Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-679-RC3, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Adding value to Extended-range Forecasts in Northern Europe by Statistical Post-processing Using Stratospheric Observations" *by* Natalia Korhonen et al.

Anonymous Referee #3

Received and published: 19 September 2019

This paper describes a post-processing method to improve sub-seasonal forecasts of northern European winter temperatures, based on the state of the stratospheric polar vortex and QBO in the period immediately preceding the forecast. The paper is interesting, topical and clearly explained, but I have some reservations about the method that need to be addressed before the paper is suitable for publication.

Major comments

1. I'm not convinced about the inclusion of the QBO as a predictor of the Arctic Oscillation (AO) in the method. The authors note that the westerly / easterly QBO is associated with a stronger / weaker polar vortex, but the polar vortex is already included

Printer-friendly version

Discussion paper

as the other predictor. Unless the QBO directly influences the AO independently of the polar vortex, it's hard to see how the QBO can provide additional skill in forecasting the AO.

The authors state that the results are more significant when both the polar vortex and the QBO are used as predictors of the AO, presumably referring to the results in figure 2. The method partitions the 144 observed winter months (for NDJF, 1981-2016) into two sets based on a stratospheric precursor criterion (eg the polar vortex winds are either anomalously weak, or not). The aim is to make the two sets as distinct as possible in terms of their AO index values. Figure 2 shows the distribution of the AO values for each pair of sets obtained using various different criteria.

The partition based on the polar vortex and the QBO (green and purple boxes) does show marginally lower p-values than the partition based on the polar vortex alone (yellow and red boxes) consistent with the authors' claim. However, the partition based on the polar vortex alone is split 9 months to 135 months, leading to quite a large uncertainty in the mean AO-index value for the set of 9 months. I suspect this is leading to a higher p-value. Were other thresholds for the polar vortex winds tried, other than 3.8m/s?

In the figure, the difference between the median values appears larger for the partition based on the polar vortex alone (yellow and red boxes) than for the partition based on the polar vortex and QBO (green and purple boxes). This suggests to me that the QBO isn't obviously adding any skill in discriminating between high AO and low AO winters.

2. The method is based on partitioning the winter months into

i) those with an anomalously weak polar vortex and/or easterly QBO, and ii) all the remaining winter months

It's not really obvious how this method was arrived at. Have the authors considered also separating out the set of winter months with an anomalously strong polar vortex?

Printer-friendly version

Discussion paper

It seems like an obvious thing to try, and may provide additional skill in predicting the AO.

Minor comments

p2, line 26: How exactly is the Arctic Oscillation index defined? The authors just say it's based on 1000hPa geopotential height for 20-90N.

p4 line 19: "the CRPS_RF of the CRPS_rf" - what does this mean?

p6 lines 15-22: I didn't entirely follow the method for making anomalies here - if you're just taking the mean of the 7 anomalies based on different years, aren't you going to get the same answer as just using all the years?

p7 line 19: The method defines the AO value as the lowest value of the daily AO index in different weeks of the forecast. Why was this chosen - is it representative of the northern European temperature in those weeks? The weekly mean AO value would presumably be less noisy.

p8 line 6: the zonal mean zonal wind threshold is stated as 4.8m/s here, but 3.8m/s in figure 2.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-679, 2019.

Printer-friendly version

Discussion paper