Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-44-SC1, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Hydrological Modeling in an Ungauged Basin of Central Vietnam Using SWAT Model" *by* A. Rafiei Emam et al.

s. ferrant

sylvain.ferrant@cesbio.cnes.fr

Received and published: 15 March 2016

Dear Editor and authors,

I have prepared this comment to start the interactive discussion in order to improve the lack of references cited in introduction regarding the modelling strategy that has been designed for studying the water resource management in this study.

I will focus only on this methodological aspect, hoping that it will help authors to improve the introduction and method sections and initiate a discussion to assess the points raised by referee #1.

The agro-hydrological SWAT model has been used everywhere in the world for various applications and is known to be robust in simulating discharge at watershed scales

(many references could be found here: https://www.card.iastate.edu/swat_articles/). The main specificity of the SWAT model, compare to other spatially distributed climate-hydro (ISBA-TRIP (1), VIC (2), SURFEX (3)) or hydrological model(TRIP, Topmodel, MODCOU) is that it is an agro-hydrological model, i.e. It is capable to simulate crop rotation and agricultural practices (irrigation and fertilization) in specific agricultural HRU, based on spatial input data such as such as soil and landcover map. The section 2.2 pages 3 gives some detail of soil data, but no details about the agricultural statistics, rice area extent and irrigation practices. A landcover map is missing and crop area extent and their irrigation practices should be mentioned in the text to make clear if it will impact discharge or not. Please also provide any information about the irrigation practices: inundation of the rice from surface or groundwater.

Authors have cited Perrin et al., 2012 as a reference of the use of SWAT in semi-arid area. But the main contribution of this study to the literature is that the SWAT model was calibrated on aquifer recharge rather than discharge, and that the spatial heterogeneity of agricultural practices are essential in the assessment of groundwater availability for irrigation. Various groundwater scarcity conditions are experienced under a same climatic forcing depending of the spatial irrigation pressure and groundwater resources. In that highly impacted and semi-arid context , climate change impact on groundwater storage and shortages are projected to be opposite within the area, depending on the spatial pressure of irrigation of few (1 to 6) percent of the total area (4).

Authors have used AET-PET derived from remote sensing (MODIS) to calibrate SWAT. Material and methods should be improved, as stated by referee 1 in the point 4, by detailing the calibration procedure. Authors have cited Immerzeel and droogers, 2008, who have used Modis data to spatially estimate the groundwater extraction for irrigation using SWAT in Indian basin, but more recent studies have improved this methodology and are detailed in Cheema et al, 2014 (5). Authors should compare or adapt or discuss their own methodology to this more recent and up to date study.

Furthermore, nowadays research efforts in agro-hydrological modelling are focusing on integrating biophysical variables derived from high resolution remote sensing (Leaf Area Index, water surfaces dynamics) to improve hydrological and agro-hydrological modeling. For instance, SOL-AWC parameter, which defines the soil water holding capacity in SWAT model, is evaluated by the authors in term of sensitivity. Authors will find in a recent study an example of the use of this soil parameter together with the re-setting of agricultural practices to optimize crop productivity on LAI derived from remote sensing in an agro-hydrological model similar to SWAT (6,7). A recent study in Surveys in Geophysics discusses the benefit of remote sensing to assess the water budget in river basin affected by human activities (8). The content of these studies should be used to identify the limits of the present study and to discuss its results.

I have found much methodological confusion throughout the manuscript: Modis PET is used (line3 page 9) instead of Actual EvapoTranspiration for instance. MODIS16 product is the “land surface evapotranspiration product which represents all transpiration by vegetation and evaporation from canopy and soil surfaces, expressed in 1-dimensional vertical mm/day units.” (http://modis.gsfc.nasa.gov/data/dataprod/dataproducts.php?MOD_NUMBER=16).

Results of SWAT parameters sensitivity are obvious, well known and should not been presented as results: ESCO is the parameter that allows the modeler to tune the actual evaporation, which impacts obviously the actual evapotranpiration fluxes. BLAI is used to calibrate AET for forested area but also for rice? As stated by the Referee 1, the calibration procedure using these sensitive parameters is not well described. I advise to remove the sensitivity analysis results and replace by a strong justification of the choice of each input parameters that are used to optimize simulations variables rather than selecting the most sensitive.

