

**We would like to thank you for your positive feedback and constructive comments. Please find below answers to your questions and details of changes made based on your suggestions. The referee's comments are shown in italics, our responses in bold, and the revised text in standard font.**

***General comments***

*The authors present salinity measurements from the Ammassalik mooring array downstream of the Denmark Strait and focus on a negative salinity anomaly that occurred in 2004. Several hypotheses are formulated to explain the cause of this anomaly, which are subsequently tested using a high-resolution model. The authors conclude that the anomaly was caused by an increase in southward wind stress along the east coast of Greenland north of the Denmark Strait.*

*I think that this work is an interesting and valuable contribution to the study of variability in the Denmark Strait Overflow Water. The paper is well written and the figures are for the most part clear and easy to understand. However, I do have a few reservations about the robustness of the model results and would like the authors to consider my comments below. As such, I recommend that the paper must be revised before it can be accepted for publication.*

***Specific comments:***

*My major concern regarding this manuscript is the extent to which the results are model dependent or actually represent the real ocean. From Figure 2 it is evident that there is a large discrepancy between observed and modeled salinities at the Ammassalik array. The authors argue that this is of little consequence since salinity anomalies are examined, and show in Figure 5 that the observed and modeled salinity anomaly timeseries are highly correlated. However, the absolute salinity impacts the density structure of the ocean and hence its dynamics. In particular, the observed DSOW plume is banked against the east Greenland slope (Fig. 2a), while the dense model isopycnals are flat (Fig. 2b). From the velocity field (Fig. 4) it is evident that the model's spatial representation of the overflow significantly differs from the observed structure. In the current state of the manuscript, I do not think that the authors have showed convincingly that OCCAM does represent the basic structure of the dense overflow. At the very least, timeseries of observed and modeled DSOW transport must be shown and discussed. Are the mean and variability of the observed and model transports comparable?*

**Thank you for these helpful suggestions. We now discuss more explicitly in section 3 OCCAM's ability to realistically represent structure of the overflow as follows:**

*'We are not expecting the model to represent accurately the details of the overflow downstream of the Denmark Strait sill. However the processes that occur in the ocean, especially upstream of the sill, should be represented by the model and therefore we use OCCAM as a tool to examine the mechanisms driving the variability of the salinity that are observed. We now examine the structure of the overflow at the moorings in OCCAM.'*

**We now present in section 3 time series of observed DSOW transport taken from Dickson et al. (2008), and compare this with DSOW transport calculated in OCCAM (Fig. 5c). We show that the OCCAM DSOW time series is comparable with observations both in magnitude and variability.**

‘The volume flux of the dense overflow in OCCAM from August 1998 to December 2004, calculated by integrating the net transport for potential densities greater than 27.6 from the Greenland continental slope to 150 km from point A (Fig. 2), is roughly consistent with observations (Fig. 5c). The mean observed transport in the core of the overflow, calculated by integrating the fluxes measured by current meters UK1, UK2 and G1 for all potential densities greater than 27.85, was 2.2 Sv with a standard deviation of 0.3 Sv (Fig. 5c, see also Fig. 19.7 of Dickson et al., 2008). The transport calculated for the full mooring array of 6 current meters (F1, F2, UK1, UK2, G1, G2) for potential densities greater than 27.85 yielded a mean flux of 4.0 Sv with a standard deviation of 0.36 Sv (Fig. 5c, see also Fig. 19.7 of Dickson et al., 2008). For OCCAM, the transport of water denser than 27.6 was 6.0 Sv with a standard deviation of 0.83 Sv (Fig. 5c). Although transport calculations in both observations and model are compromised by the undefined boundaries, we conclude that OCCAM represents the transport at the core of the overflow to a reasonable degree of accuracy both in terms of magnitude and variability.’

