Answer to the referees of the paper “A multi-decadal meridional displacement of the Subpolar Front in the Newfoundland Basin” by Ismael Núñez-Riboni, Manfred Bersch, Helmuth Haak, Johann H. Jungclaus and Katja Lohmann

General comments of the authors

We thank both referees for their thorough remarks about our paper. We have modified the paper following their comments and offer herewith a revised version (attached as a supplement to an “Author Comment”). Next we describe the article’s changes in a detailed response to the referees. Our answers have been inserted after each of the referees’ queries and to recognize them clearly they have a grey background. Important changes of the article’s text are highlighted with bold fonts inside the quote. Unless stated otherwise, page, line numbers and figures refer to the current revised version of the paper. Titles in bold font are shown before key questions, for cross-references inside this text. The list of the cited bibliography is at the bottom.

During the review process we have worked further with our colleague Katja Lohmann, who has calculated some of the indices that we discuss in this answer and with whom we have had intense discussions since the beginning of the study. Therefore, we have included her now as a co-author of the paper.

We hope that this resubmission meets the requirements of the referees and editors of Ocean Science and are looking forward to their reply.

Answer to anonymous Referee #1

The authors study the multidecadal variability of the Subpolar Front in the Newfoundland basin. This manuscript gives interesting results on the link between the variations of the front and the Meridional overturning circulation, and meridional heat transport. However, the analysis of the causes of the variations of the SPF position is not totally convincing, and additional sensitivity experiments could be run to clarify this point. Therefore, I recommend publication after a major revision.

Major Comments:

Key Question 1. Match between observations and model at the interannual time-scale

The authors state that "the modelled mean position of the Subpolar Front in the Newfoundland Basin is roughly in phase with the observed one". However it does not seem so obvious when comparing both curves on the lower middle panel of figure 1. What is the correlation between these two curves?

The deviation between the two curves in the first 10 years of the run leaded us to drop these data from our analysis in the present version of the paper. Such measure allows the model for 10 years of adjustment to the forcing (see for instance Frankignoul et al., 2009) and stress better the match between model and observations. The correlation between the SPF indices is 0.4 now. While this is a relatively poor correlation, we strongly believe that

1) The inter-annual variability of observed and modelled indices is in phase, i.e., year to year increments or decrements are nearly simultaneous, but the amplitude of the variations differs between the indices and mainly only before the 1980s. After the 1980s the phase and also the amplitude match well between the two indices.
2) Most important for us: the correlation between the filtered indices is 0.96, which indicates that the modelled SPF index reproduces the multi-decadal displacement of the SPF correctly.

Changes in the paper due to neglecting the first 10 years of the time series are the following:

1) We have inserted the following sentence at the end of the actual Section 3.1 (page 4, line 18): "Because model and observations differ strongly in the 1950s as the simulation adjusts to the forcing, we discuss only the results from 1958 onward".
2) Because the record start has changed, the filtered time series are also slightly different; therefore, the description of the filtered time series (page 7, line 22) have been corrected to be consistent with the modified Fig. 3 (currently Fig. 4): "The NAO (panel a) increases **from anomalously low values in the 1960s** to its maximum in **1991** and finally decreases to the end of the record. The MOC

(panel b) is anomalously slow until **1975**, when it starts to increase to its maximum in **1988**, to slow down to the end of the record. The SPG intensity (panel c) decreases to its minimum in **1975**, increases to its maximum in **1992** and then decreases again to the end of the record. The SPF in the NFB (panel d) is south of its mean latitude during the 1960s as it shifts northwards to its maximum latitude in 1981 (before NAO, MOC and SPG maxima). Then, the front displaces southwards **to the end of the record**".

Further changes relating the comparison of model and observations at the interannual time scale are:

1) Following a suggestion of Reviewer #2, we have modified the description of the comparison between modelled and observed mean salinity indices (page 6, line 9):

"Modelled salinity at 500 m depth in the subpolar region (...) matches the CliSAP observations well (...), **but the arrival of low salinity waters of the 1980s is unfortunately missed by the model. One possible explanation could be that MPIOM does not reproduce the Great Salinity Anomaly in the 1980s (GSA; Belkin et al., 1998)**".

