Hydrol. Earth Syst. Sci. Discuss.
https://doi.org/10.5194/hess-2018-307-RC1, 2018

Reviewer #2

Overview

The authors present a detailed analysis of rain-snow partitioning over northern China using meteorological measurements and precipitation phase observations from several hundred research stations. They show marked spatial variability in the rain-snow air temperature threshold, which is generally highest near the Tibetan Plateau and lowest in the northeast part of the country. They found that the threshold was correlated with latitude, longitude, relative humidity, and precipitation across their study domain. The authors then used those variables, minus longitude and relative humidity, in a stepwise multiple linear regression to predict the rain-snow air temperature threshold. The regression performed well relative to observations.

R: Thanks for the brief review and the following comments.

Major issues

Overall, this work adequately presents spatial variation in rain-snow partitioning using a robust meteorological dataset. However, I would not accept this paper in its current form. First, the paper is titled “A new method to separate precipitation phases.” My contention with this is that the method is a multiple linear regression that cannot be transferred to other regions. The authors use latitude as a predictor variable instead of other meaningful physical quantities that might be transferrable in space. Because of this shortcoming, their method could not be used in other geographic regions (Europe, North America), thus limiting its utility. Froidurot et al. (2014) presents regression methods that use more meaningful independent variables.

R: Thanks for the comments. In our analysis, latitude and longitude are indeed not so good as altitude and relative humidity in indicating the threshold temperature. Latitude is actually representing temperature in a large extent, however, and the threshold temperature would be dependent on thermo-environment or temperature field. It should be a better predictor than say longitude. We rechecked the relationship of the threshold temperature with climatic variables, and we found that annual precipitation days were not as good as annual total precipitation amount. However, annual mean (winter mean) temperature was almost as good as latitude. Thus, we did modify the model to use annual mean temperature rather than latitude, and also revised the section of the manuscript. We believed that the model is robust for use in separating snow and rain on a large scale.

Additionally, there is not much novelty in the work the authors present. For one, spatial variability in rain-snow partitioning has already been described at local (Wayand et al., 2016), regional (Rajagopal and Harpold, 2016), continental (Ye et al., 2013), hemispherical (Jennings et al., 2018), and global (Dai, 2008) scales. In this context, the finding that rain-snow air temperature thresholds vary over large distances is unsurprising. Secondly, the authors relate this variability to relative

humidity and altitude, which has been covered in depth by previous authors. I feel that this work may be considered more of a case study than a significant contribution to hydrologic science. There is novel research that can be done with the datasets the authors have at their disposal. For example, figure 3 presents interesting differences across the regions in rain-snow partitioning and mixed-phase events. However, most of the work presented is not currently suited to the high standards of HESS. I would therefore either recommend major revisions or rejection depending on the opinions of the Associate Editor and other reviewers.
R: We presented a novel single temperature threshold method to objectively partition the snow and rain of precipitation data, based on the latest and highest observational dataset, in a subcontinent characterized by the most complicated topography and climatic condition in the world. We believed that the method had potential to be applicable in other regions outside of mainland China, especially in studies of long-term change in snowfall and extreme snow events in mid- to high latitude.

Throughout

Given HESS's large international audience, I would recommend working with a translating and copyediting service to clean up the English for clarity. I have noted in my specific comments below certain sentences and paragraphs that need particular attention, but the writing needs improvement before resubmission, if resubmission is suggested by the Associate Editor.
R: Thanks for the suggestion. We have invited a copyediting service company to polish the English.

Specific comments

Line 1: I would change the title as multiple linear regression is not a new method and the regression only applies to northern China (i.e., it cannot be used in other geographic regions).
R: Thanks. The title has been changed to "A new method for determining the single threshold temperature of precipitation phase separation". The study area almost includes the whole mainland China with snowfall each year, because south of the Yangtze river usually experiences little or no snowfall.

Lines 45–48: The motivation could be clearer (i.e., change from snow to rain).
R: The introduction has been rewritten, and the motivation of the study has been made clearer than the original version of the manuscript.

