Weather Clim. Dynam. Discuss., referee comment RC1
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# Comment on wcd-2021-77

Anonymous Referee #1

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Referee comment on "Stratospheric modulation of Arctic Oscillation extremes as represented by extended-range ensemble forecasts" by Jonas Spaeth and Thomas Birner, Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-77-RC1, 2021

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The article by Jonas Spaeth and Thomas Birner entitled “Stratospheric Modulation of Arctic Oscillation Extremes as Represented by Extended-Range Ensemble Forecasts” discusses the influence of the stratospheric polar vortex on the surface climate. This topic has been discussed a lot in the literature and it is well established that extreme states of the polar vortex tend to shift Arctic oscillation towards certain states with implications for regional weather conditions. However, the strength of the link between stratospheric conditions and surface weather is poorly estimated because of a low signal-to-noise ratio. To address the low signal-to-noise ratio problem the authors turned their look towards model simulations which provide much more data sufficient to obtain robust estimates of the stratosphere-troposphere coupling. The underlying assumption is that the models provide a reasonable representation of the real atmosphere. The assumption seems to be violated at least in some cases because some estimates obtained from the two different models considered (ECMWF and UKMO) diverge significantly. Which of the two models is closer to the real world is difficult to establish. Therefore, the interpretation of the results should be done carefully. Nevertheless, the article presents novel results which in my opinion go beyond state-of-art. I believe the article can be published in Weather and Climate Dynamics after revision. My main criticism concerns the causal analysis. This should be better described and, in case if the approach used by the authors is a well-established one (I apologize for my ignorance), proper references to background literature should be made. Additionally, I strongly recommend a language check before publication.

Major comment:

- The authors distinguish between increased probability of AO extremes following stratospheric events (their question 2) and how often stratosphere can be considered a cause of extreme AO events (question 3). Both questions are addressed in terms of probabilities (e.g. Figs. 6,7,8). While I understand the difference between the two

questions in principle, I do not understand how you manage to solve them separately, given that both questions can only be answered in statistical sense. In figure 7 you show probability of at least one SSW day preceding a randomly sampled day; however in Figure 8 this probability becomes a probability of AO extreme preceded by a SSW day by chance (left panel of Figure 8). This is not the same, clearly. Assume hypothetical world in which all AO extremes are caused by an SSW occurred during previous 30 days. Then dashed lines in Fig. 7 would reach 1 by day -30. However, this would not affect your climatology because it only measures probability of SSW. As a result, you would never be able to correctly answer question 3 using your methodology. For day 30 you would only obtain the difference between 1 and an SSW probability, which is not the right answer to question 3. Figure 11 illustrates the same problem – there is no evidence in data that AO>3.5 can occur without an SPV within previous 40 days, yet only about half of those events that occurred after SPV can be attributed to SPV following your methodology. I believe the methodology needs to be revised (or I miss something).

Other comments

L18: What does it mean: "up to a degree of 27%"

L31: Please clarify whether you cite daily AO index value, monthly value or seasonally value.

L34: Do Kim et al discuss wildfires in winter or in another season?

L54: "are needed" for what?

L146: Please explain what does "dynamical SSW" mean and provide reference if it has been introduced elsewhere.

L151: "we therefore do" what?

Figure 1: Although interannual variability of predicted SSW frequency is not the main point of your article I wonder if upper panel of Fig. 1 could show relative frequency of p-SSW rather than absolute numbers. It is quite exciting to see so small number of p-SSWs in 2008/09, a winter in which an SSW occurred in the real world.

L165: “the event was generally very rare” sounds strange to me

L175: Please provide equation which you apply

L197: A rather complicated deseasonalization approach has been used. Why not used a simpler approach in which climatology is estimated using other hindcast years? For example, for ECMWF hindcasts this would provide 19x11=209 realization to build a climatology for each date and lead time. Why do you think it is not enough?

L219: “occur only few days after the event” can you provide the exact lag?

L223: I do not think NAM1000 distribution is significantly different from 0 at negative lags.

L226: the trend goes to weaker negative values, not positive.

L234: I am not sure the name “ECMWF S2S model” is correct.

L236: “most phases of negative NAM1000”, perhaps: “most cases of negative NAM1000”

L243: I do not think NAM1000 in ERA5 follows AR1 process either, or have you checked it?

L258: Should not probability of negative NAM be exactly 50%, by construction?

Figure 6: What is the period used for calculating the probability increases?

L429: I do not think that increasing number of models would help to make definitive quantitative statements unless you know which models are right and which models are wrong. Since all models are different you could only possibly increase the spread.