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Ocean Science Discussions

Interactive comment on "Wind forcing of salinity anomalies in the Denmark Strait overflow" *by* S. Hall et al.

Anonymous Referee #2

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Review "Wind forcing of salinity anomalies in the Denmark Strait overflow" by Hall et al., submitted to OSD

The authors investigate the origin of the salinity anomaly in the Denmark Strait overflow observed in 2004 at the Angmagssalik mooring array. Several hypotheses are formulated and tested using the mooring observations, outputs from a numerical simulation and reanalysis fields (for the wind fields).

Overall, the manuscript is very well structured and written and the figures are clear. The study is a welcome contribution to the current research effort to better understand the variability of the overflows, which feed the deep branch of the MOC.

Unfortunately, I have large concerns on the realism of the numerical simulation on

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which most of the conclusions rely. Moreover, the model flaw and its implications for the results are almost not discussed in the paper. These prevent the publication of the ms. as it is now.

Main point:

My main concern is the realism of the simulation, and the extent to which the results are model dependent.

- From Fig. 2, it is clear that the model does not represent correctly the salinity and density structures (but it is not stated clearly in the text!). From Fig.4, we see that the main core of current is situated close to the slope, but the realism of the velocity structure is again not discussed in the text. I'm afraid that the DSO is not separated in the model from the EGC, which might indicate a large misrepresentation of the mixing/entrainment on the sill.

- Moreover, the difference of both salinity and velocity structures might lead to large discrepancy in the mean DSOW. This question is eluded in Fig.5 as the anomalies are shown. The mean values need to be indicated somewhere.

- From Fig. 5, the observation and the model times series seems to agree only in 2004 (what might be enough to get a correlation over the whole time series, as the 2004 events is the main structure of variability for the time series from observations). Is there an explanation for that?

- Regarding the first hypothesis, how could it be tested in the model, as the model doesn't represent the mixing and entrainment processes correctly?

- Finally, H3 doesn't seem to be fully tested in the model. This would add credibility to the results. All these model deficiencies need to be at least clearly acknowledged in the text, and the method dependency of the results need to be discussed as well.

Minor points:

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Title: the study addresses only the origin of the 2004 anomaly. It has to be clearly stated that the mechanism proposed here hasn't been tested for other salinity anomalies. I would recommend changing the title to "Wind forcing of the 2004 salinity anomaly in the Denmark Strait overflow".

P. 1407 & Fig. 2: I would recommend adding the different water masses defined in the text on the salinity section. I also recommend using the same color scale in Fig. 2 to add clarity.

P. 1408, L. 13-15: I do not understand how the authors can state something about the temporal variations of the salinity using one single section.

P. 1408, L. 18-19: This sentence is purely speculative and should be removed. As I said before, the study deals only with the 2004 events and the results can not be extended to other events without further investigations.

P. 1409: I think that there is another possible hypothesis to explain the change in salinity of the waters feeding the overflow. One can imagine that the AW coming from the South presents a salinity anomaly, or that water flowing with the EGC from the Arctic presents a salinity anomaly. The anomaly doesn't need to be caused by the atmospheric forcing in the Nordic seas.

P.1410, model description: The model needs to be described in more details. Is the model domain global? If not, the boundary conditions might have an impact on your results. Does the model use any mixing parameterizations? It is stated that the overflows suffer from an unrealistic representation. This need to be discussed as it might again impact on the results presented here.

P. 1411 & Fig. 4: How does the observed velocity structure compare with the modeled one? As shown here, it seems to me that the DSOW core in the model is not separated from the EGC core. Thus the water mass properties of the so-called DSOW are totally unrealistic. On Fig. 4, there is also a signal of large correlation visible on the shelve?

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Is it discussed somewhere in the text?

P. 1413: Regarding the tracer experiment: as the model might not correctly represent the mixing/entrainment processes, the factor of dilution found here is probably unrealistic as well. It needs to be acknowledged in the text.

P. 1416 and after: Has H3 been somehow tested in the model? As H1 and H2 are rejected from model results, H3 needs to be fully tested in the model as well. In particular, is the Greenland Gyre spinning up visible in the model? I was also wondering if altimetry could be used to assess the Greenland Gyre spinning up.

P. 1420, L. 25-28: I don't think it is really clear that the NAO is controlling the Greenland Gyre strength. Local winds might also contribute.

Fig. 5(c): It is difficult to read the different color on the plot.

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