Lines 48–52: Break into two sentences. Clearly define the rain-snow temperature threshold. Additionally, the study mentions other types of temperature (dew point and wet bulb), so always note when it is air temperature.
R: Revised already. Dew point and wet bulb temperature data have not been used in the analysis, and the temperature in the text is always the surface air temperature if not mentioned.

Line 49: Based on the rest of the article, I'm assuming that weather phenomenon records are visual observations of precipitation phase as in Ding et al. (2014) and Dai (2008). This line should

be changed to reflect this (along with all other mentions of “weather phenomenon”).
R: Yes. “weather phenomenon records” has been changed to “visual observations of precipitation phase” throughout the text.

Lines 52–53: The first and second clauses are describing the same thing.
R: changed already.

Line 56: Although the en dash is correct here, it might be mistaken for a negative symbol. Perhaps write out the ranges (i.e., -1.2 ℃ to 6.3 ℃).
R: Corrected, and the ranges given also.

Line 57: Do the temperature data go to two significant figures after the decimal point (2.81)? If not, please correct this and all other instances.
R: All temperature values have been changed to one significant figure after the decimal point.

Lines 57–58: What is the “actual threshold?” Use consistent terminology throughout.
R: Corrected, and “actual” deleted.

Line 59: Low or lowest?
R: Thanks. It is lower relatively rather than the lowest.

Line 62: Do you mean more variable instead of dispersed?
R: We mean more dispersed, rather than more variable. Changed already.

Line 64: Remove semi-colon and split into two sentences.
R: Changed.

Lines 66–69: Annual precipitation does not control precipitation phase partitioning. I would assume this was likely an effect of relative humidity based on the other data presented in the work and the research done by other authors.
R: Thanks for the comment. Indeed the relative humidity has a better correlation with the threshold temperature. However, relative humidity data are generally unavailable in most regions outside of China, and thus we chose to use annual total precipitation which has a good positive correlation with relative humidity, and also is more easily available anywhere. We also built a model with relative humidity as predictor, and added the formula for reference in section Discussion.

Lines 74–76: This seems like an important finding, but I’m unsure of what it is. What is a relative deviation of snowfall days? Is that the number of days misidentified as rain when snow was actually occurring? Please use more precise language to clearly convey the findings.
R: Thanks for the comments. We have changed the term of relative deviation to relative error, which is defined as percent ratio of absolute error of modeling value to observational (true) value.

Lines 76–78: You must note that this is only for northern China as the regression would not apply to other areas.

R: The study area almost includes the whole mainland China with snowfall each year, because south of the Yangtze river usually experiences little or no snowfall. This, together with the fact that mainland China is characterized by the most complicated physical geographical and climatic conditions in the world, may make the model developed in this study potentially applicable in other regions. We made a further discussion about the applicability of the method in section Discussion.

Lines 91–94: I'm not sure what this means. Blizzards? Heavy rain events? Freezing rain? Please clarify.

R: Modified as "…as more than 50% of the global meteorological disasters are closely related to abnormal precipitation, including extreme intense rainfall, heavy snowfall or blizzards, freezing rain and droughts…"

Line 95: What is a precipitation condition? Depth?

R: Yes, precipitation amount or depth. Changed to "Under the similar precipitation amount…"

Lines 102–109: What do these studies actually say? Be specific.

R: We have revised this paragraph. A sentence has been added to explain the main findings of the studies related to climate change.

Lines 112–116: Break up into two sentences and rewrite for clarity (i.e., there are few direct observations of precipitation phase at the global scale). Also, researchers can partition precipitation phase, but there are difficulties in doing so at air temperatures near freezing (Ding et al., 2014; Jennings et al., 2018; Stewart et al., 2015).

R: Done. The publications recommended have been cited in the revised manuscript.

Lines 120–134: Please give more information on the studies you cite. The readers need to know the relevant conclusions of the papers, not just what was studied. For example, Harder and Pomeroy (2013) showed the psychrometric energy balance was more effective at predicting precipitation phase than air temperature alone.

