, India Using SWAT Model. *Water Resour Manage* 27, 3647–3662 (2013). <https://doi.org/10.1007/s11269-013-0371-7>

Wagner, P. D., Kumar, S., and Schneider, K.: An assessment of land use change impacts on the water resources of the Mula and Mutha Rivers catchment upstream of Pune, India, *Hydrol. Earth Syst. Sci.*, 17, 2233–2246, <https://doi.org/10.5194/hess-17-2233-2013>, 2013.

Authors Response: Thank you for your comment. yes we can go beyond these studies with our innovation since we are representing the trajectories which include changes in time and space. We see an example of study in Pangani by Ndomba et al. 2008, the cropping seasons were not implemented because land use classification in the tropics is challenging as indicated by unclearly defined land use practices in this study area (Ndomba et al., 2008).

The paper by Merriman et al. (2019) is another example where the crop rotation has been implemented in details and where our innovation could be adopted. The study has shown crop rotation in detail e.g. “one crop cover, changing from corn silage to cereal rye” however, this method might not apply in most of the tropical African catchments like our catchment, because one crop cover may have different crop rotation practices. Hence, we need to know the location (having a specific rotation). For example, we have indicated that the same rainfed maize crop cover may change in one field and form a trajectory CORN→AGRL→AGRL or change from CORN→AGRL→BSVG. Thus, it is very crucial to use the changes in seasonal land use to represent these changes in space and time.

l.74: ‘By default, SWAT simulates a single growing cycle every year.’ This is true, but it can be argued that the modeler should adjust the default, if the default is not applicable.

Authors Response: We agree with the reviewer's argument. We will revise the sentence and add more literature on how most of the modelers have and have not adjusted the default model to highlight our innovation.

1.80-83: Please outline stronger what the new contribution of this paper is. If it building on earlier findings is fine, but this could also be outlined in the methods section.

Authors Response: Thank you for your comment. This is well noted. Our innovation is that we use an agro-hydrological model (SWAT+) to represent blue and green ET for different cropping seasons (represented by trajectory with time and space). Additionally, we used validated remote sensing ET to evaluate the simulated ET from SWAT.

We will restructure the introduction and focus more on the blue and green ET. We will include the detail explanation of our innovation in the revised manuscript.

1.80-92: Suggest to shorten the paragraph to the aims. Please move the methodological details to the methods section.

Authors Response: The comment is well taken. We will move the methodological details of the last paragraph in the introduction to the methods section.

1.95 As there has been SWAT research on the Pangani basin, I would suggest to relate your research (literature review + findings) to it. See e.g.:

Notter, B., Hurni, H., Wiesmann, U., and Abbaspour, K. C.: Modelling water provision as an ecosystem service in a large East African river basin, *Hydrol. Earth Syst. Sci.*, 16, 69–86, <https://doi.org/10.5194/hess-16-69-2012>, 2012.

Authors Response: The comment is well taken. We acknowledge related works using SWAT in the Pangani basin. Hence, we will relate their research findings to our study in the revised manuscript.

Fig. 1: Inset map is not readable. Please revise.

Authors Response: The revised version of the manuscript will include a revised figure 1.

Fig. 2: It would be preferable to show a 30 year average of rainfall to depict the climate, if data is available. The authors state that there was at least data available for 2006-2013, 1.122. Certainly, a longer period would be better. This figure is also depicted in Msigwa et al. 2019. Please, make sure that there are no copyright issues. You may include temperature to provide a bit more information here.

Authors Response: We agree with the reviewer. The average of 30 years will be analysed and included in the revised manuscript. Also, we will include temperature averages to provide more information.

1.115: Please add which DEM was used, not only the source for download. SRTM?

Authors Response: The source of DEM will be added to the revised manuscript. DEM is SRTM from NASA.

1.123-126: As the entire paper relies on the accuracy of these land use maps, you need to provide classification accuracies here. I would suggest to show at least overall accuracy and the range of user accuracies for the different land use classes. Please also state which and how many classes have been identified and which classification algorithm was applied.

Authors Response: The information on the classes accuracies will be included in the revised manuscript.

1.127: ‘For instance,...’ One example is not sufficient. Either provide the setup information for all land use classes or refer the reader to a publication where you have shown that.

