Authors response to comment on hess-2022-21

We are thankful to Reviewer-2 for the time and feedback on our manuscript. In the following we provide our response comment by comment. We have made several changes according to the comments by the reviewer in the revised manuscript which are highlighted by magenta colour. The texts in blue colour are the changes already made in the previous round of the review process.

This is my second review of the paper entitled "A gridded multi-site precipitation generator for complex terrain: An evaluation in the Austrian Alps" by Hetal Dabhi and co-authors. My general opinion about the paper did not evolve much since my first review because, except for the implementation of a cross-validation experiment, most of my comments have been rejected without convincing counter-arguments.

To start with a positive note, I am glad to see the addition of a cross-validation experiment, which is convincing.

A more debatable aspect is the new statement (L 151) that "elevation dependence in the covariance structure is the natural assumption in complex terrain". I may accept "is a natural assumption" if more details were given (for instance by discussing in more details the references Wilks (1999, 2009)), but it is definitely not "the" natural assumption.

Beyond the choice of phrasing, I red with interest the two above references that are cited by the authors to justify the choice of an elevation dependent covariance. I acknowledge that in Wilks (1999) an altitude dependent covariance function is used, but I would like to draw your attention to the fact that in this study, the authors used a model selection step which leads to sometimes remove altitude from the covariance function of precipitation occurrence (2 months over 12), and most of the time for precipitation amounts (7 months over 12). In my opinion this shows that this kind of parametrization is fair (and I would like to emphasize here that I have no problem if you want to keep it), but should be handled with care (hence the necessity to discuss it thoroughly, and not take it as obvious). In addition, it should be noticed that in Wilks (2009), the same author does not use altitude in the covariance function of precipitation anymore, but keeps this parametrization for temperature only. Which makes sense to me, because vertical lapse rates are more obvious for temperature than for precipitation.

To sum up my opinion on this point: I am ready to be convinced that the proposed covariance function incorporating altitude is worth implementing in your context, but you need to put a bit more efforts in justifying why.

Response: First, we would like to mention that in Wilks (1999), the model is implemented in the complex topography with highest elevation of 2500m, so there is elevation dependence included in the covariance structure. Whereas in Wilks (2009), the model is implemented in a region with the highest altitude of approximately 900m only and hence altitude was excluded from the covariance structure of precipitation. Elevation was included for modelling temperature in Wilks (2009) because, as the reviewer already have mentioned, temperature lapse rate has a sharp gradient.

Now, concerning the term 'the natural assumption', it is also a fact that elevation affects precipitation. It is well documented in literature that high altitude often receives more precipitation. One can find many such studies where precipitation modelling involved elevation in complex topography. Gafurov et al. (2006) showed the relationship between precipitation and elevation (also known as 'precipitation lapse rate'). Sasaki and Kurihara (2008) also showed precipitation lapse rate and pointed out that the correlation between precipitation and elevation is weak but statistically significant. Daly et al. (2008) pointed out that the relationship between elevation and precipitation is highly variable, but precipitation generally increases with elevation with exceptions when terrain rises above the height of a moist boundary layer or trade wind inversion. Thus, one can say that elevation is a significant predictor for precipitation in the mountains, especially in a complex terrain like the Austrian Alps where the highest elevation in our study area is 3533m a.s.l., it is natural to consider elevation as a predictor variable. Zhang et al. (2021) showed that the micro-physics also vary with altitude (due to a temperature and pressure dependence).

Also, we already have given two references on line 176, where the authors used elevation for precipitation interpolation in the mountains and also showed that KED outperforms OK. In Hiebl and Frei (2018) one can find the use of KED for constructing high-resolution (1 km) gridded daily precipitation climatology for Austria which indeed includes our study area. Hence, including elevation for precipitation modelling in the mountainous region is well 'a natural assumption'. We admit that the use of the article was wrong in the sentence and it should be 'a natural assumption' and not '*the* natural assumption'. This is corrected in the revised manuscript on line 152.

We hope our answer justifies the use of elevation in the covariance structure.

Finally, there are several aspects that I still find problematic:

1) I have the impression that the absence of nugget in the Kriging step may generate artifacts, and your response to my comment as well as the associated changes in the manuscript (which are restricted to acknowledge the absence of nugget) are not convincing.

Kleiber et al. (2012), which was the starting point for the present study, used a nugget effect when kriging model parameters. You decided to remove this nugget. This may be Ok (I honestly don't know), but you have to justify it. This can be done either by explaining why do you think there is no small scale variability in the parameters to interpolate, or (even better) by showing that these parameters do not display small scale variability. For the later option, the best solution is probably to show that the empirical variograms of the parameters interpolated by Kriging are close to zero for short lags.