R: Thanks for the suggestion. We have revised the section according to the comment, and the unrelated citations have been deleted in the new version of the manuscript.

Line 123: Stefan et al. (2008) should be Kienzle (2008). Please double-check all citations to match HESS's style, which is to give the author's last name (I know this can be tricky as in most western countries the family name comes last, which is not the case in China).

R: Double-check has been done, and the similar problems have been solved.

Lines 132–134: The Jennings et al. (2018) paper showed that including humidity improved the predictive capacity of precipitation phase methods over air temperature alone. This was also shown by other authors (e.g., Ding et al., 2014; Harder and Pomeroy, 2013; Marks et al., 2013).

R: The phrasing has been changed according to the comment.

Lines 135–137: International readers will need more info on this event.
R: The paragraph has been deleted, and the similar contents show more information on studies of snowfall in China.

Lines 135–155: This information would be best combined with the previous paragraph in a discussion of how precipitation phase can be best predicted. There are air temperature methods and then those that use other meteorological and physiographic quantities. The conclusions presented in lines 139–147 are much more specific than any other introductory material and seem out of place.
R: We modified the contents of this part. The more specific information was deleted.

Line 148: What is a discriminant index?
R: This phrase has been changed to "discriminant condition".

Line 150: Dew temperature should be dew point temperature. Correct this throughout paper.
R: Corrected already. This section has been deleted. Thanks.

Lines 156–162: You could add a separate paragraph about precipitation phase methods that use atmospheric (i.e., not just surface) quantities to predict rain and snow. Much of this is covered in detail in two review papers (Feiccabrino et al., 2015; Harpold et al., 2017a).
R: In revision, we have deleted most of the unrelated citations, but explained why we had to use temperature data to make investigation in this work.

Lines 163–164: Be specific on how rain and snow have differing effects on the land surface (e.g., rapid runoff from rainfall versus winter storage and spring release for snowfall).
R: This has been simplified in the revision.

Line 164: "Therefore" is not correct here.
R: It has been deleted.

Lines 164–180: As with my previous comments, this section should be shortened and cleaned up. Here the authors list different ways of partitioning precipitation phase, but the paragraph is introduced as if it is providing different information. Double and single thresholds are covered in depth in the aforementioned review 化 papers (Feiccabrino et al., 2015; Harpold et al., 2017a).
R: These have been simplified and shortened largely in the revision, and the section has been restructured.

Line 177: What is an auxiliary indicator?
R: This sentence has been deleted.

Line 179: Many gridded climate datasets provide humidity information that can be used to estimate dew point temperature with reasonable accuracy.
R: Thanks for the information. Compared temperature and precipitation, relative humidity data or

other humidity data are very difficult to obtain, especially for continents and the global land. The data quality of relative humidity is also problematic. However, we built a model with RH as predictor and included it in section Discussion. By the way, we do not think that the reanalysis data of atmospheric humidity could be used in this kind of researches.

Line 180: There are several mentions of larger-scale analyses and how current precipitation phase methods struggle over broad spatial extents. While this is correct, the method the authors provide in this paper is specific to northern China and can also not be applied to large scales outside of the country.
R: The study area almost includes the whole mainland China with snowfall each year, because south of the Yangtze river usually experiences little or no snowfall. This, together with the fact that mainland China is characterized by the most complicated physical geographical and climatic conditions in the world, may make the model developed in this study potentially applicable in other regions. Of course, we have to do more in future to reach the goal, including the verification of the model in the neighboring regions and selection of more suitable predictors in a larger region.

Lines 181–185: This paragraph provides redundant information. It should be edited and combined with previous information on phase partitioning methods.
R: Thanks. This paragraph has been rewritten in the revision.

Lines 181–182: Again, many authors have shown that humidity improves phase prediction over air temperature only methods.
R: Yes. Our work also shows that. As we discussed before, relative humidity data are not easy to be obtained for other regions, and this would prevent the model from being applied in regions outside of China. Please see the replies above.

Line 182: Arpold should be Harpold and Keith should be changed to Jennings. Please doublecheck all citations.
R: Thanks for pointing them out. These have been corrected.

