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Ocean Science
Discussions

Interactive comment on "The link between the Barents Sea and ENSO events reproduced by NEMO model" *by* V. N. Stepanov et al.

V. N. Stepanov et al.

vlnst@hotmail.co.uk

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Reviewer #1: The main shortcomings of the manuscript are a weak support of the model results by the observations and lack of a convincing explanation how the link between ENSO events and ocean temperature in the Barents Sea operates. On the other hand, the impact of the local forcing in the Barents Sea is clearly described and is an asset of the manuscript. The atmospheric teleconnections, frequently referred to in the manuscript, are not well explained. The study uses a forced OGCM with a strong restoring to climatology or with assimilation. This approach constrains the ocean model and does not allow it following freely the atmospheric forcing. Yet, the authors put an emphasis on the role of the atmosphere in ocean variability. A discussion on how the model setup affects the analysis is required.

Additional references (published in English) supporting the model results by the observations have been added. It is unknown how the link between ENSO events and ocean temperature in the Barents Sea operates, though additional references were added demonstrating teleconnections between ENSO events and the northern hemisphere. There is no relaxation of the deep ocean in any of these experiments and data assimilation only helps removing model bias, therefore we would not be so confident that this ocean model does not allow it following freely the atmospheric forcing (e.g., results by Stepanov et al. 2012 (Q. J. Roy. Meteror. Soc) demonstrate similar variability of the MOC for both free and assimilation runs that is a main climatic factor, though the mean values are different). The same we see for model results obtained with coarse and fine resolutions described by the paper. Some discussion on how the model setup affects the analysis is added.

The other minor criticism is that the model description needs tiding up. Overall, clarity of the text needs improving. The reviewer suggests that the manuscript could be published after a revision. Please see the comments below.

The model description was improved.

Specific comments Title The word "reproduced" does not sound right in this context. The model does not "reproduce" a physical event, but simulates it. The reviewer suggests that the authors may consider changing it to "simulated" or similar. Done.

1. Abstract Page 2122, Line 19 Please consider ": : :shows that the strength of the Atlantic inflow in the Barents Sea is the main source of heat content variability of the in the sea, : : :". It is rewritten.

2. Introduction Page 2123, lines 1-3. "This is because the southern Barents Sea remains open during all year while other Arctic Seas are covered by sea ice that prevents further cooling." The reviewer disagrees with the statement. First, the Siberian shelf seas, the Kara, Laptev and East-Siberian Seas were either partially or completely ice-free in summer during 1979-pres.; Chukchi Sea

and Beaufort Sea were also ice-free. Since 2005 (except 2006) the Arctic Seas were summer ice-free. Please see the NSIDC website for the ice concentration fields ftp://sidads.colorado.edu/pub/DATASETS/nsidc0192_seaice_trends_climo/monthlymeans/br The 1979-2010 multiannual ice concentrations show that most of the Arctic Seas were ice free in summer during this period. Secondly, continental runoff brings very fresh water in the Siberian shelf seas and the Arctic Ocean, increasing upper ocean stratification and limiting upward heat flux from the Atlantic layer towards the sea ice. In contrast, the river runoff in the Barents Sea is less but the Atlantic water dominates the inflow in the sea, as a result the Barents Sea is much less stratified than the rest of the Arctic Ocean. This allows Atlantic water heat reach the ocean surface. Please give a better argument regarding the contribution of the Barents Sea seasonal heat storage and include relevant citations. The text was rewritten, the references were added.

Same page, line 5. What does the “final winter mixed layer depth” mean? Is this the maximum winter ML depth or something else? Please clarify. Please also give references for the cited observed ML depth. The text was rewritten, the references were added.

