

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1  
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## Comment on hess-2021-424

Anonymous Referee #1

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Referee comment on "A method for predicting hydrogen and oxygen isotope distributions across a region's river network using reach-scale environmental attributes" by Bruce D. Dudley et al., Hydrol. Earth Syst. Sci. Discuss.,  
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In this manuscript, the authors produce isoscapes for the river networks of New Zealand, based on reach-scale environmental attributes. Their data and new maps for the surface runoff isotopes could be useful contributions in the region, although there are some issues related to the contributions, data, methods and results.

### Main issues

(a) The authors have to articulate their contributions clearly. They should not include irrelevant claims which take away people's attention on their real contributions of the work.

- The main contributions of this work can be (1) new isotope validation dataset (File S1; e.g. Additional monthly data for New Zealand in 2017-2020), and (2) the isotope maps of surface runoff based on precipitation isotope maps and other reach-scale environmental attributes.
- Our readers would want some more specific information related to the specific contributions of this paper on the data legacy and isoscapes in New Zealand.
- Instead of just giving a summary of general processes related to rainout or temperature effects of isotopes, which has been routinely discussed in other similar previous works, the authors could provide a review of the history of environmental isotope studies over New Zealand, so that they can introduce all the crucial datasets or sampling campaigns in the country.
- It will be good that the author can include the georeferenced maps (e.g. the GeoTIFF files) in their supplementary materials.

- One of the main contributions of this paper is that the authors generated surface water maps from a precipitation map. Therefore, please show the river network and catchments in Figure 1 to give people some ideas of how different isotope sampling locations can be related to their data sources or references.
- Although the author used water balanced methods, I did not really see any results related to surface flow mixing or patterns. Moreover, the authors have to recognise their main contribution of the work is not about isotopes in animals or plants. Only the implication of this work can be related to isotopes in animals or plants. However, the current abstract makes people think that the main topic of this work is about isotopes stored in animal and plant issues.
- In Section 3, the authors should articulate their overall results by removing irrelevant and weak discussions.

(b) The authors have to clarify the details of the data and methods. In this study, the used methods are a well-developed kriging approach. Although these used methods may not be a significant advancement for spatial analysis, they should be suitable for this manuscript's purpose. Even though it is somewhat expected, the authors showed their regression-based kriging was better than the ordinary kriging.

- The authors recognise that that "distance-based" geospatial and statistical interpolation is less appropriate (Ln 15 and Ln 54), but their regression-based kriging methods is still "distance-based" geospatial and statistical interpolation at the end of day.
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- In Section 2, there are not many details about how to select five environmental variables in Table 1 from Table S1 (Ln164-Ln165). There are some logic issues here. The authors used the small number of available samples to justify the use of stepwise regression to reduce the number of independent variables.
  - A table of the data for developing, calibrating and validating the models should be provided. Therefore, in the table, the authors should give the details of data sources (e.g. related publications), locations (e.g. south or north islands), sampling periods (2007-2009 in Ln 114) and number of samples (e.g. 51 sites Ln113).
  - The authors should think clearly why they choose the data between 2017 and 2020 for the residual calculation (Ln 126). The author mentioned a poorer longer-term fit in the other study (Ln 200). Let's think about it together here. For the annual values between 2007 and 2010, there could be only four data points for computing the correlation...

- At the moment, the model in Equation 3 is only a first order model of environmental variables. Authors may explain why they did not try to explore higher order models for the environmental variables.
  
- In Section 3, the authors should try to discuss how their selected environmental variables can be related to ground water and vegetative surface (Ln49-Ln50). The author did recognise that their model system was biased (Ln 403) which is very likely related to their selected environmental variables in Table 1.
- In Equation 1, there is no storage consideration. In the implication section, the authors should discuss how storage can affect their overall map results in Section 3.

(c) Some interpretation of results can be problematic and speculative. More discussion of the limitations of the study is needed.

- In L260-L285, the discussions and interpretations related to air masses, regional circulations and orographic effects are very speculating. These discussions are without much strong quantitative evidence in the manuscript.
- For example, the results in L223-L235 are very hypothetical. They are also very repetitive in the manuscript, because the authors repeat these speculations again in Section 3.4. Moreover, the current results are only marginally or speculatively related to cloud processes in Ln43.
- The authors should revise their discussion, similar to Ln 285-L302 where the authors discussed their result based on the fitted model variable results (e.g. usAnRainVar).
- For orographic effects, the authors may need to consider more about "aspect" and "wind" variables in their models, so that they can justify their discussion based on Kerr et al. (2015).
  
- As I have mentioned in my first comments, the results of this work are unlikely to be useful for studying movement of aquatic organisms (L430). The current maps are only for hydrogen and oxygen. There were no other isotope results such as nitrogen. In general, the discussion of animal and plant tissues (Ln10) is far-fetching in this manuscript. The results of this paper are not really giving much insights into them.

- The system bias of this study (L403) is unlikely to help others improve understanding of isotope patterns. Therefore, the authors should try to reframe their writing by reducing their discussion based on speculations, and suggest more how we can improve our understanding patterns of precipitation isotope values by using hydrological process-based models to investigate how flow and evaporation processes affect isotope patterns.
  
- Currently, I did not see much mixing and surface flow results which is suggested in Ln16. I also did not see the dam results mentioned in Ln13 and Ln68.
- Until the authors could have results similar to Figure 7 for all the main catchments in New Zealand, the discussion in Ln355 - Ln379 could not be justified. For example, there are no similar results of Figure 7 for the South Island in the manuscript.
- Perhaps, the authors can have more discussion on how results in Figure 7 are related to the "dendritic" patterns (Ln62).
- More insightful thoughts on variations between precipitation and surface water will be useful to demonstrate the values of this work. It would be great to have more quantification and discussion on how the precipitation and new runoff maps could be different in terms of their patterns.

Overall, the data of this work could be useful regionally.