Response: We would like to point out that the nugget has not been forced to be zero but rather it is estimated as a very small value – nearly zero which we have allowed as it is. Since the reviewer suggested to give information on the nugget term, we mentioned it. However, with the changes in the manuscript where we have added information on the variogram. It has been estimated using MLE and the estimated value albeit close to zero is what we have allowed in the model. We do not think it is necessary to show the variogram in the article. However, for an example we present one of the variograms here (Figure R1).

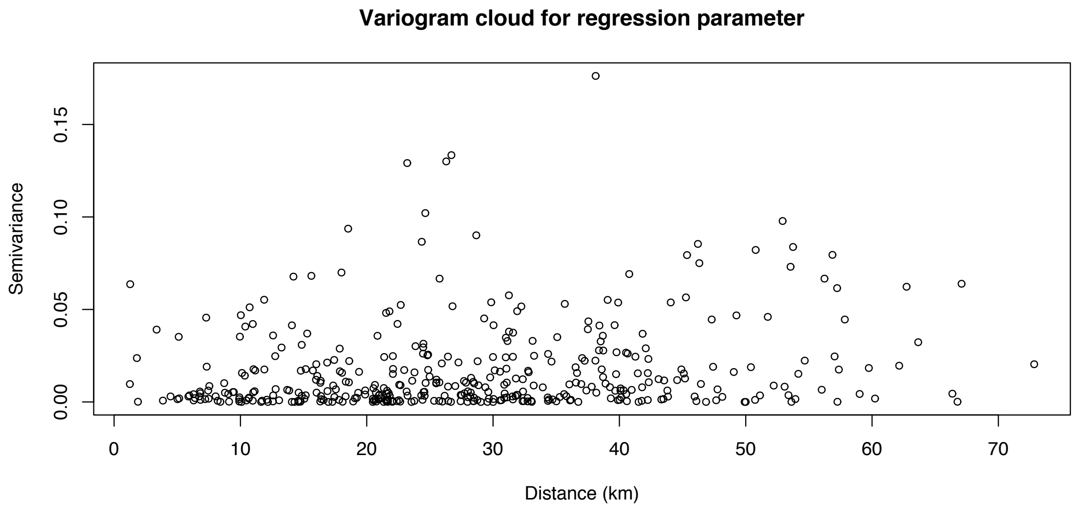

Figure R1: Variogram cloud for one of the regression parameters in the GLM for occurrence

Necessary changes have been added in the revised manuscript and have been highlighted on L168-170, L180-184 and L186.

2) The figure R1 displaying scale paramater=f(elevation) and shape parameter=f(elevation) in the response to my major comment 2 (in the response to reviewers file) does not show any clear relation between the parameters to interpolate and the external drift, and therefore does not really justify KED.

In the same line, I don't think that the new figures 16 and 17 (formerly Fig 19 & 20) show a clear advantage of KED vs OK, nor of isotropic vs anisotropic covariances. This is in line with the comment 14) of Reviewer#3: "Maybe there are some improvements, but I don't have the feeling that they are 'considerable'!", and the changes made in the manuscript (L 567) are in my opinion too minor to reflect this shared concern.
I therefore strongly recommend to the authors to rewrite the discussion of Fig 16 and 17 to clearly acknowledge that the improvements associated with the use of KED and anisotropic covariance are rather limited.

Response: We agree with the reviewer that the relationship between the parameters and elevation is not strong. As described in our aforementioned response to the main comment, Sasaki and Kurihara (2008) pointed out that the correlation between precipitation and elevation is weak but statistically significant. Please read our response to the main comment. Regarding the use of the anisotropic covariance, we already have acknowledged the performance of the model in the conclusion on L732-L733 in the revised manuscript.

For Figure 16, we agree that saying KED outperforms OK in ALL the months can be criticized. Hence, we have modified the sentence and highlighted the change in the revised manuscript on L570-L573.

For Figure17, it is noteworthy that the presented statistics for dry/wet spells is a frequency **per year**. Since the anisotropic model has a slightly positive impact on the frequency of dry spells, an improvement of say, 2 dry spells per year means in 30 years of data there will be a difference of 60 spells which is indeed not negligible. Therefore, in our view, the model performance can be said *better*.