Authors Response: The comment is well noted. We will refer the reader to the additional setup information in the appendix within the revised manuscript.

1.136: Full stop missing

Authors Response. Thank you, we will include the fool stop.

l.145-147: Sentence and reasoning not clear to me. Bananas and coffee should probably not change within a year. Did they in the trajectory analysis? If so, how would you explain that? Also, how would you parameterize a combined class of coffee and bananas? Please clarify.

Authors Response: We didn't implement the trajectory analysis for banana and coffee. However, in the catchment we have farms that change from banana and coffee to banana, coffee and maize because they plant maize during the rainy season. This analysis was explained in the previous paper by Msigwa et al. 2019.

Figure 3: While this map provides a good first overview, regarding the topic of the paper, I think it is necessary to show the different land use trajectories in more detail.

Authors Response: The comment is well taken, we will revise the map and provide different land use trajectories in more detail.

l.164: Otherwise spelled as 'sub-basins', please unify.

Authors Response: Sub Basin will be changed to sub-basin in the whole revised manuscript.

l.170: Are you using the option to grow two or more crops at the same time? If yes, this should be highlighted, if not, why mention this?

Authors Response: We did not grow one crop at the same time. We will remove the highlight as suggested by the reviewer.

l.175-176: suggest to revise to 'limited amount of input data'

Authors Response: Reviewer comment is taken. We will rephrase in the statement in the revised manuscript.

l.177: 'rather than using remote sensing climate data' Sentence not clear, please clarify.

Authors Response: Reviewer comment is taken we will rephrase the statement in the revised manuscript.

l.181: Table 1B+2B do not show 40 trajectories, please clarify. Also, some of the trajectories seem to be no real rotations, e.g. “indn CORN-BSVG-BSVG“, seems to be a single crop corn in one cropping season and no cropping in the other seasons. I think it should be highlighted which of these trajectories describe real crop rotations and which are only single crops, which could probably be well represented by a model without a seasonal representation of crops.

Authors Response: The comment is well taken. We will revise and include all the 40 trajectories and specify the trajectories with only the cropping seasons.

Table 1: I would suggest to write 2-3 sentences to explain the shown management file highlighting the capabilities, e.g. tomato and soy bean are grown on the same field. Suggest to delete white space. Moreover, if you have tomato and soy bean on one field, how was that derived in the land use classification? And if this was a class for itself, how good was the classification performance?

Authors Response: The reviewer comment is well taken. We will include a detailed explanation of the management file with the implementations. The tomato and soy bean were grown on one field at different times, the land use class was term as “irrigated mixed crop”.

l.217: I cannot find the source , IHE Delft, 2020‘ in the reference section.

Authors Response: The references will be reviewed and IHE Delft 2020 will be included in a revised manuscript.

l.239: ,statistical matrices‘?

Authors Response: Thank you for the comment, we will revise and change to model evaluation statistics as suggested by Moriasi et al. (2007).

l.213-239 The Model Evaluation section needs a thorough revision, please address the following points:

Authors Response: We will revise the section and add all the details suggested by the reviewer in the revised manuscript.

1) Setup of the two models: Which land use map was used for the static model?

Authors Response: The March land use was used for static model. This will be clearly stated in the revised manuscript.

2) Calibration approach? Did you calibrate your models? How did you do that and did you do this separately for the static and dynamic model?

Authors Response: We did not calibrate any model static or dynamic model. Our aim was to improve the spatial distribution of blue and green water consumption and not discharge simulation. We compared the static and dynamic model in default conditions. This approach allowed us to compare model results in default parameter conditions, considering parameter calibrations vary with different catchments. Nkwasa et al., (2020) also suggested that improved representation of crop and agricultural land use processes should precede any model calibration efforts. Thus, we evaluated the simulated ET by comparing with the remote sensing ET. This will be clearly stated in the revised manuscript.

3) It seems as if the model performance is solely evaluated with ET. This needs a better justification and explanation. What about the discharge data described in the methods section? Please provide more information on the ET data used for calibration (?) and validation. What exactly was compared? Basin values, sub-basin values, grid values? If that has been carried out in a previous study, you may also refer to that study for details, but you need to provide the reader with the main information that is necessary to evaluate the performance of your model.

