

Interactive comment on “Long-term regional precipitation disparity in northwestern China and its driving forces” by H. F. Lee et al.

H. F. Lee et al.

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First of all, thank you for confirming that our paper provides a valuable insight in revealing regional precipitation difference. Our responses to the comment are listed below:

Regarding Point 1, which concerns about the content of our Introduction, we think that not all readers know very well about our study area (i.e., NW China). Therefore, we provide some background information of NW China in our Introduction section. By doing so, it may help readers to comprehend the context of our study area (i.e., the socio-economic impacts of drought). This may help to reveal the importance of our study.

Regarding Point 2, which concerns about our data and methodology, our data descrip-

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tion and the method of composing regional precipitation index have been stated in p. 3101–3102. Besides, the sites covered and their spatial weightings have been listed in Tables S1 and S2 in our Supplement. It is true that missing data is an issue for historical records (in fact, missing data in early period is also an issue pertinent to all proxy records). So far, the flood and drought dataset we employed is regarded as the most accurate and comprehensive one. The dataset has also been repeatedly verified in our previous studies. Besides, we have conducted a robust test for our precipitation indices (see Robustness Test in our Supplement). As indicated by our statistical test results, no significant dating errors are found. This also implies that our precipitation indices are not affected much by missing data. When we revise our paper, we will further emphasize this point to address readers' possible concern of missing data.

Regarding Point 3, which concerns about our study area, the delineation of Sha'anxi and GNQR has been shown in Figure 1 and our Supplement (Tables S1 and S2). Ningxia is part of GNQR. Helanshan Mountain Range, which locates in Ningxia as mentioned by the reviewer, has been included in our study area already.

Regarding Point 4, which concerns about the meaning of “reconstruction,” we may need to seek advice from the handling editor and see whether we should use another term.

Regarding Point 5, which concerns about the statistical test of our RPD index, we agree that this is an issue we need to address. When we revise our manuscript, we will use bootstrapping method to calculate the confidence interval of our RPD index.

Regarding Point 6, which concerns about the content of our discussion and also the originality of our research, we need to make it clear here. Yes, there are studies mentioning about the influence of Asian Monsoon and ENSO on Asian climate. However, most of those studies are based on instrumental meteorological records (<50 years) to figure out the short-term (say inter-annual to decadal influence) of Asian Monsoon and ENSO on Asian Climate. As climate dynamics varies across different time-scales, we

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cannot assume those short-term dynamics to be identical with the long-term (say multi-decadal to centennial) ones. In our study, we examine the long-term (multi-decadal to centennial) regional climatic variability in NW China over extended period (AD580–1979, 1400 years in length), and also quantitatively identify which factors are more important in driving the variability. We have cross-checked our results with those of other studies, making sure that our study does have original contribution to knowledge as stated in our discussion section.

Regarding Point 7, which concerns about the application of wavelet analysis, we would like to say that apart from wavelet coherence, we have also calculated the phase difference and the distribution of the phase difference (see Figs. 4 & 5). But, we find that there is not any significant phase lag in our wavelet coherence results (i.e., the distribution of phase difference is centered at 0). Put it in another way, the associated periodicities are almost synchronous. Therefore, the phase lag is not emphasized in our text. But, we will further highlight this point when we revise our manuscript. Regarding the suggestion of separating various frequency components for comparison, in fact, we have demonstrated and compared various significant periodicities/ year bands in our results already (see Section 3).

Regarding Point 8, which concerns about the presentation of our figures, the dashed line in Fig. 1 is the north limit of Asian Summer Monsoon (which consists of both East Asian Summer Monsoon and Indian Summer Monsoon), not for East Asian Summer Monsoon only. We will take the reviewer's advice to use a belt instead of a line to show the north limit of Asian Summer Monsoon. Besides, we will add color legends in our figures as suggested. We agree with the reviewer that the size of some of our panel figures should be enlarged. We will work with the editorial office closely to further improve the presentation of our figures.

Interactive comment on *Clim. Past Discuss.*, 10, 3097, 2014.

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