2) To improve the match with the observations, LSW thickness has been redefined by taking values between December and May. The description of the corresponding panel of Fig. 2 (previously Fig. 1) has been modified (considering also the GSA) as follows (page 6, line 14):

"The model roughly reproduces observations from Curry et al., 1998 (lower left panel in Fig. 2): decrease of LSW from the 1960 until reaching a minimum in 1970 and an increase towards the 1990s. However, and despite our efforts to objectively choose the isopycnals defining LSW (Sect. 3), the match is poor during the 1980s. This could be due to the absence of the GSA in the 1980s and the corresponding convection suppression. LSW thickness has been calculated in the region of maximum winter convection (which is not necessarily the same in model and observations). On the other hand, it is also possible that the negative LSW thickness anomaly in the 1980s from Curry et al., 1998 is overestimated: it is considerably larger than the negative anomaly of the 1970s, while the PEA anomalies of the 1970s and the 1980s are of similar magnitude. The SPG intensity and subpolar salinity show even a larger negative anomaly in 1970s than in the 1980s".

3) Finally, we believe that the absence of the GSA explains also the mismatch in the 1980s between our MOC and the one from Frankignoul et al., 2009. Therefore in the comparison with their study we have inserted the following sentence (page 6, line 27):

"The intensity of the MOC (...) is in good agreement with the MOC index of Frankignoul et al., 2009. **The mismatch in the 1980s could be also due to absence of the GSA, convection suppression and the corresponding negative LSW volume anomaly"**.

As the authors are interested in low frequency variability, they should plot the low pass filtered mean latitude of the observed SPF in figure 3d.

In agreement with the referee, we have inserted the filtered observed SPF index to Fig. 4 (dashed curve in panel d, previously Fig. 3), showing a fairly good match with the modelled index. Changes in the paper due to this new plot are:

1) Insertion of this sentence at the end of the figure caption (page 25, line7): "The thick dashed curve in panel d is the low-pass filtered SPF index from the observations".
2) Insertion of the following sentence in Section 5 (page 8, line 22): "The low-pass filtered SPF index from observations (Fig. 4d, dashed curve) roughly matching the modelled one (solid curve) confirms that the model correctly reproduces the multi-decadal displacement of the front".

Key Question 2. Sensitivity experiment

In order to obtain the contribution of the wind stress curl to the barotropic stream function, the authors compute a Sverdrup balance, using, I suppose, a flat bottom. However to take into account the influence of the topography and the stratification of the model on the contribution of the wind stress, they should perform a numerical sensitivity experiment forced with variable wind and constant heat fluxes (e.g Eden and Willebrandt, 2001).

In agreement, we have performed a sensitivity experiment with variable wind and climatological heat fluxes, putting special attention on using the same parameters and executable than in the previous NCEP run. The results support a large effect of the wind stress curl (WSC) on the SPF dynamics at the multi-decadal time scale, as expected. However, we believe that the sensitivity experiment gives also only a partial answer to the separation between the WSC and heat flux contributions because the climatological heat fluxes could change the mean ocean state (for instance, the MOC). We still consider Sverdrup transport as useful because it is a simple representation of the wind forcing and avoids climatological values. Therefore, both methods are discussed in the present version of the paper as complementary to each other.

Changes in the paper due to the sensitivity experiment are:

1) Modification of Fig. 3 (currently Fig. 4) by inserting a panel f showing the SPF index of the sensitivity experiment.
2) Insertion of a new figure (currently Fig. 9), showing the anomalies of the barotropic stream function of the sensitivity experiment (i.e., the inter-gyre gyre)
3) Discussion of these results, by inserting the following 2 paragraphs at the end of Section 5.1 (page 10, line 22):

"To corroborate the idea that the WSC plays an important role on the multi-decadal variability of the SPF, we have performed a sensitivity experiment with variable wind and climatological heat fluxes. The SPF index from the sensitivity experiment (SPF_{sens}, Fig. 4f) becomes positive some years after the SPF index (full forcing experiment). The same applies for the maximum, which is in the early 1980s in the case of the SPF index and in the late 1980s for the SPF_{sens} index. However, the overall behaviour of the SPF in the sensitivity experiment at the multi-decadal time-scale is the same as in the full forcing experiment: The SPF was south of its climatological position in the 1960s and after 2000, while north of its climatological position from the 1970s to the 1990s".

