Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
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Comment on hess-2022-103

Anonymous Referee #1

Referee comment on "Characterizing basin-scale precipitation gradients in the Third Pole region and associated determinants" by Yaozhi Jiang et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-103-RC1, 2022

This study applies three datasets, ERA5_CNN, IMERG, and HAR to study precipitation gradients in the Third Pole region. Instead of the absolute precipitation gradient, this study uses the relative precipitation gradient (RPG) throughout the manuscript. However, I think the RPG is not an appropriate index which can cause misleading and even wrong results. This problem makes the study questionable. My comments are as below.

1. RPG problems

The definition of relative precipitation gradient (RPG) in Equation (2) is problematic from my perspective. There is no obvious linkage between precipitation intensity and precipitation gradient because precipitation gradient is just a spatial pattern no matter how large the mean value is (e.g.,1 0 or 1000). The authors state that RPG is used because ERA5_CNN has systematic bias in the TP. The most common systematic bias is overestimation or underestimation. If we assume the spatial distribution of overestimation or underestimation is even (cases with uneven distributions are more complex), the precipitation gradient has nothing to with the magnitude of precipitation, which means the normalization operation in Equation (2) is meaningless. Actually, the normalization could be harmful. For example, Let's assume a is 0.2 mm/m and P is 2 mm/day according to ERA5_CNN. Then, RPG=a/P=0.2/2 = 0.1. If we have another dataset which could have similar spatial distribution with ERA5_CNN but an overestimation bias. Let's say its P=4 mm/day. According to results from this study, the gradient for this new dataset will be 0.1 X 4 =0.4, which is incorrect. I can imagine that this simple example could be quite common in applications motivated by this work.

The RPG problem could also result in wrong comparison between different products. For example, when comparing ERA5_CNN and gauge data in a basin, RPG cannot provide useful information. Even the RPG values of the two products are the same, their absolute

precipitation gradients are often different because their mean precipitation is different. Similarly, even the RPG of a product is biased, its absolute precipitation gradient could be correct. Therefore, the use of RPG does not make sense.

Besides, I recommend the authors also present absolute precipitation gradient. RPG maps cannot show the impact of atmosphere vapor. For example, large RPG may happen in dry basins, but its significance could be weaker compared to relatively smaller RPG in wet basins where precipitation amounts and bias are larger.

2. Atmospheric model still has large bias in complex terrain including the study area which is also acknowledged in this study. This problem needs more discussion.

3. The discussion between the relationship between precipitation and wind speed can be further improved. They are both aspects of air mass movement which is affected by both atmosphere and topography. If you try to investigate the causality (e.g., some sentences in abstract and discussion parts), you should go further. High or low wind speed is also the result of various environmental factors.

4. Section 2.1: what's the time period of the 1/30-degree WRF simulations? Since ERA5_CNN is taken from Jiang et al. (2021), it is recommended to introduce more method and evaluation details in the manuscript.

5. Section 2.2 introduces IMERG and HAR V2 which should be in "Section 2.1 Precipitation Datasets".

6. Equation (1): units of P and H are needed.

7. Line 145-149: This description has a logical problem. It is stated that "the precipitation gradient is estimated only when the following three principles are met" and the third principle is "(3) the p-value of the Student's t-test for the regression equation should be less than 0.05". But you cannot know p-value without estimating precipitation gradient. Please revise this sentence.

8. Figure 2: I suggest adding rain gauge number of different elevation bands.

9. Is Tibetan Plateau a more suitable word compared to "Third Pole"? Tibetan Plateau is clearer as a geographical concept.

10. The writing and presentation of the manuscript can be further improved.

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Comment on hess-2022-103

Anonymous Referee #2

Referee comment on "Characterizing basin-scale precipitation gradients in the Third Pole region and associated determinants" by Yaozhi Jiang et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-103-RC3, 2022

This study throws up an interesting topic on the precipitation gradients in the Third Pole. However, the presentation of the manuscript is rather rough. Some conclusions drawn from RPGs seem to be unreasonable. The manuscript needs to be further improved. My comments are shown as follows:

- The conclusions in Figure 2 are subjective. It is difficult to conclude that ERA5_CNN is better than the other two products. In Figure 2(b) and (e), the conclusion that ERA5_CNN is the most consistent with rain gauge data is clear. However, in the other sub-basins, the conclusion is not obvious. An indicator to describe the goodness of ERA5_CNN may help.
- Are the sub-basins used in this study reasonable? It has been mentioned in the manuscript that the precipitation decreases with altitude above 2500 m. In a sub-basin, the altitude can change from below 2500m to above 2500m. As a result, the precipitation gradients in a sub-basin are not consistent. It may need more discussion on the basin-scale precipitation gradients.
- As the numbers of gird cells in different sub-basins are different, the same values of R in different sub-basins have a different mean. For instance, R with the value of 0.5 may mean a weak correlation in a 10-grid-cell sub-basin but a strong correlation in a 100-grid-cell sub-basin. Significance tests are necessary to show the strong correlations between precipitation and altitudes.
- In Section 4.3.1, more evidence is needed to support that strong seasonal variation exists in RPGs. The RPG is a value that the absolute precipitation gradient divided by the basin mean precipitation. The RPG will show a strong seasonal variation even if the absolute precipitation gradient has not changed. The strong seasonal variation in RPG exists but may not have any meaning.
- Why do the authors use the average RPGs of the five sub-regions to study the interannual variations? The interannual variations of RPGs in some sub-basins may be covered. It does not make sense to average RPGs of the sub-basins to represent the RPG of a sub-region.
- Where are the CV of annual RPGs for the sub-regions? The results should be shown in the manuscript. As RPG is a percentage, it is necessary to clarify the unit of CV. With

the value of CV less than 0.12, it does not account for the conclusion that RPGs change little between different years. For example, the maximum and minimum values of RPGs in Qaidam are ~9% and ~13% respectively. Considering the range of RPGs, the change is not little. Moreover, it can be seen that there is a periodic variation in RPGs in Figure 5.

- The trend tests are not found in the manuscript. How to draw a conclusion that there is no significant trend in RPGs in all the sub-regions?
- Because of the equation RPG=a/P and the positive correlation between P and RH, there is an inverse proportional relationship, rather than a linear relationship between RPG and RH. This analysis in Figure (a)-(e) does not make sense.