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Hydrology and Earth System Sciences Discussions

Interactive comment on "A new method to separate precipitation phases" *by* Yulian Liu et al.

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Responses to the interactive comments by anonymous Referee #3

I appreciate the efforts made by the authors to share their findings from the study presented in this manuscript. I was particularly intrigued by one of the largest datasets used in this type of studies. Probably most interesting of all, was the title that attracted me to this manuscript. However, I have to admit that after reading the manuscript very carefully, I think there are significant gaps in many respects of this paper in its current form that prevented it from becoming an otherwise a very promising paper with large geographical coverage and data availability. As for the title, I feel hard to agree that it reflect what the paper actually does, nor can I see the novelty in the method applied in the first place.

R: Thanks for the comments. A further revision has been made referring to the comments, on basis of the last revision as suggested by other reviewers. Please note that some of your suggestions were also proposed by the two previous reviewers, and we had already made changes of the manuscript and submitted the revised version and responses before receiving your comments. We hope that this version of manuscript has been further improved.

I share many views of the other reviewers, however, my personal opinion is that substantial re-editing is needed for a re-submission (if re-submission is agreed by the editor responsible), hence no need asking the authors to mend it piecewise. However, I would like the authors to consider the followings when editing/improving the paper.

R: Thanks. We have made changes, and will make further revisions, referring to the comments and suggestions.

1. Title. Unless there is a real novel method developed/improved from old one, I think it is wise not to use the current title. Clearly, relying on a software SPSS and using a standard package to do a step-wise regression fitting, does not warrant a new method. I did want to see if there were any improvement to the method per se to justify the title, but unfortunately none was there. I do feel that the study can make a good case-study, feature-finding paper instead, even use existing method with careful analysis and justification. So my suggestion is to use an alternative, more accurate title to reflect the real work done.

R: This is consistent with the suggestions by two other reviewers. The title has been changed to "A new method for determining the single threshold temperature of precipitation phase separation". This paper proposes a Snowday-Direct-Definition-Mothod (SDDM) to determine the threshold temperature of rainfall and snowfall for a complex and diverse geographical and climatic region in the world.

2. The methodology. I felt particularly uneasy when I saw a method, before being carefully validated and justified, having been used to drive another analysis. As to the paper in its current form, I don't see sufficient evaluation and justification made against

the regression model fitted. It immediately started talking about the analysis of the distribution of the threshold temperature based on the regression model (which has a rather low R-squared value in the first place) without recognising whether those results are reliable or not. My suggestion is to include more convincing analysis against the model instead showing at least the model is acceptable and focus on the uncertain nature. Also even within the category of those simple statistical models, there are plenty of alternatives. Please consider a more extensive view as to the choice of your models.

R: Thanks for the comments. This manuscript applies the Snowday-Direct-Definition-Method (SDDM) to determine the threshold temperature of snow and rain days in a large region where there are recorded weather phenomenon data of rain and snow events, and develops a statistical model with an intention that it could be used in other regions where weather phenomenon data of rain and snow events are unavailable. We will make further revision, with an emphasis given to the validation of the model we developed. For simplicity, the method to develop the model is usually stepwise regression. The physical relationships among the threshold temperature and the climatic variables are explicit. The method has been verified and validated, and it is therefore valid. We will add an additional analysis of the validation and uncertainty of the model in Section Discussion of the revised manuscript. Information on verification of the threshold temperature simulation could also be found in a Chinese paper"Liu YL, Ren GY, Sun XB. 2018. Establishment and Verification of Single Threshold Temperature Model for Partition Precipitation Phase Separation. Journal of Applied Meteorological Science. 29(4):449-459", which has been cited in section Discussion of the revised manuscript.

3. The science. It would be nice to see the results/distribution linking to any scientific findings, or at least, proper scientific explanations associated with the pattern found. This would be far more important in a 'case-study' paper than a 'method-development' one. Please consider in-depth views in this respect.

R: Many thanks. We have added explanations of the results revealed in the analysis

in the revised manuscript. The examples include, but not limited to, the follows. L391: A further explanation of relationship between snow season and snow days, and the possible effects of altitude and relative humidity on the threshold temperature: "This spatial distribution is highly consistent with the spatial distribution of the length of the Chinese snow season and the number of snowfall days (Liu and Ren, 2012). The threshold temperature of the Qinghai-Tibet Plateau with a longer snow season and more snowdays is higher. In the areas with higher altitude and lower relative humidity, the critical temperature is higher, and the critical temperature is lower in the region with lower altitude and higher relative humidity." More explanations were added to the relevant subsections. For example, in "3.3 Correlation between threshold temperature and geographical/climatic factors", we cited Ding (2014) in discussing the altitude influence. Over higher-elevation regions, snow droplets can land faster and thus absorb less heat from the ambient air. So the droplets tend to stay as snow at high elevations and a higher threshold temperature is needed for discriminating snow and rain.

4. Last but not least, the presentation of the current paper is way below the bar. Apparently, editorial helps from native English speakers or a professional service are strongly advised. In addition to many grammar glitches, the paper was structured in a very confusing way and very hard to follow - it looks like that it has been directly translated from another document written in different language or for different purposes. I would suggest to follow a thread of: problem statement -> literature review -> Data and study area -> Methodology and justification -> results discussion/science revelation.

R: The structure of manuscript has been adjusted according to the suggestion. The English has also been polished once again.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-307/hess-2018-307-AC7-supplement.pdf

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