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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Global isoscapes for δ^{18}O and δ^{2}H in precipitation: improved prediction using regionalized climatic regression models" *by* S. Terzer et al.

S. Terzer et al.

stefan.terzer@gmail.com

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We greatly appreciate the reviewer's comments and suggestions, and have the pleasure to submit our responses. As noted by the editor, most of the comments were minor, or required clarifications, but we have attended to each query in detail below.

RCWIP is not a static product but is scheduled to periodic updates by the IAEA as new data arrives, thus the comments presented by the editor and the referees will also be considered in future updates of the publicly available RCWIP model grid files.

Finally, we encourage HESS readers to consult the IAEA web page hosting the grids

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and maps, which has been established at:

http://www-naweb.iaea.org/napc/ih/IHS_resources_rcwip.html,

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Response to reviewer's comments:

Reviewer #1:

[. . .] However there are two minor points that the authors should address: 1. The symbol Rˆ2 is termed either as correlation coefficient (pages 7354 and 7366), which is wrong or as coefficient of determination (e.g. page 7361) which is correct. The term coefficient of determination should be used for Rˆ2 throughout the text.

RESPONSE: Agreed. All references to the symbol Rˆ2 to "coefficient of determination" will be changed in the revised manuscript.

2. Kriging variance is a measure of the spatial configuration of the sampling sites. The only information about the data values passes through the calculation of the variogram. Thus the kriging variance is not a robust measure for the comparison of different models. A combined calculation may be used containing variances from the interpolation method, from the gridded data and from the model development (e.g. Lykoudis et al.2007). Also, cross validation may be used for the comparison of different spatial models.

RESPONSE: The core issue is how to best portray and quantify the comparative outcomes of different geospatial models in order to determine which gives a better result. The comparison of the two models (M1 vs. M4) was based on regression residuals (upon which a regression uncertainty is computed), which in turn served as input for kriging. Hence, if regression residuals are lower, kriging input data shows less variance, which then results in lower kriging error. Therefore, the use of the kriging error is sufficient for the comparison of the two approaches between M1 and M4. However, we agree with some points on other potential statistical approaches; including that re-

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gression uncertainty could be incorporated as additional uncertainty estimation into the future versions of the RCWIP model.

Concerning 'kriging variance', we partially agree, and suggest rephrasing to 'variance of the kriging estimator' (where σˆ2 is concerned) or 'kriging standard error' (where σ is concerned). We agree with the reviewer's comment that input value and distance related information is included in the (semi-)variogram. However, it must be pointed out that the variogram function is a key weight in any subsequent kriging equation, and is actually a critical element of the kriging method's superiority over deterministic methods.

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Reviewer #2:

[. . .] I do have some minor suggestions to improve the manuscript. The influence of M2 on the results remains unclear. When the data is to scarce M2 is used instead of M4, what is the spatial extend of the influence? How many stations are pure M4, etc. A discussion and visualization would help to understand that. [see RESPONSE point 7.] Other than that, I only have minor comments.

1. Specific comments. P7355 L21: Fig.2 is mentioned before Fig. 1

RESPONSE: Reviewer is mistaken; Fig 1 appears before on p. 7352, line 24. No action taken.

2. P7358 L20: Choosing m=1.5 seems somewhat arbitrarily. Determine an appropriate value for fuzzy c-mean study is always challenging, nevertheless some justification is needed here.

RESPONSE: The selection of a smoothing factor m is indeed challenging and often arbitrary, as Cannon et al. (1986) point out in indicating a span of 'useful' m values. While a smoothing factor of 1.0 would have resulted in line boundaries (no fuzzy transitions), we experimented extensively with the smoothing factor and found out that m >

1.5 would have led to inordinately large clusters extending, for example, from one continent into another ignoring the ocean in between. Since our approach was focused on land areas, we empirically found 1.5 was a suitable (still arbitrary) compromise to balance between (within the range of the meaningful in fuzzy clustering) terrestrially meaningful clusters, and to obtain smooth transitions between them.

3. P7359 L5: Fig.1 mentioned after Fig.2

RESPONSE: Reviewer is mistaken; Fig 1 appears before on p. 7352, line 24. No action taken.

4. P7363 Lines 20-22: Please provide some reasons of setting specific relationship between nugget and sill for the variograms.