3) The explanation about the absence of outliers at Prutz station (in the response to reviewers file) and the reason why you don't want to investigate it further are not convincing (that is the least I can say). Why don't you want to display the data to understand what is going on? Either the data from this station is an outlier, and removing it will improve the performance of your model. Or there is a micro-climate at this location, and this is interesting to point out. In any case this must be discussed in more details.

Response: The reviewer stated in their question in the first review that "*for Prutz, Ried in Oberinntal, Fendels and Ladis to be sure that the modeling problem does not simply originate from instrumental errors at Prutz station…*"

In our answer, we have emphasized the fact that all the stations in our study are of high quality, so we (again) stand by our response that there is no scope for claiming that the data could be erroneous.

Now, as for the precipitation characteristics at Prutz being dramatically different from the surrounding stations, it is really interesting to investigate the reason for that, but that is not the goal of our study and is indeed out of the scope of the study. We have spent substantial amount of time over this station to figure out the reason for it being an outlier. We also would like to point out that the reviewer writes "***Either the data from this station is an outlier, …",*** this statement is not clear to us (whether the whole data set meant or just one datum). However, we assume that the reviewer meant the whole dataset from Prutz is an outlier.

We would like to point out that we already have removed this station in the cross-validation experiment and showed that the results are not affected by this station. This is also one of the findings of our study that one 'outlier station' is not affecting the results if it is surrounded by a good density of observation locations. Contrary to that, an outlier station can affect the spatial structure in the generated data if it is in a sparse network of observations which is the case with the station St. Martin in South Tyrol. In our view, these two are important findings which we have stated in the manuscript on L663 to L673 and which can be useful for other researchers in their analysis. Also, we again emphasize that removing a station away from the study because it exhibits different characteristics from other observations is not a good scientific practice — especially in the mountainous region where precipitation has a highly variable nature.

As for the comment about displaying the data for investigating the reason for Prutz being an outlier, we don't see the need for displaying the raw data because looking at the raw data one cannot investigate the reason for the presence of the outliers or micro-climate in the data. We also do not neglect the possibility of micro-climate at the location. It is not that we didn't make any attempt to investigate the possible reasons for having outliers. If the data had revealed any possible reason/s for the presence of outliers during our investigation, then it would be one of the important findings and we certainly would have discussed it with graphical illustration in the manuscript. However, we are displaying the raw data here for Prutz and its one of the nearest sites Fendels in Figure R2.