Lines 186–189: Yes, China does have diverse climatic and physiographic characteristics. No, the method the authors introduce cannot be applied to other areas in the world because it uses latitude, which is not physically meaningful in the context of precipitation phase.
R: Please see the discussion above. Latitude is generally related to temperature, and in most areas also to precipitation, due to the latitudinal distribution of solar radiation and heat on the earth surface. Our analysis also finds that latitude and the threshold temperature had a significant correlation in China. However, we have already changed it to temperature as a predictor in building model referring to this comment, as mentioned before. The results are almost the same.

Line 191: What type of observational data? Be specific.
R: Thanks. Done already.

Lines 193–196: Again, the method can only be applied to northern China where the regression was developed.

R: May be so, and may be not so. We will make more verification in future. Thanks.

Lines 202–204: Many spatially extensive gridded climate and reanalysis products include surface pressure and humidity information.

R: This is a good idea. It is interesting to apply reanalysis data in future work. However, a problem is that reanalysis data are relatively poor in estimating long-term trends of precipitation and extreme precipitation events.

Lines 207–209: Data availability should be provided in a section at the end of the manuscript.

R: OK. We will make it clear once the manuscript is accepted for publication in HESS.

Lines 208–209: As noted above, weather phenomenon should be changed to precipitation phase observations if that is what is included in the dataset. It should be clear what the dataset contains.

R: The term has been changed as suggested.

Lines 211–214: For the quantities besides air temperature, please note whether they are daily averages or totals.

R: Thanks. Clarified. Temperature is daily maximum, minimum and mean temperature, precipitation is the accumulated amount of 24 hour from 0800 to 0800 Beijing Time, and all others are daily means.

Lines 216–218: Were the stations removed or the data removed? It is unclear.

R: The stations were removed from the dataset.

Lines 218–219: Why and how were the latitude and longitude corrected?

R: A few of errors exist in the two datasets. The sentences have been deleted in the revision because it is unnecessary to mention here.

Lines 219–220: Are the meteorological stations in the same location as the precipitation phase observations?

R: Yes. They are at the same stations.

Lines 229–232. Remove and combine the first part of the paragraph with the next paragraph (lines 233–239).

R: Thanks. Simplified, and combined with the next paragraph.

Lines 233–239: Sleet is not technically the same thing as a rain-snow mix. I.e., a rain-snow mix could be sleet but there could also be rain and snow in a day without sleet occurring. Change this terminology to mixed-phase events to be more accurate.

R: Thanks for the suggestion. Accepted, and all “sleets” changed to “mixed-phase events” throughout the text.

Lines 237–239: What is the reasoning behind this?

R: Mixed-phase events occur in a day when there might be rain or snow, or sleets during different hours. The threshold temperature must be lower than the daily maximum temperature and higher than the daily minimum temperature of the mixed-phase events.

Lines 240–251: This paragraph is confusing. Are there 324 stations in the analysis or 623 as previously mentioned? I feel like the authors are trying to stay that only stations with a minimum of 100 snowfall days were analyzed.

R: We analyzed only 324 stations with snowfall in the study area. All these stations had at least 100 days of snowfall during 1981-2010 to guarantee the samples and the representativeness of the statistics.

Line 241: Random is not correct here.

R: It has been changed to “arbitrary cases of snowfall”.

Line 252: What are extreme rain and snow records?

R: It indicates extremely or abnormally large values of rainfall and snowfall at the stations.

Lines 255–257: This is very confusing. Are you saying the mean temperature during sleet was considered to be the rain-snow air temperature threshold?

R: It was taken as only a reference. The threshold temperature value must be between daily maximum and minimum temperature of the mixed-phase events.

Lines 261–270: The choice of representative stations seems arbitrary unless I am missing something. Why not provide summary statistics for the stations in each region? Or, at least provide reasoning for representative station selection. Additionally, the Monsoon Region (I) is compared to the others throughout the paper despite the fact that it has an order of magnitude more stations than II and III. This needs to be addressed. Finally, throughout the paper, please note in which geographic region each representative station is located (i.e., Zhaozhou becomes Zhaozhou-I).