Same page, line 7. “...and therefore there is a very strong seasonal cycle in heat storage.” Do the authors mean the heat storage in the Barents Sea. Is this a full depth heat content or in the ML or in the top 200 m? Please clarify. Besides, the ML depth can be change not only through convection and heat loss but also by the Ekman pumping. Please comment. Same page, lines 8-10. The sentence “It is natural therefore that any strong interannual anomalies, in either the surface heat loss or the inflow of heat from the North Atlantic, will have a signal in the heat storage and Barents sea temperatures for periods of up to a year at least.” is unclear. Is this that the inter-annual anomalies of the lateral oceanic heat flux convergence and these of the atmospheric heat would change heat content of the Barents Sea on the time timescale longer than a year? If so, it is true but trivial - It follows from averaging the data on the annual time-scale. Please explain in more detail. The text was rewritten. Here we wished only to underline the

importance of the Barents Sea for climatic subsystem of the polar region.

Same page, lines 11-15. The paragraph starting from “A recent analysis of observational data (for nearly a century) in the Barents Sea along a meridian at 33_30’ E between 70_30’ and 72_30’N (Byshev, 2003) showed...” The only reference to the observations in support of the whole study is a book in Russian. This makes difficult for the rest of the oceanographic community accessing the observational results. Moreover, there are no details in the manuscript regarding how “negative correlation between ENSO events and water temperatures in the top 200 m” has been obtained, what is the period of timeseries, degrees of freedom, where test on statistical significance have been applied, etc.. The reviewer suggests either including more accessible citations, or giving more details in the manuscript on the observational datasets, perhaps including plots from Byshev’s monograph. TS data from the Barents Sea are publically available via the BARKODE or WOA. Sorry, it is our fault that in the original version we have indicated the only reference. The references in English were added. Since the observation data and its analysis was described in detail by Byshev and Neiman (2000); Byshev and Lebedev (2000); Byshev et al. (2001) available in English, therefore we do not think that it is worth to increase the paper volume by repeating the published results.

Same page, lines 15-19. The statement “During warm ENSO events atmospheric teleconnections lead to an anticyclonic atmospheric circulation...”. needs references. The next sentence is unclear. Is this "ocean mean temperature" in the top 200-m? Please explain. Besides, it is not clear what the “atmospheric teleconnections” are. Please explain in the text and give appropriate references. The text was rewritten. We gave some references concerning “atmospheric teleconnections” found by numerical models and analysis of the observations. The aim of the paper is not the study of possible types of atmospheric teleconnections between SST anomalies in the tropics and the circulation of the atmosphere in the northern hemisphere therefore we are not going to pay much attention to consider all possible types of atmospheric teleconnections found

and published by many authors.

Same page, line 20. Please give more explanation about ocean re-analysis techniques. Not everybody in the oceanographic community is familiar with this. Some definition and reference are given.

Same page, line 22 and line 29. Please consider “Section 2 described the NEMO...” and “Section 6 provides discussion and conclusions...” Replaced.

2. Model description The description resembles a cut-down extract from the NEMO documentation. It is unnecessary detailed but is also lacking some vital information. In the present study the 1/4 degree model setup is the same as the one used for the DRAKKAR run G70, described in detail by Barnier et al., 2006, Penduff et al. 2007, Lique et a., 2009 and 2010. What is really needed in this section is a description of the model features which are important for the simulations in the Barents Sea (i.e., inflows/outflows, circulation and watermasses properties). For example, a combination of EEN advection scheme, partial bottom cells and free-slip lateral boundary condition improves simulations of the along-shelf flows in the Arctic in ORCA025 (Penduff et al., 2007), whereas ORCA1 needs including the Neptune effect to simulate the topographic currents (Holloway and Wang, 2009). Since both the Atlantic inflow in the Barents Sea and the outflow of the modified Barents water occur via the topographically guided jets, (the Nordkapp Current, the Western Novaya Zemlya Current, and the outflow through the Victoria Trough), the accurate simulation of the dynamics of these currents is essential for the study. Both the ORCA1 and ORCA025 model do not resolves Rossby radius in the Arctic, despite the grid convergence; please comment how this shortcoming affect the analysis. Does GM improve ORCA1 results? The mixed layer model should be described more. TKE has been briefly mentioned but it should be put in the context of the simulations. - Does it perform adequately in the Arctic? Please comment.