**Given the differences in the overflow structure in OCCAM, the DSOW transport calculation is highly sensitive to the cross-section bounds used for the integration. The mean value of 6.0 Sv we now quote in the text was integrated over an area larger than the DSOW core and will include entrainment of ambient water. This value is therefore more comparable with the values determined by Dickson and Brown (1994) for the DSOW plume core plus downstream entrainment at Angmagssalik and 150 km upstream. A similar integration performed on the OCCAM DSOW plume core (defined approximately by the 15 cm s<sup>-1</sup> velocity contour in Fig. 2c), produced a mean transport of around 2 Sv, more consistent with the observational transport estimated from UK1, UK2 and G1 (Fig. 5c). We also suggest that limitations exist with the calculation methods used for estimating the observational transports shown in Fig. 5c. These were produced using velocity data from only a few current meters, interpolated over an area defined from temperature and salinity data obtained from a small number of CTD deployments. The section is not a closed section so transports depend greatly on the boundaries chosen. Any more quantitative comparison between model and observational overflow transport is therefore difficult.**

**We agree that there are offsets in water properties and core depth at the array in OCCAM, but the fundamental argument that we present throughout the paper is that OCCAM identifies the salinity anomaly at the sill and the moorings with a correct time-lag, meaning that the ability of OCCAM to realistically represent the mixing and entrainment processes downstream of the sill is less important to our story. Our basic conclusion does not rely on the model, but identifies the relationship between the observed wind and observed salinity at the moorings. The model is then used to investigate how these changes related to upstream processes driving the overflow. We don’t believe our conclusions are model dependent, but we would like to repeat our study using a different model in the future.**

*Page 1405, line 20*

*It should be mentioned that also entrainment downstream of the sill can modify the DSOW.*

**This is indeed the case. A sentence mentioning this has now been added to the text in section 1:**

‘Any changes to the proportion of each constituent source water mass making up the total overflow just north of the sill, or a modification to the salinity characteristics of those water masses, have the potential to impact the salinity of the deep overflow. In addition, entrainment downstream of the sill can modify properties of DSOW.’

Page 1407, line 23

*The Walter et al. (2005) reference is hardly appropriate here. More suitable references are for example Yashayaev (2007, ProgOc) for the Labrador Sea and Pickart et al. (2003, DSR) for the Irminger Sea.*

**Thank you for recommending some more suitable references. The Walter et al. (2005) reference has been removed, and Yashayaev (2007) and Pickart et al. (2003) have been included as references for the Labrador Sea and Irminger Sea respectively in section 2.**

*'The 27.8 isopycnal separates ISOW from the overlying Labrador Sea Water (LSW), formed by wintertime convection in the Labrador Sea (Yashayaev, 2007) and Irminger Sea (Pickart et al., 2003).'*

Page 1408, line 14

*How can you make an inference about a lack of temporal variability from one snapshot?*

**We agree with your comment. The sentence relating to the temporal variability of the plume based on a snapshot of June 2009 CTD data in section 2 has been removed.**

*On a related note, do lateral movements of the DSOW core at Ammassalik occur, and can that account for some portion of the signal in the salinity anomaly timeseries?*

**We agree that we were not explicit enough in discussing this possibility. We now argue the case against lateral movements of the DSOW core causing the anomaly more clearly in section 2 as follows:**

*'This event is identified with the same magnitude at three of the adjacent moorings spanning a distance of about 40 km (Fig. 3), so malfunction in a single instrument can be eliminated as a cause. This also suggests that the 2004 fresh anomaly is unlikely to have been caused by a lateral movement of the DSOW plume core across the continental slope, because each mooring would have measured a different value for salinity if the plume changed position and encompassed more or fewer of the moorings.'*

*In particular, it would be nice to see if there is a relationship between the salinity anomaly timeseries and the transport timeseries. If full transport timeseries are not available, velocity records from nearby current meters could perhaps be used. I would think that the authors have examined the salinity and the velocity/transport records in conjunction, and am surprised that this material is not at all presented in the manuscript.*

**Thank you for this suggestion. We acknowledge that inclusion of velocity and transport data would have strengthened our arguments. So, in addition to now presenting transport data from both observations and OCCAM (Fig. 5c as discussed in response to your first question), we now include a new figure (Fig. 4), which is a scatterplot between mooring UK 1 salinity and UK 1 u and v velocity components for 1998 to 2004. This shows no relationship between salinity and velocity. Correlation coefficients for salinity versus velocity are  $r = -0.11$  for salinity vs u velocity, and  $r = -0.03$  for salinity vs v velocity. This is consistent with the anomaly not being caused by a lateral shift of the plume along the slope. The text now reads:**

*'In addition, there is no significant statistical relationship between salinity and velocity at UK1 (Fig. 4) (correlation coefficients  $r = -0.11$  for zonal velocity and  $r = -0.03$  for meridional velocity). This*

reinforces the evidence that the fresh anomaly was not caused by lateral movements in the DSOW core, or by a variation in the overall plume transport.'