R: The reasoning for selecting the three representative stations has been given. Table 1 gives the information of the three stations. We have also made an explanation of the uneven distribution of the stations. The sparse observations in the Qinghai-Tibetan Plateau will affect the analysis in a large extent.

Lines 272–279: The station colors should be included as a legend.

R: Revised already.

Lines 282–285: I might be misreading this but the “percent deviation of snow days” seems like a poor way of quantifying method success. For example, let’s say Station X has 50 snow days and 50 rain days in a year. I could still get a 0% deviation (100% success rate) if my method predicts 50 days of snow on the rain days and 50 days of rain on the snow days even though my method was completely wrong. Different ways of validating rain-snow methods are provided in previously mentioned literature (Ding et al., 2014; Froidurot et al., 2014; Harpold et al., 2017b; Jennings et

al., 2018). Please correct me if I read this section incorrectly.
R: Relative deviation is not designed to indicate success rate. It shows a relative bias of the prediction from the true value or observation. In the case that you mentioned, the relative deviation will be -100% according to the formula, and that would be the largest bias. Of course, the term may be better to be called "relative error". We have changed it to "relative error" throughout the text.

Lines 286–297: This can be shortened significantly. Stepwise regression is not a new/novel technique.
R: Thanks for the suggestion. We have largely simplified the description of the method in the revised manuscript.

Lines 300–301: What is the snow day mean temperature method? This methods section should be rewritten to provide much more clarity. It should be obvious how the rain-snow air temperature threshold is calculated.
R: "snow day mean temperature method" should be termed as "mean temperature method for snow day". This section has been written in the revision, and the term has been changed to Snowday-Direct-Definition-Mothod (SDDM). Thanks.

Line 312: Figure 2 confused me more than it helped me. This information, when clearly and logically presented, should be easy to understand by reading the methods section.
R: This figure has been removed in our revision.

Lines 324–326: Remove.
R: Thanks. The sentence has been deleted.

Lines 343–346 (Fig. 3): These plots are great (they present a lot of useful information), but could be improved slightly. First, I would flip the axes as it is generally customary to have the cumulative distribution on the y-axis and the measured variable on the x-axis (air temperature in this case). Second, I would normalize the precipitation events so the scale goes from 0-1 (fractional) or 0-100 (percentage). Third, you only need one legend for the whole figure, not one for each subplot.
R: Thanks for the comments. We revised the figure according to the suggestion, as you can see in the revised manuscript.

Lines 350–351: What is the threshold (same comment applies to line 358)?
R: It is the calculated threshold temperature, and we added the values for Zhaozhou and Shiquanhe stations.

Lines 359–370: This paragraph is mostly discussion material and should be moved. Citations should be provided that support the suggestions made by the authors.
R: This paragraph has been deleted in the new version of the manuscript.

Line 372: This table should be flipped so that the stations are listed in the first column and the metrics are in the following columns. I provide an unformatted example below:

	Snow day temperature (°C)	
Station	Maximum	Minimum
Zhaozhou	-0.9	-20.5

R: Already revised according to the suggestion.

Lines 374–377: This may be the case, but how much precipitation is falling at these extreme values?

R: Thanks for the question. It is unclear to address this issue. Further analysis is needed to answer the question based on the method developed in this work.

Lines 378–384: This reads like a figure caption and should be removed or combined with the Fig. 4 caption.

R: It has been simplified already.

Lines 385–386: Use the abbreviation once it is introduced (e.g., Tsm, Trn, etc.).

R: Thanks. Already changed.

Line 395 (Fig. 4): How were the spatial interpolations performed from the point data? The same color ramp should be used for each figure in this case and only one legend is needed. Also, there is no need to include the parts of China that were not analyzed (i.e., limit the plot to what is shown in the red box).

R: Accepted. Ordinary Kriging method was used to interpolate the point data. The figures have been redrawn according to the suggestion.

