Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-126-RC2, 2019

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "On the use of high resolution satellite imagery to estimate irrigation volumes and its impact in land surface modeling" *by* Jordi Etchanchu et al.

Anonymous Referee #2

Received and published: 16 May 2019

The paper describes an attempt to improve the modelling of irrigation in the Land Surface Model SURFEX-ISBA. The method used in the study aimed in particular in improving the simulation of timing and amount of irrigation by considering high resolution remote sensing imagery. Objective has been to make more realistic simulations possible while keeping the approach generic enough to enable global scale simulations. The topic fits very well to the scope of the journal and in general, the article is well written. However, I cannot recommend publication of the present version of the article in HESS. My major points of criticisms are:

1.) One essential improvement mentioned several times by the authors very promi-

Printer-friendly version

Discussion paper

nently is the consideration of the Leaf Area Index (LAI) detected by processing high resolution satellite imagery from several sensors. However, the authors miss completely to describe why this is needed and why using LAI is better than using other vegetation indices that are easier to calculate. The authors used imagery with a high spatial resolution (10 - 20 m) and a high revisiting time 3-5 days (page 8, line 5). With the help of the neural network tool BV-NET they derive LAI from reflectance values. After that, they averaged the derived LAI spatially for the plots compared in the study and also averaged the values in time to derive monthly values used in the LSM. I don't understand why such an effort is made when later the data will be averaged. Furthermore, deriving LAI from reflectance requires to know about the characteristics of the crop grown in the field (canopy architecture, leaf angle, crop height). This information is not available for large scale studies and using standard crop parameters introduces a considerable uncertainty into the LAI calculations. I'm therefore not convinced that using the LAI results in any improvement compared to the use of other vegetation indices that can be much easier computed with lower uncertainty such as NDVI or EVI.

2.) I doubt that the methods used by the authors are appropriate for large scale application of the model. Many characteristics described by the authors, for example in section 3.1 (LAI when irrigation of maize starts, end of the irrigation period 45 days before harvest, irrigation rate, minimal return time), are only representative for maize grown in Southern France and I don't see any way to gather this information for other regions and other crops. Therefore I doubt that the approach is generic enough for global scale applications, an objective postulated by the authors.

3.) Timing of irrigation and irrigation volume is derived in the present study mainly by considering LAI dynamics and the simulated actual soil moisture content. The authors show that using variable thresholds for the soil moisture to trigger irrigation events results in more realistic irrigation amounts for the region studied. However, again I'm not convinced that this finding can be generalized. In many other regions the timing of irrigation is fix and just determined by the water rights of the farmer. Furthermore, when

Printer-friendly version

Discussion paper

irrigation water is free of cost and provided by big irrigation canals, farmers do not have any motivation to save water and consequently, they will use all the water that is supplied. This is completely different from situations where farmers pump their own water. Here farmers are more flexible but have to pay for energy and therefore tend to use less water. So my general impression is that the authors managed very well to adjust the model to better reflect the specific situation in the region which they studied and to improve thereby the accuracy of the model results. However, this is on the expense of more complexity and an increasing number of assumptions and parameters. I don't see how the authors can manage to derive and implement this background knowledge at global scale. Consequently, because of these limitations, I see the risk that the authors turned their global scale LSM into a more detailed model that can only be applied successfully at regional level when all the background information is available.

Specific comments: 1) The figure captions are to general. In any case abbreviations used in the legends should be explained.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-126, 2019.

Printer-friendly version

Discussion paper