*Page 1409, line 24*

*Have the authors considered a hypothesis H2d, that the 2004 anomaly was caused by a change in the salinity of the source waters originating from salinity anomalies in the inflowing Atlantic water?*

**We had indeed already considered and investigated this interesting suggestion, and we now include two new hypotheses H2d and H2e in the text in section 2:**

'H2d – the 2004 anomaly was caused by a change in the salinity of the source waters originating from the inflowing Atlantic Water to the Nordic Seas'

'H2e – the 2004 anomaly was caused by a change in the salinity of the source waters originating from water flowing southwards in the EGC from the Arctic'

**A description of how these hypotheses are tested (and rejected) using OCCAM is now included in the manuscript in section 5.**

*Page 1410, line 7*

*Was the OCCAM model run on a global grid? If not, where are the borders of the grid and what are the boundary conditions?*

**Yes the OCCAM model covers a global domain from the Southern Ocean at 78.5°S to 90°N. This is now explicitly stated in section 3.**

'The OCCAM model (run by the National Oceanography Centre, Southampton) is a high resolution, primitive equation OGCM, covering a global domain from the Southern Ocean at 78.5°S to 90°N.'

*Page 1412, line 21*

*The timeseries of observed salinity anomalies at the Ammassalik array and the model salinity anomalies at the sill have a high correlation, and the authors take this as evidence that processes upstream of the sill are responsible for causing the anomaly at the moorings. This is not necessarily true if the model does not represent the mixing/entrainment processes downstream of the sill very well, which you allude to on page 1412, line 7. In that case processes downstream of the sill would have little influence, and the high correlation would necessarily follow.*

**We acknowledge the limitations of OCCAM to correctly represent the mixing and entrainment processes downstream of the sill, and this has now been more explicitly stated in the text in section 3 (discussed in our response to your first question). However, we reject H1 on the basis that the salinity anomaly is present in OCCAM both at the sill and the moorings. In addition there is a realistic time-lag in OCCAM between the anomaly appearing at the sill and the moorings. If H1 were true, then the anomaly would not be present at the sill. We suggest it is highly unlikely given that the anomaly appears at the sill with the correct time-lag, that mechanisms downstream of the sill could be responsible for causing the anomaly at the moorings, regardless of the limitations of OCCAM to represent the mixing and entrainment processes in between the sill and the moorings. We argue that processes**

**downstream of the sill in both the model and reality do have little influence on salinity anomalies and the high correlation is evidence of that.**

*Page 1414, line 10*

*The authors rely strongly on the assumption that the model is a fair representation of the real ocean when rejecting hypothesis H2a simply because the model does not contain glacial runoff which does not appear to be necessary to explain the anomalies. It is not unlikely that hypothesis H2a can be rejected, and the authors may be able to justify that using records of observed rates of Greenland ice melt. Studies show an accelerating ice melt, but no apparent anomalies that could explain the 2004 event (see for example van den Broeke et al., 2009, Science, and Velicogna, 2009, GRL).*

**We appreciate your comments on this and your helpful suggestion of relevant references. We now discuss this in section 5:**

‘The 2004 negative salinity anomaly is present in OCCAM at the sill, and in the OCCAM plume at the latitude of the moorings. The temporal variability of glacial ice-melt is not present in OCCAM, therefore an increase in glacial ice-melt cannot cause the 2004 anomaly. Furthermore, records of observed rates of Greenland ice melt do not show any anomalies that could explain the 2004 event (Van den Broeke et al., 2009; Velicogna, 2009). Therefore hypothesis H2a can also be rejected.’

*Page 1417, line 28*

*Barrier winds, southward winds along the east coast of Greenland, commonly occur in winter (see for example Moore and Renfrew, 2005, JCLim). Could this be the reason for the fresh anomalies observed every winter/spring (page 1408, line 3, and page 1408, line 18)? Barrier winds are associated with the passage of individual low pressure systems and are usually of short duration. Perhaps an unusually large number of barrier wind events in 2004 could, in an integrated sense, be responsible for the observed 6-month salinity anomaly in 2004. I would encourage the authors to include a figure showing the sea level pressure anomaly field over the Nordic seas for this period with vectors of wind anomalies overlaid, which I think would be very enlightening. A climatology of barrier wind events would immediately show if 2004 was an unusual year, but that is probably beyond the scope of this paper. Does the proposed process also explain the 1999 fresh anomaly?*