Lines 403–416: These are methods and should be moved. Additionally, I do not agree that this is the way the rain-snow threshold should be calculated as it seems unnecessarily confusing and arbitrary. The Dai (2008) method would be a preferable easy-to-understand and well-validated way of calculating the threshold.

R: These have been moved to section Data and Methods. This method is good, if not better than that of Dai (2008), and it is a novel and simple procedure.

Lines 417–441: I would remove these paragraphs and figures for three reasons: 1) A different method for calculating the rain-snow threshold should be used (see my comment above); 2) Relative deviation is not the best method for calculating method error (see my comment on lines 282–285); and 3) The findings are not central to the authors' main story.

R: Thanks for the comments. However, we would keep as it was. The reasons are: 1) this method is novel, simple and verifiable; 2) relative deviation, or relative error (as we have revised in the new manuscript), is a good indicator of the method performance. Combined with other indicators, it is able to tell the effectiveness of calculation; 3) the findings are not central, but they are the basis to verify the effectiveness and applicability of the model developed below.

Line 452: West of 90 °E, the threshold decreased from south to north, not from east to west.
R: Yes, thanks. We have modified the sentences: "The threshold temperature west of 100 °E showed an approximately zonal distribution, and the threshold temperature decreased with the increase of latitude; the east of 100 °E had a meridional distribution, and the threshold temperature decreased with increasing longitude."

Lines 458–460: Please note how the spatial interpolations were calculated. There are large spatial extents with no station data. It may be misleading to present the threshold information in this way.
R: Thanks for the comments. This will be somehow more obvious in the Qinghai-Tibetan Plateau and northwestern desert areas. We have made a brief discussion about this in the end of the same paragraph.

Lines 461–465: Remove. The threshold was calculated from the observations, so of course it should reflect them.
R: Accepted. This paragraph has been removed.

Lines 467–472: Remove. This is introductory material and the regression cannot be used outside China as it uses latitude as a predictor variable.
R: Thanks. This has been removed.

Lines 473–477: Combine with following paragraph and remove redundancies.
R: We have largely simplified the sentences according to the suggestion.

Line 480: Change disperse to variable.
R: Done. Thanks.

Line 482: Change centralized to less variable.
R: Changed.

Lines 490–492 (Fig. 7): Add legend for figure colors. Latitude needs a degree symbol. Did you check how the regression in c was affected by the extreme precipitation outlier? Why was longitude not plotted? This was given as one of the significant correlates of the threshold temperature.
R: The figure already modified referring to the suggestion. The extreme precipitation outlier would have an effect, but the effect is small. The new figure no longer contains both latitude and longitude, because latitude has not been used in building model.

Lines 495–515: Remove. This is discussion material. Citations are needed if this material is kept.
R: This explanation would be somehow useful to readers. However, we endure pains to have deleted it, referring to the suggestion by the reviewer.

Line 517: Again, I do not agree that humidity is difficult to obtain. Yes, it is less common than air temperature and precipitation, but it is available from many meteorological stations and gridded climate products. Additionally, figure 7 shows a very weak relationship between precipitation and the threshold, especially considering the extreme outlier I noted above.

R: Observed relative humidity data are less available in most regions outside of China. Reanalysis data are more easily available, but they have some intrinsic problems. One problem would be the bad usefulness in showing long-term trend over the last decades. The method we are to develop in this work was intended to be used in separating rain and snow in a large scale in forming a long-term daily snowfall dataset. This is the main reason why we did not choose to use reanalysis data in developing the method.

Line 520: As I have mentioned throughout, the use of latitude is a large limitation of this method as it means it can only be applied to northern China.

R: Thanks for the comments once again. Please see the discussion above. Latitude is generally related to temperature, and in most areas also to precipitation, due to the latitudinal distribution of solar radiation and heat on the earth surface. Our analysis also found that latitude and the threshold temperature had a significant correlation in China. However, we have revised the model by including annual mean temperature rather than latitude as a predictor.

Lines 526–527: The coefficient of variation (r2), root mean squared error, and mean bias would all be more appropriate error metrics to provide. Additionally, were any of the stations removed from the data when computing the regression? Error statistics cannot be calculated reliably if all stations were used. Ideally, the model output should be cross-validated on stations that were removed before calculating the regression coefficients.