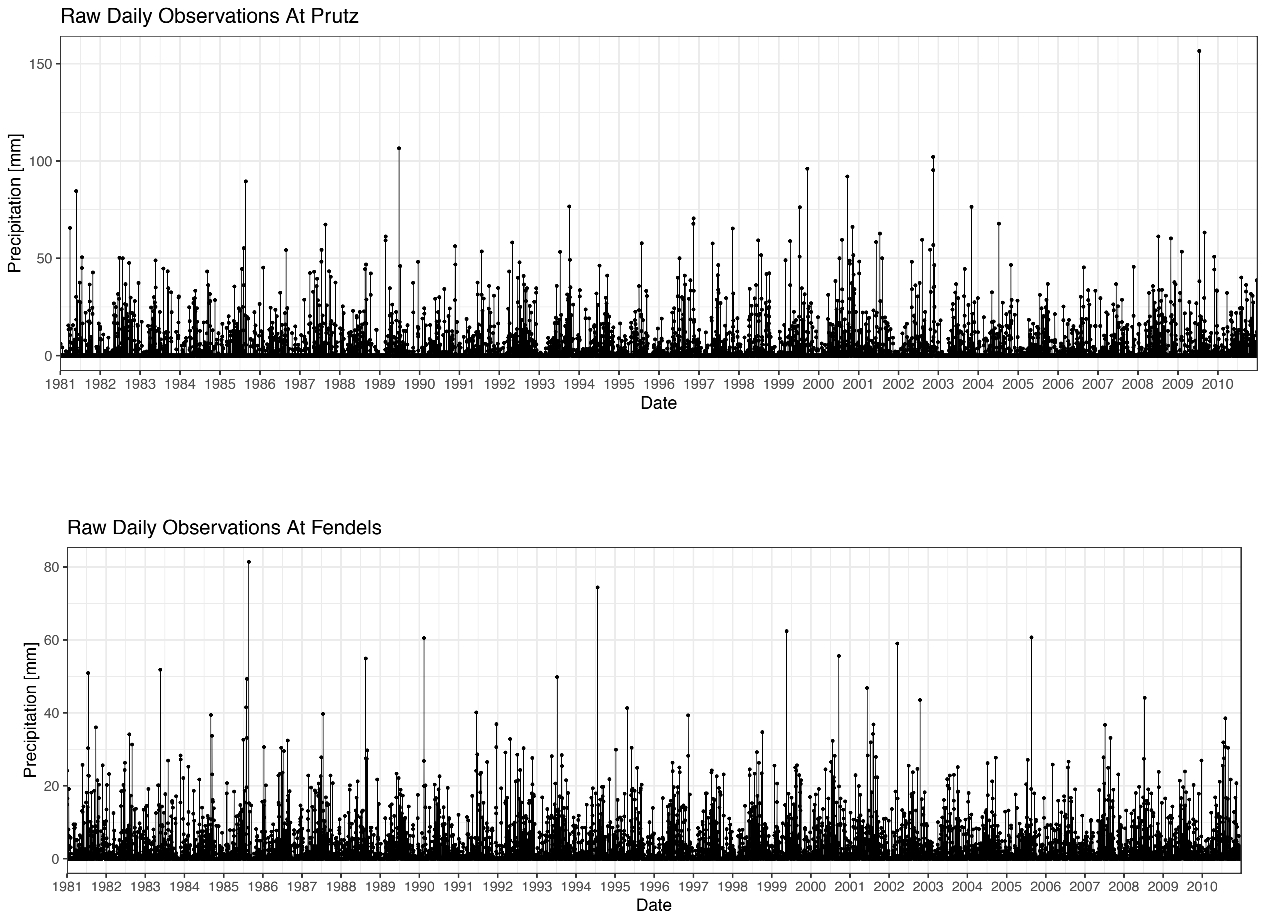

Figure R2: Raw timeseries (1981-2010) of precipitation observations [mm] at Prutz (upper) and Fendels (lower)

4) Your response to my minor comment 10) (**i.e. why using only NAOI as covariate linked to atmospheric circulation) is not satisfactory.**

Response: As pointed out in our original reply, it was a *choice* to consider NAOI as an exemplary variable for this study. Indeed, there are other good options (as suggested by the reviewer in their original review) – and this will be investigated in more detail in the future.

5) Your response to my minor comment 2) is not satisfactory. Most of your covariates for Xo(s,t) are functions of t only (hence constant through space) (e.g., cos(2*pi*t/n), sin(2*pi*t/n), etc.). What I propose is to add a covariate that is function of s only (hence constant through time). Xo(s,t) is indexed in both space and time. I hope that put this way you can see the symmetry of the problem in space and time, and then the possibility of using altitude as a covariate.

Response: We thank the reviewer for elaborating their point. We see the reviewer's point. Considering altitude in the GLM will allow the model to include spatial information but only in vertical direction. However, that would not be sufficient for considering the covariates as a function of space. In our view, along with elevation, there could also be latitude and longitude and if possible other topographic information, e.g. slope or exposition, allowed in GLM.

However, based on our experience and preliminary analysis, we can say that it (i.e. inclusion of elevation) would not add considerable improvement in the results – at least not 'more considerable' than KED. We will consider this suggestion in our future work.

References:

1) *Daly, Chris & Halbleib, M. & Smith, Joseph & Gibson, Wayne & Doggett, Matt & Taylor, George & Curtis, Jan & Pasteris, Phillip. (2008). Physiographically-Sensitive Mapping of Temperature and Precipitation Across the Conterminous United States. International Journal of Climatology. 28. 10.1002/joc.1688.*

2) *Hiebl J., Frei C. (2018): Daily precipitation grids for Austria since 1961—development and evaluation of a spatial dataset for hydro-climatic monitoring and modelling. Theoretical and Applied Climatology 132, 327-345, doi:10.1007/s00704-017-2093-x*

3) *A. Gafurov, J. Götzinger, and A. Bárdossy, Hydrol. Earth Syst. Sci. Discuss., 3, 2209–2242, 2006 www.hydrol-earth-syst-sci-discuss.net/3/2209/2006.*

4) *Zhang, Z., Song, Q., Mechem, D. B., Larson, V. E., Wang, J., Liu, Y., Witte, M. K., Dong, X., and Wu, P.: Vertical dependence of horizontal variation of cloud microphysics: observations from the ACE-ENA field campaign and implications for warm-rain simulation in climate models, Atmos. Chem. Phys., 21, 3103–3121, https://doi.org/10.5194/acp-21-3103-2021, 2021.*