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.

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.

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.

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.

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).

Lines 45–48: The motivation could be clearer (i.e., change from snow to rain).

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.

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").

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

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°C to 6.3°C).

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.

Lines 57–58: What is the "actual threshold?" Use consistent terminology throughout.

Line 59: Low or lowest?

Line 62: Do you mean more variable instead of dispersed?

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

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.

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.

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

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

Line 95: What is a precipitation condition? Depth?

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

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).

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.

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).

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).

Lines 135–137: International readers will need more info on this event.

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.

Line 148: What is a discriminant index?

Line 150: Dew temperature should be dew point temperature. Correct this throughout paper.

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).

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).

Line 164: "Therefore" is not correct here.

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 papers (Feiccabrino et al., 2015; Harpold et al., 2017a).

Line 177: What is an auxiliary indicator?

Line 179: Many gridded climate datasets provide humidity information that can be used to estimate dew point temperature with reasonable accuracy.

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.

Lines 181–185: This paragraph provides redundant information. It should be edited and combined with previous information on phase partitioning methods.

Lines 181–182: Again, many authors have shown that humidity improves phase prediction over air temperature only methods.

Line 182: Arpold should be Harpold and Keith should be changed to Jennings. Please double-check all citations.

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.

Line 191: What type of observational data? Be specific.

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

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

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

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.

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

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

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

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

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

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.

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

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.

Line 241: Random is not correct here.

Line 252: What are extreme rain and snow records?

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?

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).

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

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.

Lines 286–297: This can be shortened significantly. Stepwise regression is not a new/novel technique.

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.

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.

Lines 324–326: Remove.

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.

Lines 350–351: What is the threshold (same comment applies to line 358)?

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.

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

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

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

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

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).

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.

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.

Line 452: West of 90°E, the threshold decreased from south to north, not from east to west.

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.

Lines 461–465: Remove. The threshold was calculated from the observations, so of course it should reflect them.

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

Lines 473–477: Combine with following paragraph and remove redundancies.

Line 480: Change disperse to variable.

Line 482: Change centralized to less variable.

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.

Lines 495–515: Remove. This is discussion material. Citations are needed if this material is kept.

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.

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.

Lines 526–527: The coefficient of variation (r^2), 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.

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

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.

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.)

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

Lines 569–574: I do not understand what this paragraph is trying to say and how it relates to the previous information.

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.

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.

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 rain-snow partitioning, none of which is discussed here.

Lines 708–710: Remove.

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.

Lines 713–714: Rewrite to remove redundancies.

Line 715: Change good to statistically significant.

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).

Review references

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