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Comment on hess-2021-80

Anonymous Referee #1

Referee comment on "The application of Budyko framework to irrigation districts in China under various climatic conditions" by Hang Chen et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-80-RC1, 2021

General Comments

This article presents a study that extends the Budyko framework to irrigated areas and applies it on several districts in China. The topic is clearly relevant for HESS and this article may guide further studies aiming at taking into account irrigated areas in hydrological modeling studies. But at this stage, the proposed study relies on many hypotheses that are not tested/mentioned clearly and consequently, the reach of the results is difficult to assess.

1. Applicability of the Budyko hypothesis in unclosed systems

The Budyko framework is usually intended to describe/model the partitioning of water fluxes at the catchment scale. The catchment scale is important since it allows to work on a closed system, where inputs and outputs can be clearly and unequivocally stated. In the proposed study, the Budyko framework is applied on irrigation districts and the different fluxes considered are, to my opinion inter-dependent. For example in Eq. 4, equivalent precipitation is proposed as the sum of Precipitation, Irrigation and groundwater consumption. This suggests implicitly that the Irrigation water and Groundwater used for evapotranspiration are water fluxes originated from other sources than Precipitation falling over the district area, which is questionable. Consequently, using Eq. 4 may lead to count Precipitation fluxes twice since water provided by irrigation (and possibly groundwater) originate from precipitation. This may be true depending on spatial and temporal scales considered but this is not discussed in the paper. Both references cited in the paper to present Eq. 4 are not relevant since Wang et al. (2011) did not consider irrigation but water storage and Chen et al. (2020) considered catchment scale modelling.

2. Lack of clear validation with observed data

The authors propose a validation of ET using MOD16 product but it should be stated that the comparison to MOD16 ET cannot be viewed as a strict validation since MOD16 ET relies heavily on modelling. The validation using streamflow time series at catchment scales is to my opinion the unique way to perform a real validation with independent data. The interpretation of Fig.3 is also complicated since all variables (ET/Pe, ET0/Pe) are derived from computation with associated uncertainties that are very difficult to quantify at this stage. Interpreting the deviation of the simulations and “observations” is thus impossible.

3. Other comments

l.52-53: Phrasing problems.

l. 151: Why considering Pan evaporation in Eq.3 instead of Penman equation?

l.197-198: Computing effective rainfall is highly uncertain. Eq. 8 is a way to estimate it but may leads to large errors. The USDA SCS method provides alternative ways to take into account soil types and land use classes. Besides, I failed to understand why ET0 is not involved in this calculus of effective rainfall. I would expect that the authors quantify the uncertainties related to this estimation, or at least provide the magnitude of effective rainfall compared to rainfall and Irrigated fluxes

l.219 : Typo in y-axis label.

l. 237: Perhaps I missed something but why Pe is replaced with (I+P) in Semi-arid areas?

l.279-281. Is there a clear justification why w is different according to the climatic settings? I would expect that w be more likely dependent on land use, soil and vegetation types, not climate.

l.379: “Effective precipitation efficiency” is not clearly defined. How is it computed and what is really shown on Fig. 9?