Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-561-AC2, 2019

Hydrology and Earth System Sciences Discussions

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Interactive comment

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

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Received and published: 2 March 2019

We thank the Anonymous Reviewer 2 for the appreciation of the study, and also for the detailed comments. Please find our responses in below.

General Notes:

1. At the end of introduction, we introduced the two objectives of this study. The first one is proposing an approach that features simultaneous full Bayesian quantification of uncertainty and non-linear regression to model the predictor-response relationship.

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Discussion paper

The second one is proposing a hypothesis of hierarchical hydrologic similarity and study the key controlling factors of a dynamic hydrologic similarity system.

The two sentences mentioned by the reviewer will be rephrased to avoid confusion.

2. We appreciate the review's comment. However, at this point we intend to keep Sections 3.4 and 3.5 in the case study Section. There are multiple ways to partition the data and multiple metrics with which we can evaluate predictive distributions. Sections 3.4 and 3.5 only introduce our ways that were applied in the case study, and thus are very specific to the case study. A generic study on data partitioning or distribution evaluation is outside the scope of the present study. The materials we put in Section 2 are general and independent of the case study.

To reduce confusion, Section 2 will be revised to be more general, and we will avoid including materials specific to the case study in Section 2.

3. We thank the reviewer for the comment. Data partitioning will be kept in Section 3 and removed from Section 2.

4. We agree with the reviewer. We will remove the Bayesian model averaging Section, but will still briefly mention it to explain how one extra step could be taken to refine the estimates.

5. We thank the reviewer for pointing this out. As we found it difficult to move the explanation of the transformation to Section 3.1 because the climate variables are only explained in Section 3.2, we made the explanation of the transformation its own sub-section, Section 3.2.1.

6. We thank the reviewer for the precious comment, and we agree that it is better to explain the approach in layman terms. As a matter of fact, in the beginning of Section 2 we mentioned that this paper will only provide a brief conceptual introduction to BART, and we provided two excellent studies for readers interested in the details. Explanation of the approach without equations will be added, and Section 2.3 and 2.4

will be revised.

7. Explanation of CART, citation to a paper, and the schematic diagram of a simple example of how BART models are nested under CART will be provided in Section 2.

8. We agree with the reviewer's understanding that in-situ means taken from/at the site/location of interest. Definition of "in-situ" and "ex-situ" will be added when they first appear. The reason the terms are used when we discuss partitioning is because we cannot evaluate accuracy at real ungauged watersheds. Therefore, we partition the data into training set and testing set, and treat the testing set as if they were ungauged watersheds without in-situ data, during the model training phase. With respect to the testing set, the training set provides the ex-situ data (i.e., not from the site/location of interest). The explanation will be added to the manuscript right before we discuss data partitioning, to reduce confusion.

9. We thank the reviewer for making such a suggestion, and we agree that studying the geographic distribution of may provide insights from a different angle. However, in the present study, as discussed in the introduction, we would like to avoid understanding hydrologic similarity with geographic space, and focus more on the predictor space, which can be explored with the nested tree-based approach.

10. We agree with the reviewer that a more intuitive name convention is always desirable. In fact, we tried showing the descriptions of all code-named predictor in the text and in the Figure. However, that lead to unnecessarily lengthy discussion and distorted Figures (in order to fit in the long description of some of the predictors). Thus, we have come to the solution of providing look-up tables. To alleviate the trouble brought by flipping to the tables at the back, we will submit the next draft with tables located near the texts referring to them, and the table will be simplified.

11. We thank the review for making this suggestion. The Figures will be revised for better clarity, and legends of the color coding will be added.

12. There is no comment 12.

13. We thank the reviewer for pointing out the confusing wording. As explained in Section 3, the target response in the case study was not annual recharge itself, nor recharge ratio, but the logit transformed normalized recharge. To reduce confusion and also avoid lengthy text, we will introduce the acronym “LNR” for logit normalized recharge which it first appears, and we will use the term LNR when referring to the target response in the case study.

14. We appreciate this precious comment, which is similar to one of the comments from another reviewer. This is an indication that we did not convey the message clear enough, and we will make corresponding revision for that. Agreeing with the reviewer, we acknowledge that some of the findings are specific to the case study, but the generality of the nested tree-based modeling approach is not. In a nutshell, the approach’s Bayesian feature sets it apart from other approaches, as the limitation in data accentuates the need to account for uncertainty. The nested structure allows modelers to account for model parameter uncertainty in each individual BART model, and account for conceptual model uncertainty by proposal multiple plausible BART models and comparing them under the nested structure. The nested tree-based modeling approach can help us obtain an informed empirical probability mass function of the plausible BART models (which was exemplified in the case study). This part of the contributions of the paper is general, and independent of the case study. The other part of the contributions (including the shift in dominant controlling factor, the pivotal role of soil available water content, etc.) is indeed specific to the case study, and we will try our best to discuss the two parts separately, to reduce confusion. The explanation above will be included in the revised discussion section, and we thank the review for this precious comment.

