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Discussions

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

Anonymous Referee #3

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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.

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.

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.

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.

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.

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.

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