Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/hess-2021-156-RC2, 2021

Comment on hess-2021-156

Anonymous Referee #2

Referee comment on "On the selection of precipitation products for the regionalisation of hydrological model parameters" by Oscar M. Baez-Villanueva et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-156-RC2, 2021

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Review of Manuscript # hess-2021-156

Title: On the selection of precipitation products for the regionalisation of hydrological model parameters

Baez-Villanueva et al.

Summary

The study investigates the impact of four different gridded precipitation products (ERA5, MSWEPv2.8, RF-MEPv2, and CR2MET) having different spatial resolutions on the relative performance of three regionalization techniques (spatial proximity, feature similarity and model parameter regression) over 100 near-natural catchments in Chile, characterized by varying topography, climate and hydrologic regimes. TUWmodel, a semi-distributed HBV-like model, was driven separately by four precipitation products and calibrated/evaluated for each catchment using two performance metrics (KGE' and AOF) utilizing daily streamflow. The authors compared the performance of the regionalisation techniques through a leave-one-out cross-validation exercise, which consists of leaving out each one of the 100 catchments, transferring model parameters, conducting flow simulations and computing performance metrics. They concluded that the calibration procedure is able to compensate for the differences in precipitation products and the spatial resolution of the precipitation product does not largely affect the regionalization performance. Overall, feature similarity provided the best regionalization performance followed closely by spatial proximity, while parameter regression performed the worst. They also reported that the hydrologic regime impacts the performance of regionalization.

Overview Comment:

I would like to thank the authors for conducting this interesting work. The topic of the manuscript fits well to the journal scope and readership. The use of language and structure is well. The authors put significant effort to summarize the outcome of substantial number of cases (four precipitation products, three regionalization techniques, 2 objective functions as well as hydrological regimes) into a single manuscript. However, it is still difficult for the reader to follow through and understand the full reasoning behind some of the study outcomes. My suggestion for improving the manuscript would be to explain the link between different precipitation products and calibration/verification and regionalization methods through differences they make in model components (parameters/fluxes) and components of the efficiency metrics (KGE and flow duration curve segments). I listed my main and minor suggestions for improvement/revision in the sections below.

Main Comments:

1) Discussion of how the model components compensated for differences in precipitation products: There are significant differences in precipitation products (for example ERA5 reported four times higher precipitation in the dry north), however most of the calibrated model parameters have similar distribution for ERA5 and other products (Figures S4 and S5), except SCF, Lprat, FC, beta (I am surprised to see that parameter distributions barely touch the limits). An analysis of how the model compensated for differences in precipitation products through parameter values and fluxes other than streamflow will significantly improve the reasoning behind study outcomes. For example, did the large bias in ERA5 in dry regions compensated by more evapotranspiration/groundwater loss etc? The authors also mentioned this topic in Lines 64-67: "Although hydrological model calibration can partly compensate for errors in the representation of P, this may lead to unrealistic model behavior, thus affecting the quality of parameter regionalisation results" and in Lines 457-459: "The equifinality of model parameters may also impact the relative performance of the regionalisation techniques by producing unrealisitic parameter sets, particularly for the case of parameter regression.". Therefore, providing answers to these statements will significantly improve the manuscript.

2) The authors utilized KGE' and signature-based objective functions for calibration, both of which can be decomposed into hydrologically meaningful components to understand differences in model performance driven by different precipitation products as well as in different hydrological regimes. An analysis of how well different hydrograph characteristics (variability, bias, low flows, high flows etc) are reproduced through regionalization will significantly improve the manuscript.

3) Line 226: More information on the optimization algorithm parameters should be provided, such as termination criteria, maximum number of iterations permitted etc. to better understand the calibration procedure and its impact on calibrated parameters.

4) Lines 408-410: ERA5 re-analysis product is also a merged product and uses ground-based measurements (after 2009). Please discuss and revise where necessary. (see Hersbach et al., 2020; https://rmets.onlinelibrary.wiley.com/doi/10.1002/qj.3803).

Possibly focus on differences in gauge density used in correction of different precipitation products.

Minor Comments:

Line 123: replace "perform well over small catchments" with "perform well even over small catchments"

Line 124: replace "tend perform" with "tend to perform"

Line 127: The reader would be interested to see how well CR2MET performs compared to ERA5. A few sentences based on the reference will be helpful.

Line 128: CR2MET: Boisier et al (2018) states that CR2MET is produced from ERA-Interim but not ERA5. Please check.

Lines 149-154: It will be interesting to report the control of elevation to the precipitation differences in the products. Representative West-East elevation and precipitation cross sections for three zones may help in this regard.

Lines 166-171: There are over 20 references in 6 lines. Reducing this number to 2-3 for each point the authors are targeting may help brevity. Same in line 225.

Figure 4: Caption: Replace "Leave-out-out" with "Leave-one-out."

Figure 5: The ECDF color-coding description should be corrected in the caption based on the legend (calibration-green, Ver-1 blue). I also suggest drawing the country outline thinner and with less intensity to improve visualization of the markers. If possible, a light gray shaded relief map will add elevation information.

Line 325: CR2METv2 – please use consistent acronym throughout the manuscript (earlier use was CR2MET).

Lines 335-337: "Choice of objective function does not impact the spatial performance of regionalization methods": This sentence seems to be an overstatement because different objective functions are sensitized to different components of the hydrograph (e.g log

transformed flows will put more emphasis on low flows) and hence put more emphasis on fitting different model parameters. Instead of using a general statement, please use a statement valid for the present study. In Figure 5, comparison of ECDFs for P product columns between KGE' and AOF metrics indicates some differences. For example, compare feature similarity ECDF for ERA5 between KGE' and AOF metrics.

Lines 347-348: Please discuss which catchment characteristics are lacking that represent the hydrological behavior in this region.

Lines 354-355: "For feature similarity, all P products yielded a performance similar to that obtained for individual basin verification during the dry Verification 2 period". This statement somehow contradicts with the information provided in Figure 6. In Figure 5, ECDFs for regionalization and calibration/verification represent different time periods and hence their comparison may yield to misinterpretation. In Figure 6, however, regionalization and calibration/verification periods overlap and it can be seen that feature similarity performance is lower than Verification-2 performance.

Line 376: Indicate that Figure 8 is for AOF.

Figure 9 and Lines 401-402: "The performance of parameter regression did not change substantially when evaluated with and without nested catchments." Actually, the performances with and without nested catchments look precisely the same (median values, distribution etc.). Is this related to using one RF model for each TUWmodel parameter? (see Lines 283-287). Please clarify here (Lines 400-401) and in Lines 283-287). I see some explanation in Lines 499-502, but more direct evidence should be provided.

Lines 416-417: MSWEP is also a merged product as stated in Line 411? Please clarify.

Lines 422-423: See earlier Comment on ERA5. Also, ERA5 reported 4 times more precipitation in the dry north (Figure 2) that may lead to low regionalization performance.

Line 424: Please revise the language: "The use of ground observations as they:"

Line 465: Please clarify "opposite" here. Does it refer to the regime?

Line 477: Please move the left parenthesis before the year of the reference.

Line 489: Please indicate “snowmelt” recession curves.

Line 494: Typo “Figure” 9.

Line 512: Remove “model” after TUWModel.

Boxplots: Please describe box limits and vertical line (median) in first boxplot figure caption.

Lines 518-519: See earlier comment on ERA5 (merged product).