I really don’t agree with the statement made in line 8 page 8. Rice yields and evapotranspiration are not directly correlated but evapotranspiration highly depends on the irrigation practices that are not described in this study. The calibration procedure of the rice yield is made on plant parameters rather than physical parameters such as soil

water holding capacity or irrigation practices. We do not have any details of final range of these plant parameters that are optimized for the rice. And finally, the figure 5 shows crop yield in T/ha between 25 to 30. The rice yields in Vietnam are around 6.5 T/ha (source FAO). The authors should better explain what they did.

Main limits of this study are that ETP is corrected bias (not clear why), AET is not calibrated or discussed neither on forest nor on rice, agricultural context (area extent, pressure on water) is not described and the calibration of rice yield is made on non appropriate parameters rather than irrigation practices of AWC only. The ratio model is not well described, and the comparison between both model ishould be better justified.

Comments on figures:

Figure 1 should be revised with the land cover map.

Figure 2 does not provide any useful information and any of the many sensitivity analysis that have been published previously should be cited to explain what are the main sensitive parameters (http://www.currentscience.ac.in/Volumes/104/09/1187.pdf for instance).

Figure 3 should be improved by giving the monthly discharge and cumulative daily discharge for each period to assess the ability of the model to simulate discharge volume rather than daily dynamics.

Figure 4 is not appropriate as each simulated value (daily discharge) depends on the previous discharge. Again, monthly discharge should be compared between models.

Figure 5, there is probably a problem with the definition of yield (Aerial Biomass, which is not a yield)

Figure 6 is a summary of the methodological problem of this study. Why comparing Potential evapotranspiration that is forced (or computed with forcing climatic variables, please specify) in SWAT with the PET derived from MODIS? Is it Actual evapotranspiration? There is more interest comparing AET rather than PET.

Again, I do not understand the figure 7: PET from Modis is systematically higher than PET estimated by SWAT from the climatic variables (please precise the method priestley-Taylor, Penman, Hargreaves). It may be because natural land cover such as evergreen forest evapotranspirate more than PET (which is define by the evapotranspiration of a herbaceous cover). I have a doubt about the relevance of the corrected MODIS ETP. Please be more persuasive or try to find methodology applied in previous studies (Cheema et al. for instance)

I do not understand the Figure 8. Generally, captions of all figures give not enough details. Readers should be able to understand a figure by reading the caption.

Figure 9 is not convincing: it seems that the sensitivity of each parameters is tested using only 3 runs, with a spatially homogeneous value throughout the catchment. This is not realistic for AWC for instance.

I think this manuscript suffers from a lack of methodological review to assess the water budget using SWAT and remote sensing data together with a lack of methodological details.

I hope these remarks will help to improve the manuscript.

1. Alkama, R. et al. Global Evaluation of the ISBA-TRIP Continental Hydrological System. Part I: Comparison to GRACE Terrestrial Water Storage Estimates and In Situ River Discharges. J. Hydrometeorol. 11, 583–600 (2010).

2. Liang, X., Xie, Z. & Huang, M. A new parameterization for surface and groundwater interactions and its impact on water budgets with the variable infiltration capacity (VIC) land surface model. J. Geophys. Res. Atmospheres 108, 8613 (2003).

3. Masson, V. et al. The SURFEXv7.2 land and ocean surface platform for coupled or offline simulation of earth surface variables and fluxes. Geosci Model Dev 6, 929–960 (2013).

4. Ferrant, S. et al. Projected impacts of climate change on farmers' extraction of

groundwater from crystalline aquifers in South India. Sci. Rep. 4, (2014).

5. Cheema, M. J. M., Immerzeel, W. W. & Bastiaanssen, W. Spatial quantification of groundwater abstraction in the irrigated Indus basin. Groundwater 52, 25–36 (2014).

6. Ferrant, S. et al. Agro-hydrology and multi temporal high resolution remote sensing: toward an explicit spatial processes calibration. Hydrol. Earth Syst. Sci. 18, 5219–5237 (2014).

7. Ferrant, S. et al. Extracting Soil Water Holding Capacity Parameters of a Distributed Agro-Hydrological Model from High Resolution Optical Satellite Observations Series. Remote Sens. 8, 154 (2016).

8. Martin, E. et al. On the Use of Hydrological Models and Satellite Data to Study the Water Budget of River Basins Affected by Human Activities: Examples from the Garonne Basin of France. Surv. Geophys. 1–25 (2016). doi:10.1007/s10712-016-9366-2

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-44, 2016.