

Hydrol. Earth Syst. Sci. Discuss., referee comment RC3
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Comment on hess-2022-21

Anonymous Referee #3

Referee comment on "A gridded multi-site precipitation generator for complex terrain: an evaluation in the Austrian Alps" by Hetal P. Dabhi et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-21-RC3>, 2022

This paper discusses the results obtained using a space-time generator for daily rainfall in the Austrian Alps. This is an interesting topic and the paper is generally well written. The proposed model is very close to the one proposed by Kleiber et al. (2012) and the methodological contribution of the paper is rather limited. However, the authors have done an impressive work to validate the model on a complex data set. I think that this may be of interest for the readers of the journal Hydrology and Earth System Sciences. I suggest that the authors take into account the following comments before submitting a new version of their paper.

Line 3. "...as well as future climat ". Indeed, this would be great, but this is not discussed later in the paper. I think that it should be removed from the abstract or discussed in the paper.

Line 5 . "... propose an extension...". Please detail, contributions should be clear when reading the abstract.

Line 38 "...typically of 1 km for spatial and daily for temporal scale". Could you precise an

application where such scales are involved? I am not a specialist in hydrology, but I have the feeling that if you consider such a high spatial resolution, then the temporal resolution should also be finer?

Line 90. "Such WGs are of limited use if the observed gridded data are not available which is often the case". One option would be to "interpolate" the data on a grid before fitting the Wgs. Could you comment on the benefit of the proposed approach versus interpolation? It may be possible to obtain high quality gridded data using an interpolation method which merges all the available sources of information (meteorological stations, but also radar, models and so on), whereas the inclusion of such information in the proposed Wgs does not seem straightforward?

Line 100 "However, Kleiber et al. (2012) tested the model only for the multi-site precipitation generation, i.e. at locations with observation and not for the generated gridded data of precipitation." Does it make an important difference? If yes, please detail.

General comment on Section 2. I find that the statistical methodology is not described precisely enough. The reader sometimes has to guess how the model is defined, and I do not think that there are enough details for someone who would be interested in reproducing the results. This is especially true in Section 2.2, but I think that more details should also be given in Section 2.1. Please also

- explain how the model is fitted to the data, eventually provide the codes,

- give the number of parameters involved in the model,

- comment on the computational time to fit and simulate the model.

Line 164 "The Gaussian process itself provides a spatial interpolation method 'kriging' so that the model parameters β_0 associated with each covariate, which are estimated at observation locations, can be interpolated to any location of interest." Not clear for me, please reformulate.

Line 230 "To reduce uncertainty and add more robustness to the observations, we increase the sample size of the observed data by considering a 7-days window centred at the day of interest." This sentence is mysterious for me, please reformulate.

Table 1. Is this table useful?

Line 267. "We select the covariates using both AIC and BIC ...". If you consider AIC/BIC, then the model was fitted using Maximum Likelihood? Or only part of it?

General comment on Section 3.3. The authors have done an impressive work for validating their model. However, I have the feeling that the two following aspects are important but not discussed and thus should be further considered :

- Cross-validation. If I understand correctly, no cross-validation is performed although it is usually done when validating spatial Wgs. Cross-validation would consist in removing some stations when fitting the model and, then check if the model is able to generate realistic precipitations at these stations by comparing simulation and 'true' data. It may give confidence in the ability of the model to generate precipitation at locations where no data are available.

- Spatial dependence. The spatial dependence structure, which is an important aspect for many hydrological applications, is discussed only in the discussion (Line 585-610) and supplementary material. This should be discussed in Section 4.

Line 292. It is not clear for me why the authors use tolerance intervals instead of confidence intervals. Confidence (or fluctuation) intervals have the advantage of being widely used when validating Wgs and easily understood by most readers.

Line 300. "To quantify the model performance...". Is it useful to have all these criteria? Do they bring complementary information? It takes space in the paper (with tables and plots) but it is barely discussed in the paper.

Line 395. The Kolmogorov-Smirnov test and the Wilcoxon-Mann-Whitney test are valid for continuous distributions, whereas rain gauge measurements are usually discrete (e.g. every 0.2 mm for tipping-buckets). Could you comment on the validity of the tests in such situation?

Line 496. "It is evident that by allowing the elevation as a covariate in the kriging interpolation for prediction at each grid point, the amount of precipitation is considerably improved". Is it so obvious? Maybe there are some improvements, but I don't have the feeling that they are 'considerable'!

General comment on Section 5 and 6. There are repetitions in Section 5 and 6. I suggest that you concatenate both Sections and try to make it shorter.

