

Interactive comment on “A climatological interpretation of precipitation-based $\delta^{18}\text{O}$ across Siberia and Central Asia” by Tao Wang et al.

Anonymous Referee #1

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This manuscript used GNIP station observations and several indices as well as re-analysis data to describe the correlation between precipitation stable isotopes and temperature, precipitation amount, moisture back-trajectories and several indices, and try to conclude the temperature effect and moisture source effect on precipitation stable isotopes.

The critical defect of this study is that the necessity of this study is not well elaborated. Honestly, it seems that the author found these data and analyzed purposeless, so the data analysis cannot provide enough evidences for the conclusion. Many similar studied have been done decades ago, and it is hard to see any advances in this study. The text is not fairly organized, is lack of systematical discussion and full of hypothesis (e.g. P3, L50-51; P10, L215-216) given the available data. Authors need to present

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their work with better clarity. The authors speculate unwarranted conclusion based on correlation analysis.

The number of stations should be explained more clearly. 13? 14? Or 20? Why the different number of stations are used for analysis? Some important and necessary information are missing, for instance, the advance of the study beyond previous studies, the different background of stations. . . The “trend” and “pattern” are not definition acceptably for the seasonal and annual variations.

The relationships between isotopes and NAO, EZCI, ZI are described in the text, but the significances and relevance of such analysis are not clear in the text.

The explanation of weak temperature effect in summer is wrong in L239-243.

More analysis should be offered to provide the related variations of isotopes with the shifts of westerlies.

The meaning of amount effect analysis should be clarified. If it related with the moisture source? Or shifts of westerlies? What is the authors' point on this analysis?

The description in L415-416 is wrong, opposite with the westerly transport.

The English should be improved by the native speaker.

By considering these issues, I do not think this manuscript is good enough to be published in any high-quality journals. I would recommend the editor reject the manuscript at present and encourage the author to re-submit after improvement.

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2019-7>, 2019.

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