Point-by-point response to Reviewer #3

Florian Ehmele on behalf of the co-author

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Thank you very much for your work and the useful and valuable comments that helped to improve the scientific quality of our manuscript. Please find below our reply to the individual points.

The authors have taken a physically-based, simplified model of orographic precipitation and added mitigations in their approach. The approach has been tested with good results.

It is an interesting and valuable contribution to the literature on this subject. It is thoroughly done and, given the complexity of the approach, it is easy to follow. I would say the results are convincing and robust. Below is a few comments/questions.

Main question: It seems to me that the input parameters are treated independently, section 5.1, in this approach. We know that input parameters such as wind speed and direction are not independent, and thus should not be threated as such. Categorization helps, but still leaves us with the problem mentioned above. If I have understood this correctly, how do you justify independence (picking from pdf's in a random fashion)?

This is a very helpful comment. To address this point, we will add a new section "4.3 Model sensitivities" to the paper with a more detailed sensitivity study of total precipitation to varying initial conditions including discussion in Section 5.1. Our results show different behaviors of the correlations between the input variables. Overall, the relation between the input parameters is weak with correlation coefficients in most cases between +/- 0.3, and only for two parameters of +/- 0.7. After seasonal differentiation, there are significant correlations in only one season. Those cases with higher correlation are mainly related to stability (saturated Brunt-Väisälä frequency N_m^2). As shown in Figures 7 and 9, however, the model is less sensitive to this parameter compared to other. Taking into account the three points mentioned above, we found it acceptable to treat the input variables independently to keep the SPM as simple as possible. We will add a statement on this.

Minor comments/questions:

P7, L12, " linear model assumes penetration through the whole atmosphere...": Does it? it is contrary to what you write below Eq. 6, L24 which I thought was the idea of wave dynamics; reduced penetration with height. Perhaps the over-estimation has something to do with the saturation assumption you mentioned?

This was incorrect as wave dynamics show vertically tilted waves that also decay with height (expect when Fr = U/NH is very large and where the solution more or less resembles the simple upslope approach). Furthermore, you are right that the assumption of saturation over all atmospheric layers, where also the lifting condensation level is at the surface, may lead to an overestimation of modelled precipitation. We will correct/change this in the text.

P9: If c_oro is constant in the whole domain, it could be enter in wave space. Can it be collapsed with f_Cw into a common factor, reducing the number of free parameters?

This might be possible. However, these two parameters affect different physical processes. f_Cw acts to reduce the uplift sensitivity of the model; therefore, it mostly affects areas with strong gradients in orography (compare Figure 2), whereas over less gradients with less orographic lifting the effect is weak. Additionally, multiple ascends/descents are possible without changes in water vapor content of the air parcel. Even though c_oro has the same effect, this parameter is independent from any lifting process and is applied throughout the domain. As mentioned in the text, it is a consequence of the assumption that vertical lifting of the entire column of air leads to condensation and instantaneous fallout of hydrometeors at any time. To deal with the resulting overestimation of available precipitable water, c_oro was implemented. We will change the text to better understand this point.

Fig 15: I believe that the confidence interval should be wider on the upper side than the lower side (due to fewer data points).

Yes, you are right. After checking the data and the routine, we conclude that the used empirical formula from Dyck (1980) is not the proper way. We redid the plots of Figure 15 (old numbering) using the statistical calculation described by Maity (2018) and replaced it in the new manuscript version.

References

Dyck, S., 1980: Angewandte Hydrologie, Teil 1: Berechnung und Regelung des Durchflusses der Flüsse., 2 edn.

Maity, R., 2018: Statistical Methods in Hydrology and Hydroclimatology, Springer Nature Singapore Pte Ltd., https://doi.org/10.1007/978-981-10-8779-0, 2018.