

Interactive comment on “SWAT-CUP for Calibration of Spatially Distributed Hydrological Processes and Ecosystem Services in a Vietnamese River Basin Using Remote Sensing” by Lan T. Ha et al.

Anonymous Referee #2

We thank the reviewer for these valuable comments. In the following discussion, we will provide responses to the comments point-to-point and adopt changes as suggested to the manuscript.

General Comments

The manuscript undertakes a calibration of SWAT with the SUFI-2 procedure using remote sensing data from precipitation, ET and LAI at the HRU level for the Day Basin in Vietnam. The paper anticipates that this method is better for the quantification of eco- hydrological processes in un-gauged basins. Generally, while the authors use a novel and interesting method, they confuse the terminology between SWAT CUP and SUFI-2. As well, an ensemble of ET products was applied, and often unclear or vague process descriptions are provided (i.e. for SUFI calibration and in the results). Therefore, for these and the reasons listed below, my recommendation for this manuscript is “major revisions”.

[Response] We thank you for this observation. SWAT-CUP and its more specific SUFI-2 algorithm was used as a mean to calibrate SWAT with a fully spatially distributed data set on rainfall, ET and LAI. This is a new aspect because conventionally only flow measurements on a few locations are used in conjunction with SWAT. In the new approach, the bio-physical parameters at HRU level can be calibrated using remotely sensed data for geographically specific areas. Indeed, SWAT-CUP has many more functionalities that allows the optimization of SWAT parameter sets and the role of SUFI-2 will be highlighted in the next version of the manuscript.

The paper is written in good English. However, often the term SWAT-CUP is used, when really SUFI-2 is being referred to. The authors need to specify which programs are in SWAT-CUP and clarify which was used in this study (i.e. SUFI-2). Please make sure to make the distinction between SWAT-CUP and SUFI-2 throughout the paper. Also, the SUFI-2 calibration procedure is not described, i.e.

how does it function, what are the main selection processes for parameter sets, etc.?

[Response] Thank you for this comment. As mentioned, a more distinguishable borderline between SWAT-CUP and SUFI-2 has been included in the revised manuscript to specify more explicitly describe the calibration and optimization procedures. Since SUFI-2 has been described in various papers before and the innovation of the current manuscript is the application of remote sensing data to calibrate the eco-hydrological processes in poorly gauged basins, we will not discuss the mathematical optimizations at length. More references are being provided.

The authors should clarify how the sensitivity of parameters was carried out for the basin: information on page 7 is different than on page 16. I believe the authors only relied on literature studies of sensitive parameters. If this is the case, I strongly suggest to conduct an analysis for the sensitivity of the ET and LAI and other parameters in their basin to determine the most sensitive parameters. Also, the Nash-Sutcliff was chosen as the main objective function, I would suggest to also add the KGE to put less emphasis on the peaks and include other processes leading to lower values.

[Response] We thank you for this comment. Sensitivity analysis is indeed an important process to examine the basin response against certain parameterization. However, while reviewing the literature, we saw that many authors have done studies on this subject, as thoroughly described in Arnold et al., 2012, Gitau and Chaubey, 2010, Immerzeel and Droogers, 2012, Githui et al., 2012, Strauch and Volk, 2013. The core of this paper is not on the optimization process, but more on the unique combination of estimations of actual ET and SUFI-2, which we believe is the first paper ever since. Because the remote sensing results and final simulation of SWAT is more essential, we still believe it is appropriate that a concise paper can refer to reviewed work of peers as possible. Anonymous Referee #1 remarked that the paper is long already and need to be shortened. To preserve the original intentions of this paper, we refer to other peers when it comes to something simple like a sensitivity analysis.

Regarding the selected objective function, we agree that using Kling Gupta Efficiency (KGE) will yield different results (perhaps a more tendency towards lower values). The main reason we used NSE was its popularity in hydrology and inarguably has become a standard. The result and discussions are revised now to include also KGE as suggested.