Authors Response: The reviewer comment is well taken. The model performance was evaluated by the ET only. Hence, it was unnecessary to include the discharge data that was not used in the description. This will be omitted. Also, the description of how we compared and evaluated the remote sensing ET with the model simulated ET will be added in the revised manuscript.

See also the following HESS paper on SWAT modeling with ET data in Africa:

Odusanya, A. E., Mehdi, B., Schürz, C., Oke, A. O., Awokola, O. S., Awomeso, J. A., Adejuwon, J. O., and Schulz, K.: Multi-site calibration and validation of SWAT with satellite-based evapotranspiration in a data-sparse catchment in southwestern Nigeria, *Hydrol. Earth Syst. Sci.*, 23, 1113–1144, <https://doi.org/10.5194/hess-23-1113-2019>, 2019.

- 4) Actually the indices that were applied are well known. I would suggest to rather focus on explaining the calibration and validation strategy and do not explain the indices in such detail.

Authors Response: The comment is well taken. An explanation on the indices will be omitted in the revised manuscript and more information on how we evaluated the ET will be added.

- 5) For which period was the model run?

Authors Response: The model run from a period of 2008 to 2013. The year 2006 and 2007 was a warming period. Details will be added in the revised manuscript.

1.253: Verb missing

Authors Response: The sentence will be revised and a verb will be added in the revised manuscript.

1.260-262: Please explain and clarify, sentence not clear to me.

Authors Response: The sentence will be paraphrased and made clear in the revised manuscript.

1.266-268: Please revise sentence and check grammar.

Authors Response: The comment is well taken and we will revise the sentence.

Fig. 4: How come that the static ET peaks are some times higher than the dynamic ones? I would have assumed that dynamic ET = static ET for the period in which both have the same crop and that for all other seasons dynamic ET > static ET. As detailed and required information on how the static land use was implemented (and differs from the dynamic land use) is missing (see previous comment), it is hard to understand these differences.

Authors Response: We agree with the reviewer comment, detailed explanation of the model setup is needed to know the differences. The model setup for static used a March land use map with only 14 land use classes, while the dynamic model used a land use map with 40 trajectories. Hence, the changes in the ET might be due to the different land use maps yielding different number of HRUs. A clear explanation will be added in the revised manuscript.

1.279: Suggest ‘A notable difference...’

Authors Response: Comment is taken. We will change “The” to “A”

1.281: Please define what you refer to as ‘mass balance in percentage’

Authors Response: Mass balance was meant to say change in soil water balance in the model. In the revised manuscript we will modify to “change in soil water balance”.

1.286-292 and Fig.5: How do you explain the strong differences for the areas that show a high satellite ET? Even the dynamic model underestimates these considerably.

Authors Response: The ET from dynamic could not reach maximum satellite ET because the satellite ET estimates also have uncertainties in the mountainous areas because of the presence of cloud cover. There are no observation data in these areas that we can validate with.

1.293-294: It is hard to follow the line of argumentation here. Looking at Figure 5 I see most pronounced changes between static and dynamic implementation at the Northern border of the catchment. But when I look at Fig. 2, these are not areas with trajectories. Please explain these differences. I would expect that all areas with no trajectories show the same ET value in both models.

Authors Response: Thank you for the comment. We agree with the reviewer’s argument. We acknowledge that we have not explained how we corrected some unrealistic trajectories in the land use maps when implementing trajectories in the dynamic model. When implementing the trajectories, some unrealistic trajectories were noticeable for example; a trajectory of irrigated banana and coffee land use to forest land use to forest land use in the March, August and October maps respectively is unrealistic. Therefore, we change that trajectory to be forest land use in the

dynamic model which could be the reason why the static and dynamic ET could be different even for the static land use map in some regions. This information will be clearly elaborated in the revised manuscript.

1.295-297: Please clarify the following sentence: “Likewise, the changes seen in the high land areas of irrigated banana and coffee and the forested areas might be due to the increase in the number of HRUs in the dynamic SWAT+ model that contributed to the more accurate results.” Why do HRU numbers change? Again the implementation differences between static and dynamic scenario are not clear. From a methodological point of view, I would not expect changes in the number of HRUs. For your study aims you need to make sure that you minimize any other impact (e.g. differences in model structures) to really deduce the impact of your seasonal land use change implementation.

