

# ***Interactive comment on “Regionalization with Hierarchical Hydrologic Similarity and Ex-situ Data for the Estimation of Mean Annual Groundwater Recharge at Ungauged Watersheds” by Ching-Fu Chang and Yoram Rubin***

**Anonymous Referee #1**

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The manuscript by Chang and Rubin “Regionalization with Hierarchical Hydrologic Similarity and Ex-situ Data for the Estimation of Mean Annual Groundwater Recharge at Ungauged Watersheds” submitted to *Hydrolog. Earth Syst. Sci. Discuss* presents a new method for the estimation of mean annual groundwater recharge at ungauged watersheds based on the concept of hierarchical hydrologic similarity. Such a similarity is performed through a nested tree-based modelling approach, accounting for both the predictor-response relationship via a Bayesian Additive Regression Tree model and the predictor-predictor relationship via a CART model. The manuscript is within the

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scope of the Journal and it is certainly of interest for the Readers of HESSD. It is well written, in both presenting the research framework and previous literature and showing and discussing results. However, I have some minor concerns that in my opinion should be addressed before a possible publication on HESS. 1. Sect. 3.2 Climate. The choice of predictors is clearly related to the availability of data. In particular, being available recharge data only for 2002, one of the possible predictors is necessarily the precipitation in 2002 (and consequently the aridity index). However, being the recharge process highly non linear, and having demonstrated *a posteriori* the importance of the climate factors, I feel that the choice of a given year for precipitation (not the predictor precipitation in a given year) can potentially lead to different results, in terms of both predictive uncertainty and predictive accuracy. In particular, one of the strongest conclusions of the manuscript ("The most important message we get is the significant risk one would face if one considers aridity, or any climate variable in general, as the primary indicator of hydrologic similarity when AWC is low and aridity index is high") could be affected by the selected predictors. I think that this issue should be mentioned and discussed. 2. Section 3.4.1 (watershed partitioning). Line 21 ("Considering the logit normalized ... due to lack of data coverage"). I totally agree with Authors. However, in my opinion this could be an important limitation in the evaluation of the proposed method. Maybe the evaluation of the size of the data set for training, with respect to the size of data set for testing is somehow out of the scope of the manuscript, however this issue should be a little presented and discussed at least in the "limitations of the case study" sect. 3. Sect.5.2.1 ("Scale of the target response"). Here the Authors refer to Healy (2010) to face the problem of the scale of the target response, explicitly referring to the limitation of a baseflow analysis. They state: "At an ungauged watershed, it is unlikely that one would have enough data to verify the answers to these three questions". I agree, but the same comment can be done for predictors: "at an ungauged watershed, it is unlikely that one would have enough data for predictors". I know, this is a methodological paper, but methodology is addressed to a very practical problem. The issue of the transferability of the proposed method to real cases (which means the

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transferability in case of scarce availability of data) should be somehow faced, at least qualitatively. In the adopted case study, you have all possible data except recharge, which is an “extreme case”. 4. Sect. 5.2.2 (“artefact due to the partitioning of watersheds”). Comments on the not-transferability of information outside the overlapping range of predictors and/or targets are certainly shareable. It is shown here that one can not infer outside the observed ranges. However, if in an ungauged watershed I'm exploring a known or unknown horizon is indeed . . . unknown Minor comments on figures: 1. Figure 1. As this figure shows the BART approach in a general way, I would avoid referring to the specific case (annual recharge estimation) and adopt a more general notation in the panel (c) (in the present version of the manuscript “ex-situ” predictors and “ex-situ recharge data”) 2. Figure 2. Figure on the left side is illegible. If it does not contain useful information, please cancel boxes. 3. Figure 3. I think it could be useful adding also the distribution of long average P and long average Ep 4. Figures 8 and 9. Node numbers are illegible. As Authors often comment results on one given node, increasing the font size could be useful.

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