The description of well-known NEMO model has been rewritten. Of course, we used

the best model settings that can be seen from indicated in the paper proper references. The aim of the study is demonstrating the model response of the Barents Sea to ENSO events rather finding the best method for the simulation of the Barents Sea state, e.g. with coarse resolution model. We used the configurations that are a result of the detailed tuning and extensive model development work of the DRAKKAR Consortium. Detail description of all model aspects is beyond of the paper scope but can be found in the proper references presented in the section. Some references have been added that have shown in detailed studies that the ORCA025 model at performs very well in the Arctic.

Description of the forcing is good, except for the small correction (see below), however the river runoff scheme is not mentioned, it should be. Done

Please also describe the model bathymetry, most likely the one use in ORCA1 has changes, compare to the ORCA025 version, straits has been widened and deepened, etc.. Please give relevant references for the source of the bathymetries in the both models (e.g., ETOPO and IABCO). Please state temperature and salinity restoring timescales (if any) in the control runs. How different they are in the ORCA1 and ORCA025? The description of model bathymetry was added. To prevent drift in global salinity due to deficiencies in the fresh water forcing, a sea surface salinity relaxation to climatology was applied, with a timescale of 180 days for the top 6 m at the ice-free surface, decreasing to 60 days under ice for ORCA025 and correspondingly 36 and about 7 days for ORCA1. It can be found in the references given by the paper.

The restoring could lead to large differences in the simulations. G70 used a strong restoring to the annual climatological cycle; this can be an issue in analysing the impact of the atmospheric forcing on oceanic variability. Please comment in the text. In spite of G70 run used a stronger restoring to the annual climatological cycle and different forcings applied, the variability of model characteristics (like the top mean 200m temperature averaged over the Barents Sea, the Atlantic inflow, etc.) are in a good agreement with model output from UR025.3. However, there is difference between

mean values. The top mean 200m temperature in the Barent Sea from G70 is about 1C higher than that from UR025.3and the Atlantic volume inflow from G70 is about 10% lower than from UR025.3. We have added some comments in the text.

Page 2124, lines 9 and 11. It should be : “linear free surface” Please consider: “...tripolar “ORCA” grid (the poles are in Canadian Arctic Archipelago, Northern Siberia and at the Geographical Southern Pole)...”. Done

Same page, line 13. Please describe ORCA1 and ORCA025 horizontal resolution in the same detail. Presently there is no a such description for ORCA1. The authors could say something like: “The model configurations are (i) a global 1_ resolution ORCA1 and (ii) a global 1/4_ resolution (ORCA025). Both the models are configured on the same (except for the resolution) horizontal C-grid. ORCA025 has a horizontal resolution of 27.75 km at the equator and of 13.8km at 60_N. North of 60_N the resolution increases due to the grid convergence, resulting in the resolution of 6-12 km in zonal and of _3 km in meridional directions in the Arctic Ocean.” ORCA1 grid is four-times coarser than ORCA025 with a tropical refinement to 1/3_ in the meridional direction (Reference).” The text was rewritten. The proper references are given.

The same paragraph: The ORCA1 configuration has NOT being developed in the DRAKKAR, only the ORCA025 has been. According to DRAKKAR document (http://www.drakkar-ocean.eu/general-presentation/ Drakkar_Lefe_2009_final.pdf) it was

Same page, line 21. “...using a one-equation turbulent kinetic energy scheme..” what does “one equation” mean here? Same page, line 26. Air to sea and air to ice fluxes are NOT calculated as the 6-hourlyfields, but every coupling timestep. Please correct. The text was rewritten.