Discussion paper

The authors have combined the ET satellite products to compare the calibration to, but they should have undertaken a comparison to each individual product. Even though they justify the use of the ensemble mean by comparing it to observed data, the SSEBop performed just as well. Furthermore, the mean of the ET products is meaningless because they are based on different physical processes and equations and the differences are subdued due to averaging. Using the suite of ET products individually provides a more interesting range of uncertainty, showing the peaks and troughs, much like using an ensemble of climate change simulations and is more helpful to the reader.

[Response] The determination of ET from remote sensing is a topic widely described in the international literature. Also there are many scientific papers dealing with the comparison of different ET models, and we agree with Reviewer 2 that different and sometimes even diverging ET estimates can occur from these different energy balance algorithms. Independent ET estimates are necessary for determining the “most suitable” ET value. Such type of data collection from lysimeters or eddy covariance flux towers were unfortunately not available in Vietnam. Given that actual ET (ET_{act}) estimation using satellite has progressed significantly in the past years, we believe that the technique in general is mature enough for calibration of hydrological models. A single preferred model following the principles of “one size fits all” does not exist. A combined ET prediction based on various individual models has given much better results in the equatorial Nile basin (Hofste, 2013), that has a similar climatology as the Day Basin. An ensemble approach was taken toward generating a “best-guess” of ET_{act} in the basin, under the assumption that spatial errors between related yet differing mapping approaches will tend to cancel in the ensemble average. The Ensemble ET created in this study performed well in term of consistency with the water budget and also with crop coefficient. We have more faith in a combined product than in SSEBop as a single model. As a matter of fact, we believe that an ensemble approach will become a standard for hydrological analysis such as being done in global water accounting procedures.

The authors do not specify which HRUs had data from the satellite data for the calibration to be undertaken? In which subbasins were these located? Surely not all 7909 HRUs were calibrated individually!

[Response] We thank you for this observation. We will improve the manuscript to better explain this process.

Specific comments:

In the abstract, provide the size of the watershed. P1, L29. Remove the word nonsignificance.

[Response] Noted and changed.

P2, L3: what is mean by micro-climate cooling?

[Response] Micro-climate cooling arises if the available energy (shortwave and longwave radiation) is converted into latent heat flux, and that by doing so the sensible heat flux reduces and cools the near-surface component of the atmospheric boundary layer. This principle is also referred to as “evaporative cooling”. Since the impact will be mainly on the air mass just above the irrigated fields and forests, it is referred to as ‘micro-climate cooling’. This terminology is used commonly for ecosystem services and not invented by the authors.

P2, L24: why is SWAT recommended for un-gauged basins?

[Response] More hydrological models could be used for assessing the processes in ungauged basins, but these models do not have the unique capacities of SWAT to determine eco-hydrological processes such as crop yield, denitrification, soil erosion, groundwater recharge etc.

P2,L33: what is meant by “It is designed and applied to field measurements only”

[Response] Noted and changed to” It is designed and tested so far using field measurements” to highlight that remote sensing data is a very new concept for inclusion in SWAT modelling. Almost all calibrations of SWAT – also for assessing eco-system services – are based on field measurements that have a limited spatial representation.

P3.L1: do the authors mean SWAT instead of SWAT-CUP? Because SWAT-CUP is in fact the model SWAT.

[Response] We indeed mean SWAT-CUP, because the paper refers to the automatic calibration process that is realized within SWAT-CUP.

P3.L25: It would be interesting to add a paragraph on the uncertainties of the ET products in the introduction.

[Response] Thanks for this observation. We will provide a section describing the uncertainty of generally referred to in the international literature.

P5.L13: Are there any inter-basin withdrawals annually?

[Response] Yes, surface water from the Red River is diverted into the Day Basin, and this volume of water was not known until we came with the results of the current study. We have been able to make a realistic assessment because we had independent estimates of ET available that otherwise needs to be computed from information on the diversions (which was not possible). This reveals the power of inclusion of ET data from remote sensing techniques; high ET rates cannot be explained by rainfall, hence irrigation water supplies can be inferred.

P5,L14: The “core engine of SWAT” is what exactly?

[Response] This wording is incorrect indeed. Now it's changed to “simulation algorithm”

P6. L2: how does SWAT simulate the sequestration of carbon?

