

Interactive comment on “Precipitation reconstruction based on tree-ring width over the past 270 years in the central Lesser Khingan Mountains, Northeast China” by Mingqi Li et al.

Anonymous Referee #2

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General Comment: Based on nearly 100 tree cores of Korean pines and climate data between 1956 and 2017, the study reconstructed 270-year precipitation for the north-eastern region of China. The manuscript is not very well prepared. Some necessary technical details are missing (e.g., the specifics of the detrending functions, characteristics and the spatial resolution of the APVI), whereas parts of the manuscript are repetitive. Since the intent of the manuscript is to understand soil moisture conditions (drought and wet periods), I recommend that the authors should consider directly reconstructing a drought index based on precipitation data alone, e.g., SPI or SPEI (based on simply Tmax and latitude). A drought index may better serve the purpose of the study. The two indices can be easily derived as the authors already have all the

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necessary information.

Specific comments:

1. From Fig. 2, I suspect that the monthly precipitation was only the amount of monthly rainfall. Please clarify whether the precipitation data include the amount of snowfall.
2. For spectral analysis, I would recommend that the authors should consider using multitaper spectral analysis against a red noise background. What does the blue line in Fig. 8A represent? 95% significance level? Also, without information about what different colors represent (power or statistical significance), Fig. 8B by itself is meaningless.
3. The statistics in Table 3 suggest that the regression functions in the 1956-1986 and 1987-2017 periods may have different slopes (temporally unstable). Fig. 5B suggests this may be true. The authors should test the homogeneity of regression slopes to see whether that is the case. I would also like to see a simple sentence states that the residuals of all three regressions met the regular assumptions, given that the residuals indeed met the assumptions.
4. The authors should explain what the black solid and dotted lines represent in Fig. 5C. They should also explain how they derived their 95% CI.
5. The correlations between the reconstructed precipitation and the reconstructed NAO index they used are quite low and statistically not significant. I fail to see the main point of their arguments. Also, please plot the running correlation (e.g., using a 40-year window perhaps) over the common period between the two, so we can see how the correlation evolved. Furthermore, please provide a plausible explanation as to how NAO and the study area's precipitation are correlated.
6. The authors examined the relationships of observed (and reconstructed) precipitation and several large-scale atmospheric-oceanic circulations. After reading the manuscript, however, I still do not have a clear picture of what natural climate drivers

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are important to the study area's soil moisture conditions. Another index that the authors may want to look at is the East Asian Winter Monsoon index (use the one developed by Wu and Wang, 2014, An intensity index for the East Asian winter monsoon. *J. Clim.* 27, 2361–2374.).

Technical comment: Please read through the manuscript again. Remove repetitive information and re-structure the manuscript.

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