**Thank you for these excellent suggestions. We have now included an additional figure as suggested (Fig. 11) showing the sea level pressure anomalies and meridional wind stress anomalies. We added text as follows:**

‘Fig. 11 illustrates the composite atmospheric pressure anomalies and meridional wind stress anomalies. During the negative wind stress anomalies at section CD (Fig 11a), the composite reveals a stronger than normal southward wind, associated with low atmospheric pressure in the middle of the Greenland Sea. During the positive wind stress anomalies (Fig. 11b), the composite reveals a weaker than normal southward wind, associated with high atmospheric pressure anomaly in the eastern Greenland Sea.’

**We have investigated the possibility that barrier wind events may have caused the 2004 anomaly. We have examined a figure from a recent paper by Harden et al. (Journal of Climate, in press, doi: 10.1175/2011JCLI4113.1, 2011), which shows the number of barrier wind events occurring in Denmark Strait each winter between 1989 and 2009. 2004 is an unremarkable year with respect to barrier wind events, so we discount this as the cause of the 2004 anomaly. The new paragraph in section 6 reads as follows:**

‘It is possible that an increase in the frequency of Greenland barrier wind events (Moore and Renfrew, 2005) may be responsible for the general driving of fresh anomalies occurring at Angmagssalik. Barrier wind events occur mainly in winter, and are strong southward winds blowing along the east coast of Greenland associated with the passage of individual low pressure systems (Moore and Renfrew, 2005). North of the sill in Denmark Strait at 67.7°N 25.3°W these events occur approximately once a week with a duration of several hours (Harden et al., 2011). However, a study into the frequency of barrier wind events in Denmark Strait between 1989 and 2009 (Harden et al., 2011) showed that 2004 was an unremarkable year with respect to the number of observed barrier wind events. Moreover, barrier wind events are not resolved in OCCAM, and Fig. 11 shows only a weak tendency for the winds to blow along the coast south of Denmark Strait. So the fact that the 2004 salinity anomaly appears in OCCAM both at the sill and the moorings suggests that an increase in the frequency of barrier wind events is not the cause of the anomaly.’

*Page 1418, line 9*

*So observations show that water masses with appropriate salinities are found in the EGC, but are they also present in the model?*

**Yes, water masses with appropriate salinity are found in the EGC in the model. We now present a new figure (Fig. 12) showing the 27.7 isopycnal in the Nordic seas coloured by salinity for January 2004. This figure clearly shows a water mass in OCCAM with an appropriate salinity and density capable of causing the 2004 anomaly, present 4 ½ months before the appearance of the anomaly at the moorings and at the correct location upstream of the sill. We discuss Fig. 12 in section 6:**

‘One would expect a similar water mass to be present in OCCAM. So to further test H3 using the model, we extract salinity on surfaces of constant density from OCCAM 4 ½ months before the appearance of the anomaly at the moorings. Fig. 13 shows the mean January 2004 27.7 isopycnal in the Nordic Seas coloured by its salinity. This clearly shows water of the correct density and salinity to cause the 2004 anomaly, present in the EGC along the entire east Greenland coast 4 ½ months before the anomaly occurs at the moorings.’

*Also, the Atlantic waters of the EGC are identified by mid-depth salinity maxima – there are other intermediate water masses commonly present within the EGC (for example upper Polar Deep Water and Arctic Intermediate Water in Rudels et al., 2002) that may be more likely culprits of the negative salinity anomaly.*

**This is indeed the case, but we also argue that for our story, it doesn’t really matter what the origin of the intermediate water masses in the EGC are, because our conclusions show that it is not a change in water mass properties, but a change in transport that is causing the anomaly at the moorings.**

*Page 1419, line 12*

*It does not seem unreasonable that an elevated southward wind stress along the east Greenland coast could increase the transport of the EGC and its contribution to the Denmark Strait overflow relative to other sources. The authors state (without substantiating their claim) that an increased circulation of the Greenland Sea Gyre explains the missing advective time lag between 69°N and 75°N. Does the model show an increased circulation of the Greenland Sea Gyre? Gyres do not seem to respond very quickly to changes in wind forcing and it is likely that spinning up a gyre would be a rather slow process (see for example Spall and Pickart, 2003, JPO).*

