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Interactive comment on "Stratospheric influence on marine cold air outbreaks in the Barents Sea" *by* Hilla Afargan-Gerstman et al.

Anonymous Referee #1

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General comments:

In this manuscript, the authors evaluate whether there is a relationship between marine cold air outbreaks (MCAOs) and Sudden Stratospheric Warmings (SSWs) in the Barents and Norwegian Seas. The authors make the conclusion that 33% of SSWs are associated with an enhanced MCAO response in the Barents Sea. They furthermore conclude that a positive zonal dipole pattern in the large-scale atmospheric flow accounts for 44% of the MCAO variance in the Barents Sea. This manuscript fits within the scope of WCD in that it addresses stratosphere-troposphere coupling, and prediction on subseasonal to seasonal time scales. The authors present convincing evidence that MCAOs in the North Atlantic are most frequent over the Barents, Norwegian, and Labrador Seas, while MCAOs are more frequent in the Barents and Norwegian Seas

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Discussion paper

in a 30-day period following SSW events. However, I do not think this is strictly a new result (e.g., Fletcher et al. 2016). There is also a convincing case that the Zonal Dipole Index (ZDI) and MCAO are more correlated in the 30-days after an SSW. A key here though is that it is 'more' correlated, and it is not clear what threshold needs to be met in order for there to be a meaningful relationship. Furthermore, the composite patterns after SSWs (Fig. 3) and with MCAOs (Fig. 5) are only roughly similar. Overall, it is my opinion that while this manuscript has some promise, the results are far too premature for publication in WCD at this time. In particular:

1. The term 'enhanced MCAO' refers to "SSW events with an MCAO frequency response above a threshold of 30% are classified as SSWs with a strong MCAO response in the Barents Sea." This 30% is quite arbitrary and no justification is provided and is completely what determines the 33% value in their main conclusion.

2. There is not much of a physical connection with how large-scale fields at 500 hPa and 300 hPa connect to cold air at the surface. Given the frequent elevated inversions in the Arctic, it is not clear under what circumstances the boundary layer couples with the free tropospheric fields. The processes described by Pithan (2018) and Papritz et al. (2019), for example, may be good to incorporate.

3. To test the relationship between the ZDI and MCAO index, the authors perform a linear regression and show some scatterplots (Fig. 3), arguing that R^2 is higher just after SSW events compared to climatology. True, the values are higher ($R^2 = 0.44$ vs. $R^2 = 0.15$ in Fig. 3a), but what value of R^2 would have made the authors conclude that there is not a difference. $R^2 = 0.44$ would not be considered high in many circumstances, so why here?

4. There is not convincing evidence that the meridional wind at 300 hPa relates to meridional wind at the surface advecting cold air masses. It is argued that an R^2

of 0.13 is more meaningful than an R^2 of 0.12 to conclude that there is a stronger relationship between meridional wind and MCAO index in the aftermath of SSW events (line 145). Why not 850 hPa meridional wind (or lower)?

5. Table 1 in Butler et al. (2017) provides 24 historical major SSW events between 1979-2014, but the authors analyze atmospheric fields and climatologies from 1979-2016. Thus the range for the analysis period of this study can only be limited through 2014 only. Furthermore, the authors begin with a seasonal December, January, February (DJF) analysis, then switch to also include March (DJFM) midway through (Line 130). They should always use DJFM given that the SSWs contain March events.

6. Why is the climatology not following a standard 30-year climatology as recommended by the WMO (World Meteorological Organization 2017)? Usually it is 1979-2010.

Specific comments:

1. Table 1 in Butler et al. (2017) provides 24 historical major SSW events between 1979-2014, not 2016 as stated. This limits the range for the analysis period of this study to be through 2014 only.

2. The analysis corresponding to Figures 1 and 2 is for DJF, when some of the SSW events extend into March as the authors point out (but not until Line 130). Authors should consistently use DJFM instead of strangely adding March 'midstream' on Line 130.

3. How are the geographic boxes for the Zonal Dipole Index (ZPI) determined (shown in Fig. 3b)? Justification is needed.

4. Line 50: I assume by 'predictability', the authors actually mean 'practical predictability.' Otherwise elaboration is needed.

5. Line 75: Z_G and Z_E should be Z'_G and Z'_E, respectively, given the definition of geopotential height anomaly from climatology in Line 74. Similarly in equation (2).

6. Line 90: Insert 'in the North Atlantic' after 'MCAO index'

7. Line 105: Authors state 'More than half of the SSW events' when I count 12/24 from Figure 2 that are above 25%. Also, the text states that the mean MCAO frequency in DJF is 25%, but in the figure, it looks like 24%. So if it were 25%, that would remove at least one more sample to be less than half.

8. Line 120: It is redundant to plot anomalous 500 hPa meridional wind on a 500 hPa geopotential height anomaly plot since flow could reasonably be assumed to be quasi-geostrophic. Furthermore, it is not very convincing to assume that northerly winds at 500 hPa extend to the surface where the cold air outbreak occurs. If the point of this panel is to evaluate the large-scale flow and how it may differ from the average flow during events, it would be more informative to plot the mean 500 hPa height contours instead.

9. Line 175: The 500-hPa height patterns look quite different between Figures 3b and 5b. After SSWs (Fig. 3b), it the anomalies suggest there is a breaking Rossby wave pattern consistent with LC1 (Thorncroft et al. 1993) with a cut-off low over Central Europe, which is a very different pattern than for the strong MCAOs in the Barents Sea (Fig. 5b). This suggests that on average, the patterns may be considerably different, thus limiting how this relationship could be applied in any prognostic form.

10. The text should be reserved to discuss the results of figures, and the caption should provide instructions on how to read and interpret the figure. Lines 115 and 200 are examples where the main text repeats the information in the caption and disrupts the flow of the narrative (Fig. 3b shows..., Fig. 6 shows...).

11. What are the contours in Figure 3c?

Technical corrections:

1. Line 65: The inline subscripts ‘skt’ and ‘850hPa’ below equation (1) should be in text mode, consistent with those in equation (1).
2. Lines 75 and 135: ZDI is text, and should be written in text mode, not math mode.
3. Line 105: a half → half
4. There should be a space between numbers and units. This occurs frequently with ‘4K’, for example on line 70.

References:

Butler, A. H., J. P. Sjoberg, D. J. Seidel, and K. H. Rosenlof, 2017: A sudden stratospheric warming compendium. Earth System Science Data, 9.

Fletcher, J., S. Mason, and C. Jakob, 2016: The climatology, meteorology, and boundary layer structure of marine cold air outbreaks in both hemispheres. J. Climate, 29 (6), 1999-2014.

Papritz, L., E. Rouges, F. Aemisegger, and H. Wernli, 2019: On the thermodynamic pre- conditioning of arctic air masses and the role of tropopause polar vortices for cold air outbreaks from Fram strait. J. Geophys. Res.: Atmos.

Pithan, F., et al., 2018: Role of air-mass transformations in exchange between the Arctic and mid-latitudes. Nat. Geosci., 11 (11), 805.

Thorncroft, C. D., B. J. Hoskins, and M. E. McIntyre, 1993: Two paradigms of baroclinic-wave life-cycle behaviour. Quart. J. Roy. Meteor. Soc., 119, 17-55.

World Meteorological Organization, 2017: WMO guidelines on the calculation of climate normals. World Meteorological Organization Switzerland.

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