Authors Response: The reviewers’ comment is well taken. We will add more details on the implementation of static and then dynamic models. The number of HRUs will be different because the input land use maps have different number of land uses classes. In the dynamic model the land use map (trajectory map) had 40 land use classes while the static land use map had 14 land use classes. However, we maintain the same number of sub-basins to try and minimize the differences in model structures.

1.308-309: Please improve language ,for annual (Figure 6) and from 2008 to 2013.’

Authors Response: The caption language will be improved for figure 6.

1.320: As mentioned earlier: Please include a land use (trajectory) map, I cannot see where sugarcane is located. The reader must be able to follow and verify your conclusions.

Authors Response: The point is well taken. We will revise the land use map and include the trajectories.

1.324-335: These methods have not been explained. If you want to show these here, you need to include them in the methods section. It also looks as if some data from a forthcoming publication

is shown. Please specify if you refer to the data or to the methods with the reference. See also my general comment on the two topics covered in this manuscript.

Authors Response: The comment is well taken. We will include an explanation of the methods for estimating blue and green ET that we have compared with the SWAT. We will also specify that we have used the data from a previous paper (Msigwa et al., 2021) on the revised manuscript.

Msigwa, A., Komakech, H. C., Salvadore, E., Seyoum, S., Mul, M. L., & van Griensven, A. (2021). Comparison of blue and green water fluxes for different land use classes in a semi-arid cultivated catchment using remote sensing. *Journal of Hydrology: Regional Studies*, 36, 100860.

1.335: Forthcoming Msigwa et al. 2020 paper is not available in the reference section

Authors Response: Thank you for the comment we will include the reference in the reference section of the revised manuscript.

1.340: Please be more careful with this statement ,none of these studies represented seasonal dynamics'. As outlined above, there are a number of studies that have incorporated seasonal crop rotations in India and possibly also elsewhere. They might not have compared the effect to a static model, but they still implemented them. Please highlight what the advantage of your approach is. One example might be the spatial representation of trajectories.

Authors Response: The comment is well taken we will revise the statement and add that our approach goes further to represent the spatial location of these seasonal changes.

1.342: Typo: You did show that, didn't you?

Authors Response: We meant the study by Nkwasa et al. 2020 didn't show how the seasonal land-use dynamic improved water balance component such as ET but not our current study. We will revise the sentence to make it clear in the revised manuscript.

1.349-352: Please also discuss and explain, why static and dynamic ET do not match for static land use areas and why your ET estimate never reaches the maximum satellite ET.

Authors Response: The comment is well taken. We will include the explanation in detail in the revised manuscript.

ET from static and dynamic model could not much in most areas due to the correction of the land use trajectories as explained earlier. However, this is a very important observation we will run the models again to have to confirm our argument.

The ET from dynamic could not reach maximum satellite ET because the satellite ET estimates also have uncertainties in the mountainous areas because of the presence of cloud cover. There are no observation data in these areas that we can validate with (Msigwa et al., 2021).

1.355: Forthcoming Msigwa et al. 2021 paper is not available in the reference section

Authors Response: Thank you for the comment. We will include the reference in the reference section of the revised manuscript.

1.365: What about the uncertainties of the land use maps and the associated trajectories as well as their impact on hydrology? Mostly it is hard to assess land use with multi-spectral satellite data in all seasons due to cloud cover (in the rainy season). How did you deal with this? And what does this mean for the transferability of your methodology?

Authors Response: We agree with the reviewer's comment. There were uncertainty associated with the trajectories for example unrealistic trajectories like change from crop to forest then crop again. These types of trajectories were corrected and reclassified. We will include these details in the discussion section.

We only used the images with less than 10% of the cloud cover. We also mask the clouds by replacing with the proper land use either using our field survey information or previous land use map by Kiptala et al. (2013). This approach had some effects since we were not able to capture the peak wet seasons where the crops have fully grown. We will include this information in the discussion section of the revised manuscript.

1.338: Please include a discussion of model performance in the discussion section.

Authors Response: The reviewer's comment is well taken. The revised manuscript will include model performance in the discussion section.

l.385: ,blue water amount is in line with previous studies' Not sure to which section the authors refer here and to which studies. Please clarify.

Authors Response: The reviewer's comment is well taken. It is true that the sentence needs a reference as to which study, we mean. We will provide clarification in the revised manuscript.