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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 #2

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The authors study stratospheric influence on Marine Cold Air Outbreaks (MCAO) in the Barents Sea using reanalysis data. They show that Sudden Stratospheric Warmings are followed by enhanced frequency of occurrence of MCAO in the Barents Sea. They suggest that this connection can potentially lead to improved predictability of MCAO on sub-seasonal to seasonal time scales. I believe the paper explores an interesting topic which has potentially interesting implications for forecasting MCAO at longer lead times. However, the paper fails, in my opinion, to thoroughly document stratospheric influence on MCAO and to convincingly demonstrate that the stratosphere plays a role in occurrence of MCAO in the Barents Sea. There are several points that the authors demonstrate rather clearly: (1) Climatologically, MCAO occur most frequently over Barents, Norwegian and Labrador Seas (Fig. 1a,b). (2) MCAO are more frequent over Nor-

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wegian and Barents seas and less frequent over Labrador Sea following SSWs (Fig. 1d). (3) High over Greenland and low over Scandinavia favour MCAO occurrence in the Barents Sea (Fig. 5). (4) Anomalies over both regions contribute to MCAO occurrence (Fig. 6). I believe these points are clear, but I suspect they may be not particularly new. However, the rest of the manuscript concerning the stratospheric role in MCAO occurrence is less clear to me. Specific issues, which need to be addressed before possible publication, are listed below. I recommend major revision and encourage the author to revise the manuscript and resubmit it.

Major problems:

(1) Selection of the region: Why the authors choose to focus on the Barents Sea region while it seems from Fig. 1d that SSW impacts in the selected easternmost box is nearly absent? The selection is more puzzling since the authors state in the introduction that the interest to MCAO is triggered by the risks they pose on populated Norwegian coast. The Norwegian sea region, which has enhanced frequency of MCAO occurrence following SSWs, seems to be a more logical choice.

(2) Sub-selection of SSWs: SSWs have divergent surface impacts and not all SSWs have significant surface impacts, which, in general, justifies sub-selecting only interesting events. However, selecting events based on impacts over a small region located at the edge of the canonical SSW fingerprint is dangerous because of a small signal to noise ratio. But this is exactly what the authors do. Further, the criterion that the authors choose for selecting the events seems to be very relaxed. It may be that some of the events the authors selected are not necessarily represent stratosphere-troposphere coupling. For example, December 2001 event was followed by positive tropospheric NAM during most of the winter, which is inconsistent with established stratospheric influence on the troposphere. Situation in January 2003 was similar. It is likely that internal tropospheric dynamics played a more important role during these winters. Nevertheless, both these events are listed as those having stratospheric impact on Barents Sea MCAO. My question is, what if you randomly select 24 dates over

the ERA-I period and calculate MCAO frequency during 30 days following these dates? How likely is to get 8 events followed by periods with 30% of MCAO occurrence simply by chance, given that the climatological frequency of MCAO occurrence in the region is about 23%, i.e. not much less chan 30%?

(3) Mechanism of SSW influence: Table 1 shows that ZD index is positive for most SSWs, which suggests that it may play a role in MCAO occurrence following SSWs. However, the selection of SSWs is not based on ZD index. The March 1981 and February 2001 events both have strong ZD index but they are not selected. Why not include them? Going into more details, Fig. 4 shows that Scandinavian trough is more important for MCAO intensity then Greenland blocking. However, according to Table 1 SSW has a stronger signal over Greenland, while it has insignificant influence on Scandinavian trough. Here, 11 out of 24 events show positive Z anomaly, inconsistent with proposed mechanism. Further, even the selected 8 events do not have a consistent signal in either Scandinavian trough or in Greenland blocking regions. Thus, I wonder how MCAO forecasting can benefit from SSW predictability if the mechanism of influence is not well established?

(4) Abstract (L 5-6) says that "Overall, more than a half of SSW events lead to more frequent MCAOs in the Barents Sea." However this is not supported by Figure 2 which shows that exactly half of SSWs are followed by reduced frequency of MCAO in the Barents Sea.

(5) Figure 4 shows that stronger ZD index corresponds to more intense MCAO, likely through more intense northerly flow. But what is the purpose of showing the correlation separately for periods after SSWs? What is the physics behind apparently higher correlation between ZD index and MCAO intensity during periods following SSWs?

(6) I am surprised that the authors did not collect the data for the two recent SSW events. Surface impacts by SSWs have a very low signal-to noise ratio. For example, Maycock and Hitchcock (2015) showed that a large number of events (about 50) are

required to detect difference between impacts by splits and displacements. Establishing significant signal seem to be important also for your paper. Adding two recent SSW events would increase the sample size by 8% which is a considerable improvement. The dates for the 2018 and 2019 events could be found for example in the following paper: Afargan-Gerstman, H., & Domeisen, D. I. V. (2020). Pacific modulation of the North Atlanticstorm track response to suddenstratospheric warming events.Geophysical Research Letters,47,e2019GL085007. https://doi.org/10.1029/2019GL085007.

Reference: Maycock, A. C., and P. Hitchcock (2015), Do split and displacement sudden stratospheric warmings have different annular mode signatures?, Geophys. Res. Lett., 42, 10,943–10,951, doi:10.1002/2015GL066754.

Other comments:

L50 After reading the paper I was wondering whether the Barents regions discussed in the paper is so relevant for the densely populated Norwegian coast?

L44 “weak stratospheric forcing” Is it a combination of “weak stratospheric vortex” and “stratospheric forcing”?

L88-90: How do you calculate MCAO frequency and frequency change after SSW? Please provide equations.

L109-110: “Using this classification, we are able to capture the favorable conditions for MCAO occurrence in response to stratospheric forcing.” Are these conditions different from those that favor MCAO occurrence without stratospheric forcing?

L191: Should Fig. 5a be replaced by Fig. 4a.

L220: “often followed by a more frequent occurrence” Strange expression. I don’t think that saying “It often occurs more frequently” makes much sense but it is what the authors are trying to say.

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