**We acknowledge that our terminology was overly casual when referring to an increased circulation of the Greenland Sea Gyre in sections 6 and 7. We have now removed all references to a spin-up of the gyre, and focus on the increased EGC transport and sea-surface height gradient of the EGC, which we have demonstrated both increase in OCCAM during times of increased southward wind stress in section 6 .**

*On the other hand, it is perhaps not impossible that an increase in wind stress would generate a response in the form of a barotropic wave traveling southward along the coast of Greenland. This would be fast, and could potentially have an influence on the conditions downstream closer to the Denmark Strait without much delay.*

**We thank you for this interesting point regarding barotropic waves. We agree that this could account for the appearance of anomalies at sections upstream of the sill in the EGC, but barotropic waves would be unlikely to generate anomalies lasting 4-6 months.**

*Page 1420, line 25*

*Can you show a reference demonstrating that the NAO controls the strength of the Greenland Sea Gyre? Otherwise this and the following are somewhat speculative.*

**We agree with your comments on this issue and have removed all references to the spin-up of the Greenland Sea Gyre, and other speculative statements regarding the NAO.**

*Page 1421, line 7*

*One may argue that even the high-resolution OCCAM model does not accurately represent the EGC (at least not downstream of the Denmark Strait).*

**We have modified the relevant sentence in section 7 to reflect this:**

*‘It is therefore important that climate models used to predict the response of the global thermohaline circulation to anthropogenic climate forcing represent or parameterise the structure and variability of the EGC and its response to regional atmospheric forcing.’*

*Figure 2*

*Even though it is clearly stated in the text and in the caption, I think that using different color bars is misleading. For direct comparison it would be better to use the same color scale, and if necessary instead add a constant offset to the model fields.*

**We agree with this point. The figure has now been changed so each plot has the same colour scale for direct comparison. It was not necessary to add a constant offset to the model fields.**

*I would also like to see markers to indicate where the CTD stations were made along the section.*

**CTD station markers have now been added to the figure.**

*Technical corrections:*

*Page 1404, line 5 (and elsewhere) The new spelling of Angmagssalik appears to be Ammassalik.*

**We acknowledge that Ammassalik is the new spelling for the town of Angmagssalik, but for the purposes of this paper we use the spelling Angmagssalik because it relates specifically to the original name of the mooring array. Changing the name would also make it particularly difficult to search for publications related to the mooring array.**

*Page 1406, line 6*

*Regardless of the wind forcing, there is likely not one source that supplies all of the DSOW. It would be better to write that it originates predominantly from either of the sources.*

**We now state this in the text in section 1:**

‘By comparing water masses derived from hydrographic observations upstream of Denmark Strait, Rudels et al. (2003) show that on time scales from months to years, wind forcing variability determines whether the DSOW originates predominantly from either the Iceland Sea or the EGC.’

*Page 1407, line 16*

*A more common definition of DSOW is water denser than 27.88 (see for example Holliday et al., 2009, JPO, and also Dickson and Brown, 1994), though Dickson et al. (2008, the ASOF book) may have used 27.85.*

**We recognise that various definitions of DSOW exist in the literature. However, in our study we prefer to use the Dickson et al. (2008) definition as this is based on CTD readings taken from the Angmagssalik study region. We now state in section 1:**

‘Here we use the DSOW definition of Dickson et al. (2008) as water with a potential density greater than  $27.85 \text{ kg m}^{-3}$ .’

*Page 1412, line 18 (and elsewhere)*

*To increase readability, it may be better to repeat which hypothesis is to be tested in the title of each section.*

**Thanks for this great suggestion. We agree that readability is improved if the hypotheses to be tested are repeated in the title of each section. This has been addressed.**

*Page 1420, line 25*

*I would modify the start of the sentence to “This means that...”*

**This sentence (which related to the NAO) has now been removed.**

*Figure 1*

*The Iceland Sea is not a pathway.*

**We now correctly refer to the North Icelandic Jet rather than the Iceland Sea as being a pathway.**



**‘Fig. 1.** Map showing the Denmark Strait overflow sill (DS sill) and the Angmagssalik mooring array (section AB). All isobaths are in metres. DSOW source pathways: EGC (green), North Icelandic Jet (red) (Jónsson and Valdimarsson, 2004).’

*Figure 10*

*There is a dot missing after (b).*

**This has been addressed**