15. This will be added to the discussion mentioned in response 14 above. We will cite a comprehensive study on BART for readers interested in the details of training BART models. Like all models, the fewer data for training the more uncertain the model parameters. Our argument is not that BART is the most accurate model or the

most efficient one in terms of training, but that it offers a Bayesian representation of parameter uncertainty, which we think is of great importance at ungauged watersheds. The arguments above will be included in the revised discussion section.

16. Like the two comments above, we will discuss the generality of the approach, the importance of uncertainty, and the other findings specific to the case study in the revised discussion section.

17. We thank the reviewer for the appreciation.

18. We thank the reviewer for the suggestion. A brief explanation will be added to Section 3.4.2. We agree that more sophisticated division could help us learn something extra. However, as explained in Section 3.4.2, by no means do we expect our partitioning to yield an exhaustive list of all possible sets. We consider the effect of different proposals of plausible BART models (which represents different perspectives of the conceptual understanding of the underlying physics) an interesting follow-up that could be pursued in future studies, but beyond the scope of the present study.

19. We thank the reviewer for the suggestion. Dimension reduction of the data is certainly an interesting way forward. In fact, digging into the geology BART model, we found only a few bedrock types being frequently used as the splitting variables, and the others share a rather uniformly low appearance rate. When doing the case study, we did not have the lithological expertise to aggregate the lithology data ourselves, so we resorted to BART and let the data teach us about the dominant bedrock type. At the early stage of the study, we also tried performing principle component analyses before building BART models, and use the principle components as the predictors. However, we found that this obscured the interpretation of hierarchical similarity and the probability mass function of plausible models, so we turned our attention back to using the predictors as is. Like the response to comment 18 above, we consider the effect of dimension reduction and data aggregation an interesting follow-up that could be pursued in future studies, but beyond the scope of the present study.

20. We agree with the reviewer that the modeler has the opportunity to decide how to partition the data, and agree that a designed partitioning that makes the training and testing samples overlap could improve the robustness of the estimation. The reasons we adopt the MRB-based are listed in Section 3.4.1. To elaborate on reason 1, we would like to avoid training the BART models at the watersheds adjacent to the testing watershed. Adjacent watersheds may share a lot of similarities, and the confounding effects could obscure the results of interest. Reason 2 is a limitation and will be discussed in the revised discussion section.

21. We will elaborate on benchmark model, and will add a reference on kernel density estimation. It is actually quite naïve and does not require any background knowledge; that is why it is used as a benchmark.

22. Like the reviewer suggested it is not necessary, and will be removed.

23. Will be illustrated with a simple example and a schematic diagram.

24. We were explaining how data availability could hinder the application of physically based model. For example, a model of the vadose zone flow may require a water retention curve, which is not always available.

25. Each plausible predictor set corresponds to one BART model.

26. The explanation will be revised.

27. The details of Bayesian model averaging will be removed, but it will still be mentioned in the manuscript.

28. A map will be added.

29. The justification will be moved to the Section where the recharge data are first introduced.

30. The long term variables could compensate for the lack of data on antecedent condition. A detailed discussion will be added to the revised discussion section.

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Discussion paper

31. We agree that it would be ideal to have the same averaging period. Limited by data availability, we opt to make a working assumption of long-term steady state. This will be added in Section 3.2.

32. Yes. This will be made clear in the revised manuscript.

33. To avoid confusion a new subsection will be added.

34. Answered by the response to comment 18 above.

35. Answered by the response to comment 2 above.

36. One unique predictor set corresponds to one BART model.

37. We thank the reviewer for the appreciation.

38. Yes indeed, the benchmark model does not require predictors at all and is quite naïve and simple. Elaboration on the benchmark will be added in its own subsection to reduce confusion.

39. Instead of a one-line explanation, references will be added.

40. Will be rephrased.

41. Answered by the response to comment 19 above.

42. We could not find comment 42.

43. The algebraic explanation is provided in Section 3.5. Below is the descriptive explanation. From BART, we can obtain a predictive distribution that follows the form of a Gaussian distribution, where both the Gaussian mean and the Gaussian variance are uncertain and are modeled as random variables. What we termed "predictive variance" is the value of that Gaussian variance. Because it's uncertain, we estimated it with the sample median value. What we termed "estimate variance" is the variance of the Gaussian mean, which we estimated with the sample variance of the Gaussian mean.

44. Answered by the response to comment 10 above.

45. It was shown by the red horizontal line in panel (c).

46. Because that BART model only uses two predictors. Before see the results, we thought it would not outperform other models this much on average.

47. Answered by the response to comment 9 above.

48. We thank the reviewer for the suggestion. This Figure is supposed to be an example for the conceptual understanding. Instead of adding another Figure, we will revise the explanation in the manuscript and emphasize the take-away message from this example.

49. Title will be changed to “Nesting by RMSE”.

50. Answered by the response to comment 10 above.

51. Title will be changed to “Nesting by LPD”.

52. Answered by the response to comment 10 above.

53. We agree that the partitioning was not done perfectly. The reasons for the partitioning are shown the response to comment 20 above.

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