"Circulation anomalies in the sensitivity experiment (Fig. 9) differs from those of the full forcing experiment (Fig. 6) north of 55ºN. The 1960s and 1970s (upper panels and middle left panel) stand out because of a strong positive circulation anomaly in the sensitivity experiment north of 55ºN, while in the full forcing experiment the circulation anomaly is negative in this region. The opposite happens in 2001 (lower right panel), with a negative circulation anomaly in the sensitivity experiment and a positive anomaly in the full forcing experiment. However, south of 55ºN, and particularly in the frontal region, circulation anomalies of both experiments are similar: Positive circulation anomalies in the 1960s (upper left and upper middle panels) and after 1996 (lower middle panel), as the SPF is south of its climatological position, and negative anomalies from 1976 (middle left panel) to 1991 (lower left panel), as the SPF is north of its climatological position. The only exception is around 1971 (upper right panel), a period in which the inter-gyre gyres in the frontal region are different, in agreement with the SPF of the sensitivity experiment (Fig. 4f) crossing its climatological position some years after the SPF of the full forcing experiment (Fig. 4d). While these results suggest that WSC and heat fluxes have opposite effects on the dynamics of the subpolar region north of 55ºN (in agreement with the differences between the WSC and the LSW circulation components), they clearly support the notion of the WSC having a large effect on the multi-decadal displacement of the SPF".

4) Insertion of the following sentence in the conclusions (page 14, line 18): "A sensitivity experiment with variable wind and climatological heat fluxes (Fig. 4f and Fig. 9) supports the notion of the WSC having an important effect on the SPF displacement at the multi-decadal time scale".

Key Question 3. Dynamics

The contribution of the heat fluxes to the barotropic stream function is computed as a residual between the "WSC contribution and the total stream function". The authors considered that this contribution is mainly related to LSW changes. On one hand, the authors supposed that the oceanic response to the forcing is linear at multidecadal timescale. Could they prove it with a sensitivity experiment or give some references? On the other hand, the WSC is crudely estimated, so is the "LSW contribution". Therefore, the comparison of these two contributions is questionable.

We have not said that the ocean response is linear with the forcing, but separated the forcing in a linear sum of two components, one related to WSC and the other one to density changes. This separation is based on theoretical studies of the large-scale ocean circulation, like Sarkisyan and Ivanov, 1971 or Holland, 1973, which state that two major components of the vertically integrated mass stream function are related to WSC and bottom pressure torque. An example of this (simplified) vertically integrated vorticity equation is, for instance:

$$\beta \frac{\partial \psi}{\partial x} = \hat{k} \cdot \nabla \times \left(\tau + p_b \nabla H \right),$$

where ψ is the barotropic stream function, β the derivative of the Coriolis parameter against latitude, τ the wind stress, $\hat{k}$ a vertically pointing unitary vector, *H* bathymetry and p_b bottom pressure. This equation is derived from Equation 5 of Holland, 1973 considering steady state and ignoring lateral friction, nonlinear advection and bottom stress. An additional restriction of this relation is its inadequacy in the western boundary region (known from Sverdrup balance). The two terms on the right hand side of the equation are thus the most important circulation components of the barotropic stream function: The first one ($\hat{k} \cdot \nabla \times \tau$) is related to the contribution of the WSC (flat-bottom Sverdrup transport) and the second term ($\hat{k} \cdot \nabla \times \left(p_b \nabla H\right)$) is the bottom pressure torque, related to the contribution of density changes. Reduction of the barotropic stream function to one (WSC; Böning et al., 1991) or two components (Mellor et al., 1982) is a typical first order simplification in oceanography.

As it is clear from its mathematical expression, the bottom pressure torque is a vorticity term that couples the density stratification (inside p_b) and the bottom topography *H*. The referee suggests that instead of using the flat-bottom Sverdrup relation we should "take into account the influence of the topography and the stratification (…) on the contribution of the wind stress" (Key Question 2 above). However, this suggestion implies including the pressure bottom torque in our integration, which is exactly what we do not want. Only by ignoring the coupling between stratification and topography we can estimate the contribution of the wind stress *alone* and distinguish it from the effects of the stratification changes. Finally, the referee states that our WSC contribution to the barotropic stream function is "crudely estimated". However, our estimate matching well previous ones, like Böning et al., 1991, supports the notion that our WSC circulation component is correct.

Specific comments:

Key Question 4. Data density

Section 2: The authors should specify the time evolution of the data density in the Newfoundland basin.