3. Description of numerical experiments Page 2125, line 7. Does “cold-start” mean ocean and sea ice are initially at rest? Please rephrase. Done

Same page, lines 13-17. There is an apparent contradiction between Section 3 and Table1 in describing UR025.3; the former says the initial conditions came from G70, whereas the latter states they came from “EN3 in situ data assimilation experiment”. Please check which one is right. Same page, lines 17 and 19. Consider: “The forcing blends...” Consider: “...from 1989-2008 also used hydrographic data...” The text and Table 1 were corrected: “The assimilation run at $\frac{1}{4}$o resolution is designated UR025.3, and covers the period 1989-2008, also using ERAInterim forcing, but initiated from a previous ocean reanalysis from the same model which ended in December 2004, see Haines et al (2012) for details.”

Page 2126, lines 4-7. Please re-write the sentence: “The assimilation increments are determined...”, it is incomprehensible in the present form. It was rewritten.

4. Interannual variation in the Barents Sea during ENSO events Section heading: “Interannual variation” of what, temperature? It is worth changing the heading. Done

Page 2126, lines 4-7. Same page, lines 25-26 and also Figure 1. “: : :and the NINO3 index (using the temperature scale).” – this is confusing. Please plot a separate scale for the index and correct the text accordingly. Please describe NINO3 index and make relevant citations. Not all readers will be familiar with it. If the authors wish their paper to reach a wide scientific community the terminology needs to be explained. The separate scale will be the same as the temperature scale, so to overload the figure does not make any sense. The definition of NINO3 index was given.

Page 2127, lines 1-5. “There are three strong ENSO events: a warm event in 1997–1998 and two cold events in 1999–2000 and 2007.” – is this according to the NINO3? Yes, according to NINO3 index for 1989-2008 period.

Does a positive NINO3 index corresponds to the warm ENSO, and a negative to the cold ENSO? Yes.

Please explain in the text. “: : :the annual mean model temperatures in the Barents

Sea: : :" - Is this top 200-m mean ocean temperature? Please make an explanation in the text. Yes. The correction is made.

Same page and paragraph. There seems to be a correlation between sea ice volumes and ocean temperatures almost without a time lag. However the relationship between NINO3 index and model ocean temperature is less clear. One could argue for example, that instead of being delayed by approximately one year the ocean temperature signal precedes the NINO3 index by about two years and is positively correlated to ENSO. There is more analysis of the correlations further in the manuscript, so please make a reference to support the statement in the current paragraph. The text was rewritten.

Same page, lines 15-17. "Both the 1_ models CTL1 (3.8 Sv) and ASSIM1 (3.9 Sv) also overestimate the volume inflow into the Barents Sea compared to inverse model results (3.5 Sv) (Tsubouchi et al., 2011) which probably explains the higher Barents Sea temperatures." – the statement is interesting, but unsupported. Apparently, on the next page there is a discussion that volume flow anomalies dominate heat flux variations. Please make a reference to the analysis. Sentences have been rewritten. This sentence is accompanied by "The higher model resolution in UR025.3 produces the best estimation of the Barents Sea inflow (3.4 Sv) and therefore has more realistic upper ocean temperatures" that supports the conclusion from the previous one.

Same page, lines 20-23. "The variability however does not substantially depend on model resolution : : : and we therefore assume the mechanisms of variability are not sensitive to resolution." – the variability based on what, monthly means, annual? There is a gap in logic here. If the authors used monthly means to calculated STD, it will include seasonal cycle and, as the seasonal change is mostly governed by the atmospheric forcing, one would expect it to be the same in the ORCA1 and ORC025 runs. It is possible to calculate in the both models variability in the oceanic heat convergence and in the atmospheric heat flux and then make a conclusion. Please support the statement with some evidence. In this analysis we are interested by long period variability (seasonal time scale at least) therefore we have analysed monthly and annual data.

It is clearly that at these time scales seasonal changes governed by the atmospheric forcing are the main factors of such variability. The text was corrected.

Same page, lines 24-26. The sentence is unclear: either JFM 1989-2008 mean SLP is plotted or JFM for 1998 and for 2008 are. Which ones? The figure caption says these are JFM for 1998 and 2008. Please explain. Please consider changing as: "Figure 2 shows mean January-February-March (JFM) sea level pressure anomalies in 1998 (warm ENSO) and in 2008 (cold ENSO) from the ERA-Interim". Done.

