

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
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Comment on hess-2021-171

Anonymous Referee #2

Referee comment on "Representation of seasonal land use dynamics in SWAT+ for improved assessment of blue and green water consumption" by Anna Msigwa et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-171-RC2>, 2021

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.

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

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.

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

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

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.

Line specific comments:

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

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.

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).'

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

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

l.30 suggest ,at the` instead of ,per`

l.36: I believe these are studies that have implemented land-use dynamics. In this case, ,few` is misleading, suggest to say ,A few...'

I.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.

I.56: AfricaN basins

I.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.

I.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.

I.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.

l.80-92: Suggest to shorten the paragraph to the aims. Please move the methodological details to the methods section.

l.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.

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

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, l.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.

l.115: Please add which DEM was used, not only the source for download. SRTM?

l.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.

l.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.

l.136: Full stop missing

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.

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.

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

I.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?

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

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

I.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.

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?

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

I.239: ,statistical matrices'?

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

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

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.

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.

5) For which period was the model run?

l.253: Verb missing

l.260-262: Please explain and clarify, sentence not clear to me.

l.266-268: Please revise sentence and check grammar.

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.

l.279: Suggest 'A notable difference...'

I.281: Please define what you refer to as 'mass balance in percentage'

I.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.

I.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.

I.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.

I.308-309: Please improve language ,for annual (Figure 6) and from 2008 to 2013.'

I.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.

I.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.

I.335: Forthcoming Msigwa et al. 2020 paper is not available in the reference section

I.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.

I.342: Typo: You did show that, didn't you?

I.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.

I.355: Forthcoming Msigwa et al. 2021 paper is not available in the reference section

I.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?

I.338: Please include a discussion of model performance in the discussion section.

I.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.