We have inserted a figure marking bins of 1ºx1º in black if they had at least one observation in periods of 5 years, i.e., with a format similar to our 2-D-plots. This is Fig. 1 in the present version of the paper and is referenced in the text immediately after the description of the CliSAP data set (page 3, line 24).

Sections 3: The section 3 should be divided in two subsections: 3.1 Model set-up and analysis methods; 3.2 Model evaluation.

Following the suggestion of the referee, we have changed the titles of Section 3 as follows: The old Subsection 3.1. “Model evaluation” is now Subsection 3.2 (with the same name). A Subsection 3.1 “Model set-up and analysis methods” has been inserted at the beginning of Section 3. Section 3 is now simply called “Model“.

p. 458: The domain of the model and the frequency of the atmospheric forcing should be mentioned.

The model is global. For clarification we have modified the initial description of the model (page 4, line 6): “The model used in the present study is the ocean general circulation model MPIOM (...) **with global domain**”. The atmospheric NCEP forcing was averaged in time bins of 24 hours. The frequency of the forcing has been added to the corresponding paragraph in the (new) Subsection 3.1 (“we force the ocean using **24-hourly** NCEP/NCAR data”, page 4, line 15).

p.458, l.9: The model resolution is 0.4º, but the model outputs have been interpolated on 1ºx1º grid. I supposed that this interpolation was done to match the observations resolution. However, except for this comparison the interpolation does not seem to be useful all the more that the variations of the mean latitude of the SPF rarely exceeds 1º (fig 3d). A computation of the SPF index done on the original grid would allow to better resolve the variations of the front.

The reviewer is right; a reason for the interpolation is to compare with the observations, which are interpolated in a 1ºx1º regular grid. However, the representation of the SPF in a regular grid is needed because it simplifies the interpretation of a displacement South-North. Thus, when computing the horizontal gradient in the curvilinear grid of MPIOM we still have to rotate the gradients to match the regular geographical coordinates. This introduces an error similar to the interpolation in the regular grid. Furthermore, the inter-gyre gyre is a basin-scale circulation anomaly affecting both the SPG and the STG (see also Key Question 7 below). Hence, the average position of the SPF in the Newfoundland Basin (NFB) is the result of horizontal density gradients occurring in large regions of the North Atlantic (i.e., with space-scales larger than 1º) and is not set by the fine structure of the density field or by small-scale changes within the NFB. The suggestion of the referee of plotting the mean salinity at 500 m depth in the NFB (Key Question 5 below) harmonizes with this notion: The average over a region spanning 4 latitudinal degrees resembles the SPF index well. Therefore, we believe that the density data interpolated in a grid of 1ºx1º describes the meridional changes of the SPF well.

p.458, l. 21: The box to compute the SPF index encompasses the Northwest Corner. However, according to the authors, this loop is not present in the model. Can this explain the discrepancy between the observed and the modelled SPF index? Is the observed SPF index sensitive to an eastward shift of the box?

In agreement with the referee's suggestion, we have verified that our definition of the SPF index for both model and observations is relatively insensitive to an east-west shift of the definition box. On the other hand, the North West Corner is a part of the SPF in the NFB and thus shows the same multi-decadal shifts as the whole front. Therefore, we believe that the absence of the North West Corner in the model is not the reason of the mismatch with the observations at shorter time periods, but rather inter-annual processes in the frontal region which the model apparently cannot reproduce.

p. 460, l. 8: The authors compare the gyre index of their model and Hatun et al. model (fig 1, upper middle panel). However, the two indices are not defined in the same way. In Hatun et al. paper, the gyre index is defined as the first principal component of the sea surface height, whereas in this study the index is an average of the barotropic stream function between 45 and 65ºN and 20 to 60ºW.

Our choice was based on high-latitude stratification being weak, which renders subpolar gyres as nearly barotropic and the SSH as a good proxy of their transport. Häkkinen and Rhines, 2004 supported this notion for the SPG by combining satellite altimetry with in situ current observations. However, the referee is right and a good comparison can only be done through the first EOF of the SSH, which is what we show in the current Fig. 2 (previously Fig. 1). The reference to the figure in the text and its caption have been modified accordingly.

p. 460: A more quantitative comparison of the model variability could be done by giving correlations between the time series displayed in figure 1.

