Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-331-RC1, 2018

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Can global precipitation datasets benefit the estimation of the area to be cropped in irrigated agriculture?" *by* Alexander Kaune et al.

Anonymous Referee #1

Received and published: 9 November 2018

General comments

The paper aims to demonstrate the added value of global precipitation datasets derived remote sensing data and/or provided by re-analysis for the mapping of areas to be cropped in irrigated areas. This is done in this study by feeding an hydrological model using those global data sets. Surface water availability predicted by the model is then taken as a basis information to estimate the potential cropped areas. The specific question that the paper adresses is : when in situ measurements on water precipitation and water availability are limited in time, can global data sets, compensate their lack of accuracy by the extension of the measurement period thus, allowing to cover more

climatic variability ? The question is particularly relevant. It has already been adressed in several papers from an hydrologic point of view that evaluate indirectly the quality of the global precipitation data sets through an hydrological model and, in particular, the performance of the model to predict streamflow. The main added value of the paper is to go a step further as a second target variable is considered : the potential irrigated area based on water availability. A new metric is introduced based on the work of Kaune et al. (2017) that quantify the risk of sub-optimal water allocation to irrigation. This sub-optimal allocation can lead either to : a production loss if too large irrigation area have been planned or a so-called Âń opportunity cost Âż when too small irrigated area are proposed. The main conclusion of the paper is that the risk of choosing badly planned irrigation area based on discharge simulations with thirty years of one the global precipitation data set (CHIRPS) is found to be similar to using the observed discharge of five years. To my opinion, both the original approach and the results are of great interest for HESS readers.

My greatest concerns are related to the form of the document that I found difficult to follow. The different tools used in the study are described independently in the second part of the manuscript (Âń Method Âż). The result is that the reader discovers the approach and the different steps of the work gradually. The whole approach is (well) summarized in the first part of the discussion section. It could be moved earlier in the text, maybe at the beginning of section 2 and I propose also to add a scheme describing the workflow of the study. In addition, I feel that some important information concerning the hydrologic model description and implementation are lacking (cf. specific comments). Finally, as global precipitation products are at the center of the study and in situ data are available, an evaluation of the global data sets should be conducted as a preliminary step. This could give some quantitative elements for the discussion. Indeed, from the three precipitation data sets compared in this paper (CHIRPS, MSWEP and in situ network data), CHIRPS appear the best suited to plan irrigated areas . . . but we don't really know the reasons. Is it because it is able to better catch the real spatio-temporal variability of the catchment precipitation than the in situ data set because of station scarcity ? Maybe if the worst data set (MSWEP) appears to be strongly biased or strongly inaccurate and that the model calibration is not able to compensate for this data set deficiencies, it could be discarded the rest of the study (after section 3.1 “discharge simulations”). This could significantly simplify the results and discussion sections (and also the figures). Regarding this last point, I believe that the number of figures could be also reduced (cf. specific comments).

Regarding the comments above, I recommend a major revision.

Specific comments

2.1 A better description of the local climate should be provided : at least long term average temperature and precipitation together with average annual evaporative demand. In addition, if flooded rice is the dominating crop in the region. Typical phenological cycle should be given together with the months during which a water stress is critical for production.

2.2 Could you map the network of the meteorological stations (at least provide the number of stations) ? How is measured the discharge ? Is the river bed changing over time?

2.3 The different input of the hydrological model should be described. In particular, how are determined the physical soil and the vegetation characteristics ? Could you please explain how you determine a maximum soil moisture storage capacity max S of 176 mm from the physical soil and vegetation characteristics ? Calibration of the models (L19-29) should be re-written as it is not clear whether the model are calibrated on the full period or on five-years period. I assume the second option has been adopted. The best 5 models from the Monte-Carlo simulation with 10000 ensemble members are then kept which gives five models by 6 periods, 30 calibrated models. I think that the sentence beginning by Âń 10000 models . . . Âż is confusing.

2.4 The rationale of meeting water demand for 75% of the year seems relevant. By

contrast, the demand rate of 0.2 m3/s/km^2 is huge as it means 62072 m3/ha/year (common demands for wheat, olive orchard or citrus for instance are between 5000 and 12000 m3/ha/year). Maybe it’s ok for flooded rice but a reference should be given. What is the evaporative demand ?

Please better explain how the bootstrap resampling approach is applied to retrieve the empirical distribution of water availability.

(p.4 L15-16) Âń The areas that can be irrigated for each of the six calibrated models Âż Why considering only six models among 30 ? Are only the six best models considered here ?

2.5 Computing a realistic evapotranspiration reduction is not a detail for me as a hydric stress occuring at specific phenological stages of a crop can have dramatic consequences on yields and the effects are far to be linear. I agree that a first approximation as the one proposed by the author can be done but please reformulate to explain that a finer description of water stress should be considered in future studies.

3.2 Attributed to model errors and to forcing errors also ?

Âń Finally, for comparison, irrigated areas are derived using only five years of simulated data for each of the six five-year samples, where the simulated five years are the same as the five years used in calibration. Âż Please explain why you choose the five-year sample corresponding to the same used for calibration.

Figures

As already stated, the number of figure could be reduced:

- As the time series of discharge are not clearly readable, please choose one figure between figure 4 and 5.

- Figure 7 and 8 could be merged, it would also ease the comparison between the “real” water scarcity (figure 7 showing water scarcity with the thirty years of observed river discharge) and two example of five-year periods.

- Likewise, Figure 9 and 10 could also be merged. It would also be better for comparison purposes.

- The RUV using observed river discharges could be superimposed on figure 11 showing the RUV derived from simulated river discharges thus discarding figure 10.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-331, 2018.