Please state here and in the figure caption the sign of the net heat flux. Is it positive in the ocean? Please explain large negative winter anomalies of the surface heat flux (more cooling of the ocean in 2000?) in the northern and northeastern Barents Sea; these are seems to be due to the ice edge displacement. Done. Yes, the heat loss in the northern and northeastern Barents Sea is due to the ice edge displacement: less ice during 2000.

Same page, lines 26-29. "During JFM 1998 global atmospheric teleconnections lead to higher atmospheric pressure (Fig. 2a), and hence lower air temperatures over the Barents Sea, while during JFM 2000 lower atmospheric pressure (Fig. 2b), lead to warmer air temperatures over the Barents Sea." Please include details and references how teleconnections resulted in the anomalies in the atmospheric pressure and temperature in the Barents Sea. Presently this is not clear in the manuscript. As was above mentioned, the aim of the paper is not reviewing of possible types of atmospheric teleconnections between SST anomalies in the tropics and the circulation of the atmosphere in the northern hemisphere. Besides, so far these teleconnections have not been described and it can be special issue for further study.

Page 2128, from line 3. It is very difficult to see the ocean velocity vectors in ORCA025 in the Figure 3. The reviewer suggests plotting vector differences between 2000 and 1998 instead of vector anomalies. In the ORCA025 plots please extend the left border farther west to include the Barents Sea Opening in full. Instead of 2000 vector anoma-

lies the vector differences between 2000 and 1998 is presented now. We left 1998 vector anomalies to see the difference between model velocity fields due to different resolutions. We deliberately limited the left border in the ORCA025 plots since the velocity values to the west of 2E substantially larger than to the east and the figure with the inclusion of the Barents Sea Opening is too messy.

Same page, line 14 and throughout the text. Please explain (perhaps in Section 3) how oceanic heat fluxes have been computed. For the monthly mean and annual mean values averages of UT and VT products should be used rather than products of these averages. Please also give a value of the reference temperature. Yes, the monthly mean and annual mean values averages of UT and VT products have been used. The text was added: "The monthly mean oceanic heat fluxes were calculated by means of averaging model ocean heat fluxes calculated from 5 day mean model fields of the product of the velocity components by the temperature using reference temperature 0oC.".

5. Heat Budget variability Page 2129, line 1. "..red, peaking in June: : :" – red curve in Figure 5 is the net heat flux, not short wave radiaiton. Please correct. The text was corrected.

Same page, lines 2-4. "The Barents Sea total heat content (green, the top 200m mean temperature) shows every sign of being controlled primarily by the surface shortwave cycle, being 90_out of phase with the surface forcing." What does it mean, "90_out of phase"? Please explain. This is standard expression. Here it means there is phase shift between these 2 curves equaled to 3 months.

Page 2130, lines 9-11. "It can also be seen that strong ENSO events (blue dashed) are negatively correlated with the Barents Sea inflow." – the reviewer disagrees with this statement. The NINO3 and inflow timeseries do not appear correlated. Please cite correlations with levels of significance. The text was corrected: "... strong ENSO events (blue dashed) are negatively correlated with the Barents Sea inflow (with coefficient of

-0.6 and significant with a probability of 95% determined through the effective number of degrees of freedom following Bretherton et al. (1999))."

Page 2131, lines 1-5. "Though Fig. 8 shows some reverse correspondence between major number of peaks and troughs of NINO-index with ones of the temperature curve, however only for 3 of 7 strong ENSO events (1 warm: 1982 and 2 cold: 1973 and 1984, when the values of NINO3-index deviate more than 1 standard deviation) a negative correlation with the Barents Sea temperature is observed in the same year." – this is a long and unclear sentence, please revisit it. Please list the correlations between NINO and the Barents Sea temperatures with levels of significance. The text was corrected.