In agreement with the referee, the correlations between the indices in Fig. 1 (now Fig. 2) are shown in each of the panels in the present version. This sentence has been added to the figure caption (page 23, line 11): “The numbers inside the panels are the correlation between the corresponding solid and dashed curves”.

Key Question 5. Mean salinity in the NFB

Section 5 P 462, (l.14-16): To illustrate the variation of the salinity in the Newfoundland Basin a panel of the mean salinity at 500 m depth in the NFB could be added to figure 3 below the panel describing the SPF index. This will stress the link between these two indices.

We agree with the referee and have inserted the plot in a panel e of Fig. 4 (previously Fig. 3), showing a good match with the SPF index. Changes in the paper due to this new plot are:

1) Insertion of this sentence after the first paragraph of Section 5 (page 8, line 20): “A comparison between the SPF index and the mean salinity at 500 m depth in the NFB (42 to 46ºN and 30 to 50ºW; Fig. 4e) confirms that the salinity anomalies from Fig. 3 are related to the front displacement”.
2) Insertion of this sentence in the caption of Figure 4 (page 25, line 4): “mean salinity in the frontal region of the NFB”.

p.467, l. 2-5: The authors could compute the SPG intensity index, the DBWC transport at 53ºN and the subpolar SST in their model and compare these indices with the SPF index.

The SPG intensity index was already included and discussed in the previous version of the paper (Fig. 3c). In agreement with the referee’s suggestion, we have calculated the indices corresponding to SST in the subpolar region and the DWBC transport. Thanks to this we have realized that our study is not comparable with Latif et al., 2004 because we use an uncoupled model where the surface heat fluxes are prescribed: In our model the SST is not free, is strongly damped by the surface restoring of salinity and the feedbacks are other than in a coupled model. Therefore, we have decided to remove the comparison with Latif et al., 2004 from the paper.

On the other hand, because the DWBC is a part of the SPG, its intensity should be concomitant with the SPG intensity at the decadal time scale. We have confirmed this with our DWBC index but due to its strong similarity to the SPG index (Fig. 4c in the paper, previously Fig. 3) and for brevity we have decided not to show it in the paper and simply describe it by inserting the following (page 12, line 25):

"The latter is confirmed by comparing the SPF to the volume transport of our modelled DWBC at 53ºN (southward transport across a section next to the coast, depths larger than 2472m; not shown): This transport matches the SPG index (Fig. 4c) and is, therefore, roughly simultaneous with the MOC in the subpolar region (Fig. 4b), conciliating our results with Böning et al., 2006: the intensification of the DWBC volume transport in the 1980s and 1990s would be related to more LWS in the west, larger zonal density gradient and MOC intensification".

The authors suggest the SPF index as an indicator of intensity changes of the MOC in the subtropics. However, relationship between the MOC variability and the Greenland Scotland overflow have been proposed (e.g:). Thus have the authors investigated a possible connection between the SPF position and the Greenland-Scotland overflow?

Following the referees' suggestion, we have calculated the Denmark Strait overflow (densities larger than 27.85, depths larger than 260 m) and found out that it precedes the MOC intensification in the subpolar region by approximately 10 years. Köhl and Stammer, 2008, find an influence of the overflows on the MOC with a shorter time-scale (ca. 5 years). On the other hand, Latif et al., 2006 argue that most of the decadal variability of the MOC is due to convection in the Labrador Sea and not due to the overflows, while the observational study of Olsen et al., 2008 even disputes an influence of the overflows on MOC changes. Therefore, and while we believe that the influence of the overflows on the MOC intensity is an interesting topic by itself, we believe that our results cannot clarify the issue, which is complex and lies beyond the scope of our study. Hence, we have decided to leave the overflows out of the paper.

p.467, l. 15. What does "additional" refer to?

The word "additional" has been changed by "anomalous".

The northward propagation is not clear on the plot, neither the 15 year time scale.

This part has been revised like this (page 13, line 7): "Part of the **anomalous** heat in the frontal region seems to propagate northwards with the overturning circulation to **51**ºN in a period of **8** years".