R: A good suggestion. Thanks a lot. We have added a table and the corresponding explanations in this section of the new manuscript, with relative error being given more attention but also other metrics mentioned.

Lines 534–535: This is confusingly written. Please rewrite for clarity.

R: Thanks. The sentence has been changed.

Lines 541–546 (Fig. 8): What is the x-axis on these figures? The region lines should not be the same color as the data.

R: Modified. X-axis is stations of the three regions.

Lines 547–549: Again, please use more robust error metrics. In this case, the mean bias would be helpful as it would indicate whether snowfall was being over or underpredicted. (It looks like this is shown in the following lines and Fig. 9, so please make this information more clear to the reader.)

R: We used more error metrics to indicate the error in the revised manuscript. Thanks for the suggestion. The mean absolute errors of threshold temperature of the simulated snowfall are 0.476℃ for region one, 0.560℃ for region 2, and 0.435℃ for region 3, for example. We have also added a figure to show the spatial distributions of the absolute errors, and a table to show the mean errors of other metrics for the whole study area and the three sub-regions.

Lines 565–568 (Fig. 9): If this is the mean bias of snowfall days and snowfall, please make that clear.

R: This is for relative errors, rather than for absolute errors. However, we have already added analysis of absolute errors, including a new figure showing the distributions of the absolute errors in the study area.

Lines 569–574: I do not understand what this paragraph is trying to say and how it relates to the previous information.
R: We have revised the paragraph to include the illustration of other errors except for relative errors. It is intended to explain the reason why the relative errors in the study area are so larger as compared to other regions at the same latitudes of the world.

Lines 575–586: Rewrite for clarity. I think the main point of this paragraph is that the method underpredicts snow in high threshold areas and overpredicts in low threshold areas, but I am not certain.
R: We have rewritten the paragraph. The revised paragraph includes more illustrations of the absolute errors, relative errors, correlation coefficients, and RMSE of the calculation method, so that readers could get a more systematic picture of the performance of the model.

Line 594–641: There are many, many methods for predicting precipitation phase. I am unclear as to why the authors devote nearly 50 lines for comparing the method to one other (there is too much space given here to the Han method). If the authors wish to include this information in their results, they should include other methods and provide a robust comparison. As it stands, I would remove this section or shorten it and add to discussion along with other phase method comparison papers. Particularly relevant is Ding et al. (2014), who also used China Meteorological Administration data in their work.
R: Thanks for the comments. This subsection has been moved to section Discussion, and we also added the comparison with that of Ding et al. (2014).

Lines 654–691: This is more of a summary of the paper than a discussion and should be rewritten. There were parts of the results section that should be moved here (noted in my previous comments). Additionally, the discussion should clearly note major limitations and assumptions, which this does not except for the last paragraph. Finally, the authors should compare their work to that of other researchers. There is a lot of literature on the subject of rainsnow partitioning, none of which is discussed here.
R: According to the suggestion, this section has been enhanced largely, and the comparisons with other methods have been moved here.

Lines 708–710: Remove.
R: Removed already.

Lines 711–713: To reiterate, latitude is not a physically meaningful quantity in terms of precipitation phase partitioning. Figure 6 shows that at ~30 °N the threshold temperature decreases from 5 °C in the west to 0 °C in the east. This pattern appears fairly consistently in the data as one moves northward with the only exception being northwest China, where the threshold is low. The authors are likely seeing the effect of altitude and humidity, which is, in some cases, cross-correlated with latitude. The use of latitude throughout this paper is a major weakness that must be addressed.

R: Thanks for the comments. As discussed before, latitude is representing temperature in a large extent, and the threshold temperature would be dependent on thermo-environment or temperature field. However, we found the correlations of the threshold temperature with annual mean (winter mean) temperature was as good as latitude. Thus, we have modified the model, and also revised the section of the manuscript.

Lines 713–714: Rewrite to remove redundancies.

R: This has been revised.