Page 2132, lines 7-8. "Figure 9a shows 1989–2008 correlations of zonally averaged monthly sea level pressure (SLP)" – is this SLP from the ERA-Interim? Please explain in the text. The text was corrected.

The whole paragraph (lines 1-23) is very descriptive; it does not help the reader understanding how the ENSO events may affect the Barents Sea. Please consider revising it and including references, currently there are none. The same concerns the next paragraph (from line 24 and also on page 2133), is does not explain relationship between the ENSO and the variability in the Barents Sea. Please consider clarifying it. Page 2133, lines 10-15. "Since the interaction between the tropics and high latitudes depends on the stochastic processes, which always occur during the interaction between the atmosphere and the ocean, therefore it is difficult to reveal a definite link between the low and high latitudes immediately: some interaction delay between these latitudes can be due to the strength of current and previous ENSO events." – it is a very long and a rather unclear statement. What are "stochastic processes"? What is "interaction delay"? Which current is meant here? Please clarify. The text was removed.

Same page, lines 23-24. "The anticorrelations are clear to see and in particular in Fig.10b shows the stronger winds blowing inflow towards the Barents Sea." – the sentence does not seem to be right. Please rewrite it. Besides, the figure does not show ":

: :winds blowing inflow towards the Barents Sea", it only shows a negative correlation between SLP in the Greenland Sea and Barents Sea inflow, as well as high correlations with the SLP in the other parts of the world. The reviewer is uncertain about the high correlations between the Barents Sea inflow and SLP over land, but perhaps the authors can explain these. The text was rewritten for clarity. This is standard approach: Fig. 9b shows high negative (positive) correlation between SLP over the Greenland/ Spitsbergen (Europe) and the Barents Sea inflow, i.e. the bigger the SLP difference between Europe and Spitsbergen, the higher the Barents Sea inflow. The SLP difference between Europe and Spitsbergen together with the effect of the Earth's rotation defines "winds blowing towards the Barents Sea". Hence, the bigger the SLP difference between Europe and Spitsbergen, the stronger winds blowing towards the Barents Sea.

6. Summary and Discussion Page 2135, lines 13-15. "ORCA1 model with coarse resolution (experiments CTL1 and ASSIM1) overestimates the annual Barents Sea inflow about 0.5 Sv (due to not adequate resolution of Faeroe-Scotland channel)." – how did the authors come to this conclusion? There is nothing in the text to prove it. Since it is not important for the presentation of our main results we have removed this piece of the text.

Same page, lines 20 and 21: TW not "TWt". Corrected

Page 2136, line 11-14. The first sentence ": : :and atmospheric depression (higher sea level pressure) over theWestern Europe are settled." - What does "settled" mean here? Here it means the same as on page 2122 and 2123: we have replaced this word by "developed".

The next sentence "These changes in the atmospheric pressure substantially influence the westerly winds in the North Atlantic that in turn change the Barents Sea inflow." Is vague. Please be more specific. The text was rewritten.

Table 1. The description of the Exp. 3 is lengthy. Please consider changing as: "Control

1/4_NEMO simulation forced with ERAInterim atmospheric forcing. The initial ocean and se ice states are taken from G70 run, please text for details." Corrected.

Figures. Caption for Figure 1. It is either "beginning" or "onset" of the ENSO events. Please correct. Corrected.

Please plot a separate scale for the NINO3 index. The separate scale will be the same as the temperature scale, so to overload the figure does not make any sense.

Please explain what the red curve in the panel (b) is – if these are the geographical boundaries of the Barents Sea, why the curve is so wiggly? Red line in the panel (b) is the boundaries of the Barents Sea that were used for calculation of the mean temperature. It follows the land mask in the coast region and becomes wiggly.

Please explain in the caption that this panel also shows bathymetry in metres and include units for the colourbar. Done.