Answer to anonymous Referee #2

Key Question 6. The depth of LSW

The main idea of the paper, as I see it, is to compare observations with a model, and offer some explanations to observed variability. This is an interesting study, but I think simple correlations are not sufficient for linking two processes, the chains of causes and consequences are too long and each link is essentially non-linear, so I would suggest a more careful rethinking of the process and mechanisms suggested in the paper. Like the volume of LSW is not an easy measure – LSW is not a body proportionally expanding and shrinking in time, so its volume can't be measured by a position of the SBF, and even more it can be much deeper than a type of processes discussed here.

LSW volume is related to its thickness, which is an indicator of its spreading (Curry et al., 1998). On the other hand, because we are interested on the barotropic component of the circulation, the depth of LSW or any hydrographic changes is irrelevant in our study: the two major circulation components (WSC and density changes) arise from the vertically integrated vorticity equation, as described in the Key Question 3 above.

More specifically, but not in order of appearance in the manuscript:

Key Question 7. Space scale of the inter-gyre gyre

I heard about "inter-gyre gyre", but don't know of any eddy-like oceanographically sound or quasi-permanent feature in the Newfoundland Basin other then Mann Eddy, a smaller eddy E-SE of Flemish Cap and the North-west corner or a distinct anticyclonic loop of the North- Atlantic Current. Is IGG the same thing as the Mann Eddy or all three features I mention here combined together? I suggest to authors to relate their characteristic to more established or classical definitions, crediting the real features and not their model imitation.

The inter-gyre gyre exists only as circulation anomaly and is a concept from the modeling community well established in the literature (Marshall et al., 2001, Eden and Willebrand, 2001, Eden and Greatbatch, 2003). Its space magnitudes are from north to south between 10 and 20 latitudinal degrees and from east to west the complete North Atlantic Basin (Fig. 6 of the paper, previously Fig. 5), i.e., its diameter is more than 1000 km. The Mann Eddy (the largest feature that the referee mentions) is a persistent circulation, one order of magnitude smaller and restricted only to the western Newfoundland Basin (NFB).

I don't thing the separation of wind and water-mass production is valid. Wind in fact is a strong player in the latter, because it establishes gyre circulation important for water retention and together with SST-TA determines the heat loss from the sea surface controlling production of LSW and other intermediate water masses.

Our answer to this topic is below under Key Question 9.

I disagree that the seasonal processes are small at 500 m. In fact in and near the convection and boundary regions, the winter pulses of water mass renewal create strong signals seen as deep as 500 m (line 14), at least, and 1000 m or even 1500 m in some years. So, any work like that would require a proper examination of seasonal variability if data coverage permits. This brings the next question.

The seasonal changes at 500 m are one order of magnitude smaller than at the surface (as can be seen with Levitus, 2005 data) and also one order of magnitude smaller than the decadal changes (the hydrography re-stratification relating convection in the Labrador Sea is not a seasonal but a decadal feature; Lazier et al., 2002). Therefore, even in the convection regions the seasonal processes are small enough to be neglected in our study.

How good is the data coverage for the annual resolution? How strong is a bias of shifting distribution of available observations over the decades? Could any of the observed changes be due to shifts from the IIP data in earlier years to Soviet Section to Argo? Consider that the box used in the analysis is large and any shifts in data coverage between years and decades would play there. Each value should have some indicator of its error due to irregularity and sparseness in sampling. Adding here the question to seasonality (you would definitely see it in Argo if not in other data), I would question the quality of the observed metrics presented in the work – I wouldn't if had this paper for review two decades ago, but we know better by now about the data-related problems I bring up here.

The issue of the data coverage in time and space is clarified in the present version of the paper with the new Fig. 1 (Key Question 4 above), which shows a good data coverage in the study area. With “IIP data” the reviewer seems to refer to the “International Ice Patrol” (IIP, 2011), which uses satellite tracked drifters (but not profiling floats) to monitor SST and currents near the Grand Banks of Newfoundland. Our study concentrates at 500 m depth and, hence, we did not use these surface data. We only use quality controlled CTD, bottle and Argo data from international renowned programs as WOD05, HydroBase2, ICES, WOCE and CLIVAR. Because our observations are low-pass filtered, errors due to irregularity and sparseness in sampling have a minor impact on our results.

Key Question 8. LSW definition

Potential density anomaly of 27.6 is too low for LSW, and it is not as trivial for defining if there is more than one vintage present at the same time. So, the approach to identification of LSW may also be improved.