Line 715: Change good to statistically significant.

R: Changed already.

Lines 716–717: Change specially to especially. Additionally, much previous work has shown how relative humidity improves phase partitioning. This is another weakness of the paper that the authors show a strong relationship between relative humidity and the rain-snow temperature threshold, but do not include in their regression (I have noted in previous comments that although relative humidity is less available than air temperature and precipitation, it can still be widely found in ground observations and gridded climate products).

R: Thanks. We discussed about this before, and would you please refer to the above replies to the interesting question and comments.

Review references

Dai, A., 2008. Temperature and pressure dependence of the rain-snow phase transition over land and ocean. Geophys. Res. Lett. 35.

Ding, B., Yang, K., Qin, J., Wang, L., Chen, Y., He, X., 2014. The dependence of precipitation types on surface elevation and meteorological conditions and its parameterization. J. Hydrol. 513, 154–163.

Feiccabrino, J., Graff, W., Lundberg, A., Sandström, N., Gustafsson, D., 2015. Meteorological Knowledge Useful for the Improvement of Snow Rain Separation in Surface Based Models. Hydrology 2, 266–288. https:// doi.org/10.3390/hydrology2040266

Froidurot, S., Zin, I., Hingray, B., Gautheron, A., 2014. Sensitivity of Precipitation Phase over the Swiss Alps to Different Meteorological Variables. J. Hydrometeorol. 15, 685–696. https://doi.org/10.1175/JHM-D-13-073.1

Harder, P., Pomeroy, J., 2013. Estimating precipitation phase using a psychrometric energy balance method. Hydrol. Process. 27, 1901–1914. https://doi.org/10.1002/hyp.9799

Harpold, A.A., Kaplan, M., Klos, P.Z., Link, T., McNamara, J.P., Rajagopal, S., Schumer, R., Steele, C.M., 2017a. Rain or snow: hydrologic processes, observations, prediction, and research needs. Hydrol Earth Syst Sci 21, 1–22.

Harpold, A.A., Crews, J.B., Rajagopal, S., Winchell, T., Schumer, R., 2017b. Relative Humidity Has Uneven Effects on Shifts From Snow to Rain Over the Western U.S. Geophys. Res. Lett. 44, 2017GL075046. https://doi.org/ 10.1002/2017GL075046

Jennings, K.S., Winchell, T.S., Livneh, B., Molotch, N.P., 2018. Spatial variation of the rainsnow temperature threshold across the Northern Hemisphere. Nat. Commun. 9. https://doi.org/10.1038/s41467-018-03629-7

Kienzle, S.W., 2008. A new temperature based method to separate rain and snow. Hydrol. Process. 22, 5067–5085. https://doi.org/10.1002/hyp.7131

Marks, D., Winstral, A., Reba, M., Pomeroy, J., Kumar, M., 2013. An evaluation of methods for determining during-storm precipitation phase and the rain/snow transition elevation at the surface in a mountain basin. Adv. Water Resour. 55, 98–110. https://doi.org/10.1016/j.advwatres.2012.11.012

Rajagopal, S., Harpold, A.A., 2016. Testing and Improving Temperature Thresholds for Snow and Rain Prediction in the Western United States. JAWRA J. Am. Water Resour. Assoc.

Stewart, R.E., Thériault, J.M., Henson, W., 2015. On the Characteristics of and Processes Producing Winter Precipitation Types near 0 ℃. Bull. Am. Meteorol. Soc. 96, 623–639. https://doi.org/10.1175/BAMS-D-14-00032.1

Wayand, N.E., Stimberis, J., Zagrodnik, J.P., Mass, C.F., Lundquist, J.D., 2016. Improving simulations of precipitation phase and snowpack at a site subject to cold air intrusions: Snoqualmie Pass, WA. J. Geophys. Res. Atmospheres 121, 9929–9942.

Ye, H., Cohen, J., Rawlins, M., 2013. Discrimination of Solid from Liquid Precipitation over Northern Eurasia Using Surface Atmospheric Conditions*. J. Hydrometeorol. 14, 1345–1355.