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Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "Temperatures and precipitation totals over the Russian Far East and Eastern Siberia: long-term variability and its links to teleconnection indices" *by* V. V. Krokhin and W. M. J. Luxemburg

**Anonymous Referee #2**

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General Comments:

The conclusion "We found that interannual variability in Eastern Siberian and Far Eastern time series of temperature and precipitation anomalies can be represented by the single or two dominant complex principal modes" is incorrect and inconsistent with previous work. The statement may be true for the filtered anomaly series input into the rotated complex PCA, but not otherwise. This is because several methodological mistakes have been made in the preparation of the anomaly time series for analysis. These errors make the results difficult to interpret, unrepresentative of the actual inter-

annual variability and of questionable value. The paper also has a number of highly debatable choices, interpretations and assertions. Lastly, apparent weak links to the North Pacific and the Arctic Oscillation for the region are already well established, but represented as much stronger in other correctly-performed analyses.

Specific Comments:

1. Errors in formation of anomaly time series:

a. Failure to respect the enormous differences in the character of interannual variability from one season to the next, i.e. mixing of inhomogeneous, different climates: Results will be compromises of multiple regimes preferentially occurring at different times of the year but will be biased towards the season (certainly winter for temperature series) with the strongest interannual variability. There will be further distortion from the application of the Butterworth filter which will mistake year to year variability in different seasons as inter-seasonal variability and smooth it. Analyses of interannual variability can only correctly be conducted by treating each season separately.

b. Inappropriate arbitrary removal of a linear trend from the full 1949-2003 anomaly series: This is always incorrect unless there is an a priori physical reason to expect such a trend. In fact it is more likely that any non-local trends in the temperature data are related to global change, which is decidedly non-linear in at least Eurasian winter temperatures, with the increases dominantly after the 1970s. Thus removal of a linear trend in this instance will at best remove only part of the global change signal and at worst introduce artificial decadal to multi-decadal variability (which we see in Fig. 5a). PCA can accomodate trend signals so this step was unnecessary regardless of whether it was justified or not.

c. Failure to areally-weight the input anomaly time series: Fully one-fourth of the stations are clustered in less than 1/20th of the domain, one-half of them in only one-fourth of the domain. Consequently the PCA results will be strongly biased by these smaller subdomains. The results (aside from the other debilitating problems described in a.



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and b.) are probably only representative of the Russian Far East and not of Eastern Siberia. This is obviously true from inspection of Figs. 2a and 3a, where the loadings are dominated by the southeastern stations.

2. Other questions about methodology:

a. In the absence of convincing evidence for strong traveling (vs. standing) variability on interannual time scales I am bewildered by the choice of complex PCA for this study.

b. As noted above the Butterworth Filter probably made a bad situation worse.

C. Rotation is desireable and often necessary in these kinds of analyses, but the results can be seriously compromised if either too many or (but especially) too few modes are rotated. The choice to just rotate two complex modes has not been justified and is especially suspect for the precipitation analysis. This could be a further source of distortion of the results from anything resembling actual reality. At the minimum the explained variances for the first four or five modes should have been tabulated.

3. Comments regarding interpretation:

a. Fig. 3a represents standing, not traveling, variability. The loadings are inconsequential outside of the southeastern cluster of stations dominating the analysis (see above) where there is little difference from location to location in phase. The mode is likely representing interannual variability in one season.

b. The time series of the modes should have been displayed.

c. The coherences in Figs. 5b, 6b, and 7b may be statistically significant, but they are not practically significant. Others, using far more direct, simple approaches (like composites means or distributions from samples keyed on index thresholds) have found strong associations between the focus region's temperature and precipitation in particular seasons and the AO and NP modes. The analysis here has mixed, blurred, and distorted them to insignificance.



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4. Questionable remarks:

a. p. 1916; " ... the study of observed climate variability may be summarized as a climate model verification problem." I disagree.

b. p. 1916; "The most widespread linear correlation techniques are unable to clearly recognize the climatic signal in the short and noisy data series." PCA is a "widespread linear correlation technique" in many of its incarnations.

c. p. 1917; "At present the tandem 'spectral analysis + principal component analysis' is more preferable than other methods." I disagree.

d. p. 1918; "... we approximated the East Asian monsoon activity via circulation teleconnection indices, i.e. the North-Pacific (NP) index and Arctic Oscillation (AO) index." This statement implies both cause/effect and completeness, neither of which are justified.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1915, 2006.