RESPONSE: Our aim was to subject any of the models tested (12 monthly for oxygen and hydrogen, plus one annual each, for both M1 and M4, in total 52) to the Principles of Identical Treatment (PIT). We had to be sure all models would be undergoing exactly the same procedure in order to make apples-to-apples outcome comparison. However, when having the kriging model parameters (nugget, sill and range determined automatically by the 'gstat' software, a number of experimental variograms ended being unique, with the model function not matching the curve given by the empirical variogram. Hence, we used a parsimonious PIT approach that would not result in singular variograms. We concluded that for any of the variograms a nugget effect must be a given, resulting from sampling error, analytical error, and other artefacts related to the isotopic data, and that the modelled variogram function cannot pass through the origin (e.g. no sampling error). As for using the lowest nugget possible, a suitable minimum γH was chosen. For estimating the sill, we sought to fit it to the variogram function's variations which were well represented by a multiple (we found 2.25 to come closest when comparing with correctly auto-determined models) of the σ of γH, added it to (half the) minimum of γH. We are well aware that this conservative PIT approach might not result in a perfect nugget/sill/range combination for each of the 52 models tested,

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but at the least comparability was maintained in all steps of the prediction / comparison workflow, which we deemed to be of greater importance for the RCWIP comparison approach.

5. P7367 L12: The use of significance does not seem to be related to a statistical test and more of a subjective comparison between the RMSE values. The use of significance/significantly/ significant should always be related to a test of significance and a corresponding p value. I would suggest using clearly.

RESPONSE: Agreed. The term 'clearly' will replace 'significantly' – noted for revised MS. However, we wish to emphasize (without going in detail) that p-values were determined for the 463 regression combinations and were all <0.01, and for 435 of them <0.001. We hope to incorporate p-statistics in future versions of the model as part of the regression model and/or coefficient selection process for each zone/month combination.

6. P7368 L15f: Authors usually exemplify their results with the 18O data, why now changing to the 2H data? Would be more consistent using 18O here. Please explain.

RESPONSE: From Figure 1, 18O and 2H outcomes are strongly correlated. Our intention was to portray only one isotope here to reduce the number of journal figures, especially graphical outcomes are more or less the same. We used 2H to provide some contrast with 18O only maps throughout the manuscript (had we used 18O, the reviewer may have asked why not include 2H?). If the editor insists, we might replace the Figure with 18O (or include both), but since they look the same we suggest this Figure is left as is.

7. P7369 L1ff.: It would be nice to visualize better where M2 and where M4 was applied, although I understand that this will be based on the fraction of each climate cluster.

RESPONSE: We agree with the reviewer that this is useful information, but it is very

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difficult to portray graphically because the stations are nested within various climatic clusters, and a simple global proportioning is therefore not useful. To accommodate this request, we prepared a map of M4 coverage (annual δ18O values portrayed, attached as Fig. 1) which we propose to include in the supplementary materials, combined with adding a sentence to page 7365/line 25, indicating that this information is available.

8. P7371 L 9: Use of “significantly” without the use of p value and a statistical test, please change.

RESPONSE: will be changed to “clearly. See also point 5.

9. P7380: Font in figures (Figure 1) might be too small for final print.

RESPONSE: Issue noted, will be fixed in revised manuscript.

10. P7384 Fig 4 caption Line 2: It should be ‘five climatic clusters’ but not ‘five climatic lusters’.

RESPONSE: Issue noted, will be fixed in revised manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 7351, 2013.

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Interactive Comment

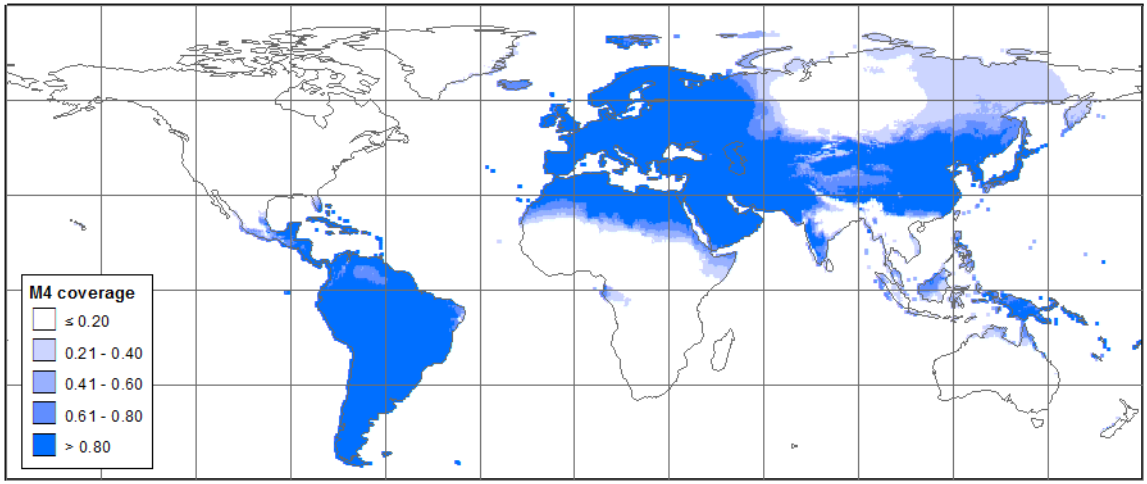

Fig. 1. coverage fraction of M4 for annual δ18O

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