Indeed the potential density of the modelled LSW does not match the observations. This lack of agreement between modelled and observed density is not particular to our model (MPIOM), but is a known issue of many (if not all) OGCMs. Because of these disagreements between observations and modelled hydrography, we have tried to define LSW in the observational way, i.e., by tracking the salinity minimum in the model output. From our point of view this approach is an improvement over previous modelling studies which did not even find a salinity minimum in the North Atlantic and were compelled to define LSW with kinematic or vertical density shear criteria. Hence, we are confident that our definition reproduces the multi-decadal variability of LSW in the North Atlantic fairly well.

I wonder what was the effect of salinity drift in the model used in the current study on the ocean stratification, mixing and the important characteristics presented here?

The model has a multi-centennial spin-up phase and the salinity drift is small (as in similar modelling studies). Furthermore, salinity values are constrained with surface observations with a short relaxation time.

Figure 1. Considering amount of smoothing applied to the data, and that not the actual value, but its normalized form used, I don't think that the agreement is as striking as the authors conclude. One could notice agreement in parts of the simulated and observed records, while other parts don't show any. What the author might show was a simulate vs observed plots showing lines and indicating respective years or decades. Separating the data and model lines by an offset would also show spots of agreement/disagreement a bit better. For example, if you remove the last five years from SPF plot, would you see agreement in the lines? Or take just the second half of salinity data. The arrival of low salinity waters of the 1980s was totally missed by the model.

Contrary to the referee's interpretation, the data in Fig. 1 (currently Fig. 2) are non-filtered annual averages (no smoothing has been applied). Considering this, the match between observed and modelled data is in some cases (like the PEA) outstanding good. To avoid similar confusions, we have modified the caption of the figure like this (page 23, line 6): "Comparison between our simulated (...) and observed or previously simulated (...) indices derived from **non-filtered** annual data...".

To scale the indices is a typical procedure to compare observations and model output considering the hydrographic bias inherent to all models described above (Key Question 8). But Referee #2 is right and the general lack of agreement between model and observations at the interannual time-scale has been pointed out by Referee #1. We have answered this issue, including the salinity mismatch, under Key Question 1 above.

Key Question 9. Relative importance of WSC and LSW

Page 464, lines 3-4. As I already mentioned the two processes are not independent – you can't say WSC has no affect on atmospheric circulation or other way around and heat flux and therefore production of LSW.

Page 464, lines 10-13. It is very strange to hear that if it is not due to WSC, it mast be a result of hydrographic changes, mainly LSW, and therefore everything at 500 m not explained by WSC is attributed to LSW. I have a different opinion and tend to think that some other no less important processes are left out of picture.

Our study deals with the North Atlantic’s large scale circulation and its multi-decadal variability. The principal dynamics, those of the ocean gyres like the SPG, are described as answer to Key Question 3 above, where the separation in WSC and LSW contributions is justified. While we cannot exclude that other processes (like mesoscale processes local to the NFB) could play a role on the SPF displacement, our two-component separation is a first order approximation to the problem. We actually agree with the referee that WSC and water mass formation (or heat fluxes) are coupled processes and conclude in the paper that they share similar amounts of variability. To confirm this notion, we have performed the sensitivity experiment (discussed in the present version of the paper and in Key Question 2 above). But even though the coupling, the question about which process plays a larger role on the North Atlantic dynamics has no obvious answer because of different time responses of the circulation to changes of WSC (some months) and density (years). Furthermore, this question has not been rose by us but by others (as stated in the paper’s introduction), like Eden and Jung, 2001, who stated that heat changes are mainly responsible for oceanic changes at the decadal time scale. Others have found a different dependence of the SPG’s variability with the forcing at other time scales, like Häkkinen et al., 2011, who found a larger influence of the WSC.

Pages 464-465. The statement between the two pages is very speculative, and I would add very much counterintuitive – there was a record high production of LSW in the late 1980s – through mid-1990, so how can one be sure that WSC played a greater role on the southward displacement in the 1990s?

The record high production of LSW during the 1980s and 1990 that the referee mentions is seen in Fig. 8. This positive LSW thickness anomaly will be related to a fully developed anticyclonic inter-gyre gyre in the 1990s (lower panels in Fig. 8), which arrives to the frontal region only at the beginning of the 21st century (lower right panel). On the other hand, changes on WSC induce anticyclonic circulation anomalies at the frontal region already in the 1990s (Fig. 6, lower panels). Therefore, changes of WSC are a more probable reason for the southward displacement of the SPF during the 1990s than LSW changes. We see, thus, no contradiction.