[Response] The amount of carbon sequestered is computed from the biomass production that SWAT computes on the basis of a Light Use Efficiency model. The portioning of the biomass into wood, roots and soil stocks has been assessed for every land use class as a post-SWAT process. The factors are taken from international literature.

P6, L16: what is meant by ecological change? Provide examples.

[Response] An ecological change describes the total change of ecosystem services as a result of an internal or external intervention. Further explanation was included in the manuscript.

P6, L23: SWAT-CUP does not have any intelligence. Only living beings have this.

[Response] Noted and changed.

P6, L23-25: confusion with SWAT-CUP and SUFI-2, they are used interchangeably.

[Response] We meant SWAT-CUP here. But it's good also to mention SUFI-2 so an extra text will be added here.

P8, L10-13: how were the performances of the satellite products determined?

[Response] The rainfall has been compared to rain gauges. The actual evapotranspiration has been compared to crop coefficients.

P10,L5-7: explain what a hot and cold reference value is.

[Response] This aspect relates to the scaling of the evaporative fraction of the surface energy balance into specific pixel with zero latent heat flux and other pixels with zero sensible heat flux.

P10, L12-14: Taking an average of the ET products is not recommended! Rather, each individual ET product should be compared to the simulated outcome so that a range of uncertainty can be gleaned.

[Response] We have explained earlier that the necessary independent data sets to do so are neither available in the Day basin, nor in Vietnamese irrigated agricultural land use systems

P10, L15-17: the downscaling of the products requires clarification and more detailed information, especially for CMRSET. This is an important step that is not well described.

[Response] Thank you. We will provide further explanation on the downscaling process using various types of vegetation indices

A year reference to Guerschman is also missing.

[Response] Noted and added.

P10, L20: which 10 consecutive years were used?

[Response] Noted and added.

P10, L24: it is unclear which "analysis" is being referred to? The one from Simons et al., 2016 or this study? This section should also go in the results (if it belongs to this study) about comparing the ET products.

[Response] We thank you for this observation. While it looks like the result from an analysis, it is directly tied to Section 4 on Spatial input data sets. We thought it would be

more consistent to leave it here instead of moving it to Results and Discussion, where more observation and outcome from the modelling part was discussed.

P10, L23, I can only count 4 ET products mentioned (MOD16, SEBS, CMRSET, SSE-Bop), not five.

[Response] Noted and changed.

P10, L25: I believe the “ensemble mean” is meant, not the ensemble.

[Response] Noted and changed.

P11, L13: How was the ET ensemble tested further before being implemented?

[Response] Thank you for this comment. It was the next section on crop coefficient. We will add some text here to better link these two parts.

P11, L14: SUFI-2 is meant, not SWAT CUP.

[Response] We meant both, so SUFI-2 is also added here.

P12, L8-9: Please list the typical crops found in the watershed, if there are any other than rice.

[Response] We thank you for this observation. Other crop type will be added in this paragraph.

P15,L13-14; Describe the LAI process in SWAT. What is meant by “the empirical LAI parameters prescribed in SWAT use an internal database”?

[Response] Noted and further description will be provided in the manuscript.

L14: Again SWAT CUP is not meant here. The “fine tuning” needs to be explained (brief mathematical description) how in the methodology under calibration section.

[Response] Noted and revised in the manuscript.

P16,L5: remove this sentence

[Response] Noted and as also suggested by Anonymous Referee #1, we will rewrite this sentence.

P17,L6: SUFI-2, not SWAT CUP. Explain why Abbaspour (2015) recommended 1500 simulations.

[Response] Adjusted to “SWAT-CUP and SUFI-2”. Further explanation will be provided on number of simulations.

P20,L11-13: this is a very poor explanation for what is being observed.

[Response] Noted and further explanation will be added here.

The conclusion section needs to be reworked to reflect the results and “take home message” of the study.

Additional reference:

Hofste, R.W. Comparative Analysis among Near-Operational Evapotranspiration Products for the Nile Basin Based on Earth Observations; Delft University of Technology: Delft, The Netherlands, 2014.