I am not sure if SPF is really affected by LSW – LSW is generally deeper than SPF, but is there was such a connection between the two the whole period of the 1990s would be ideal place to look for one.

As stated above, the depth of LSW is irrelevant for the processes described in our paper (Key Question 6 above).

Section 5.2. I totally disagree with the logic of measuring LSW volume changes with SPF position – the processes may be connected to a common large scale pattern, and therefore correlated, but I wouldn't be using one as an indicator of the other – this would mislead the readers. Disagree with the last statement. Fig. 3d is probably similar to NAO (even that might be questioned), but don't see much of LSW.

It is accepted that density changes modify the barotropic component of oceanic gyres (two of various possible citations are in the answer to Key Question 3). Similarly, Eden and Willebrand, 2001 have modelled that the amount of LSW modifies the barotropic component of the SPG at interannual and lager time scales. We have no knowledge of disagreement with their study or similar ones. Such recognized studies are only one step away to the conclusion that the reviewer debates here: When the LSW changes arrive to the frontal region the associated circulation and density changes are inherently related to a SPF displacement. However, to deal with the referee's concern and acknowledge that our results cannot be generalized, we have added the following sentences to the conclusions (page 14, line 6):

"Since we have analysed only one complete front displacement, we would like to stress that ours is a case study with results reflecting the dynamics of the last 50 years only. Further research is needed to prove if the shown relations between WSC, LSW and the latitudinal position of the SPF at the multi-decadal time scale are of general nature".

Key Question 10. LSW and the low-pass filter

Section 5.3. Starting statement – I though LSW volume changed not like that (negative before 1976 and positive after 1976), there were strong changes inside decades and between the decades.

Figure 8. I don't think it agrees with observations. First the magnitude of LSW thickness anomaly – from 1966 or 1971 to 1996 it must be more than 100 m of total change in the Labrador Sea and near. Then the strongest convection was observed in the early 1990s leaving a very thick LSW to mid-1990s and first years after. So, why 1996 is so

much smaller than 1986 and even 1981 when not much of LSW was formed? 1986 was a record high, but was it really?

The data is low-pass filtered with a cut-off period of 25 years. This softens the large changes that the referee might be expecting to see inside and between decades. If we keep in mind the filter, the changes agree with the referee's description: From Fig. 8 we see changes of approximately 150 m in the Labrador Sea between the 1960s (-50 m) and the 1990s (more than +100 m). The reviewer might be expecting larger amplitude, but he/she is overlooking the softening effect of the filter. The reviewer speaks here again about a peak of LSW in the early 1990s, which is displaced some years towards the past (in the late 1980s) in our model output due to the low-pass filter. Maxima and minima do not match between filtered and non-filtered data.

Section 5.4. I don't understand why 1991 was strongest in the mid- latitudes. If the strongest convection was observed closer to mid-1990s and it takes time for the signal to arrive it should be there after 1995 or so. So, it is not obvious to me from the present study how all these processes are inter-related, and how do they express themselves in the position of SPF at 500 m.

The reviewer refers here to Section 5.3. She/he means the maximum intensity of the MOC in the subtropics by the year 1991. As described above (Key Question 10), the strongest convection in the mid-1990 is a peak belonging to a time scale which is filtered out in our results. The interannual maximum convection does not necessarily match the multi-decadal maximum, which occurs some years before as reflected by our modelled low-pass filtered LSW thickness (Fig. 8). That the signal arrives to the subtropics at the beginning of the 1990s is, hence, consistent. On the other hand, the mismatch between model and observations at the interannual time-scale has been extensively targeted in the paper.

I think the theme is a very interesting and also challenging area for research, but more effort needs to be put to advance along the lines suggested by the authors. The Newfoundland Basin is a very complex area with 3-dimesional circulation and variability and as a good starting point I might also suggest to examine existing literature on the Newfoundland Basin and the areas around it.

We thank the reviewer for her/his suggestion and agree that future studies should deepen into the 3-dimensional circulation and local dynamics of the NFB. However, as a first step to understand the multi-decadal variability of the SPF, we concentrate in our study on the basin-wide barotropic circulation involving the whole North Atlantic, i.e., the subpolar and subtropical gyres (Key Question 3).

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