Figure 2 and the caption the figure. Please check consistency between the text and the figure caption. For readers' convenience, please consider either plotting the same regions for SLP and heat flux anomalies or marking the common area on the SLP plots. The text and the figure caption are checked. Since the space atmospheric and ocean scales are different, therefore we have shown bigger area for SLP. We do not think that presenting different scales on figures can impact the readers' convenience, on the contrary it shows global picture that can lead the readers to new thoughts and new studies. Any reader (who will read the paper) knows where the Barents Sea on the global map is.

Please state the sign of the net heat flux here and in the text. Is it positive in the ocean? Done.

Figure 3. Panels for wind stress, ocean velocities from ORCA1 and ORCA025 – all show different area. Please make area the same. We do not think that presenting slightly different areas on figures can impact the result presentation. See also the

above answer.

Ocean velocity vectors are two small to see. Please also consider plotting vector differences between 2000 and 1998 rather than anomalies. We have added these figures.

Caption: please consider changing as: "Winter (JFM) anomalies of wind stress over the North Atlantic and the Barents Sea (a, b) and these of ocean velocity, averaged for the top 200 m in the Barents Sea; (a, c, e) show anomaly in 1998 after warm ENSO event and (b, d, f) show the one in 2000, after cold ENSO event, respectively; (c, d) – is the experiment CTL1; (e, f) – is the UR025.3 run. All anomalies are taken relative to the 1989–2008 mean". Corrected.

Figure 5. Caption. There are no square brackets. Please consider change: ": : :(to have a zero mean and a standard deviation of one, mean and std of the original time-series are given) : : :1_C isotherm (solid blue line, mean and std are 267_79 m): : :". Replaced.

Figure 8, Caption. It should be: ": : :subsequent year mean of the Barents Sea heat inflow (dashed red),.." The caption was corrected.

Figure 9. Explain axes and include all necessary units in the panels (a) and (b). Done.

Figure 10. Caption. The regions are marked by black crosses in the Figure 10 a,b not 9a,b. Corrected. The authors would like to thank anonymous reviewer for very useful comments.

V. Byshev (Referee) . . .However horizontal resolution even 10 km is too rough for numerical modelling of hydrodynamical processes in these latitudes becourse of very small value of radious of Rosby Deformation. It is well known fact that the parametrisation of deep convection remains the serious problem in the numerical modelling of ocean circulation.. According the author's model the vertical convection in the Barents Sea attains only to 200 m. But it was established by the observations in [Byshev et al. , 2001; Byshev et al., 2002] that in winter 1997-1998 vertical convection in the Barents

Sea have developed up to the bottom of the Sea (more then 250 m).

All references concerning this issue are given, which demonstrate that the ORCA025 model at least performs very well in the Arctic. We could not find in our text that the model vertical convection in the Barents Sea attains only to 200 m. We only consider the heat content of the Barents Sea in the top 200m. The model shows that during winter of 1997-1999 period the temperature in the basin under study was very low and the minimum achieved the depth $\sim$300m instead of usual $\sim$100m.

Southern Oscillation was discovered by Leighly [Leighly,1933], who proposed to use the differences of the sea surface pressure between Tahiti and Darwin as the index of this oscillation. Bjerknes [Bjerknes, 1966, 1969] was one of the first scientists who paid attention to the response of the atmosphere in the extratropical latitudes to the large-scale positive anomaly of the surface water temperature in the tropical zone of the Pacific Ocean. In a new version of the paper we did not concern the possible mechanism of the Southern Oscillation.

On my view in the reviewing work was not be taken into account some important papers, concerning the numerical modelling of the circulation in the Barents Sea [Semenov, Chvelev. 1996; Sidorova, Shcherbinin.2009,2011]. They recieved that in the period of El-Niño the inflow of water from North Atlantic in the Barents Sea is rather small, but there is an intensive inflow of cold waters with low salinity from central Arctic ocean and from Kara Sea. These references were added.

Some mistakes are in the references on the pages 2125 and 2126. Paper of Brodeau et al. was published in 2010, but not in 2009. Corrected.

The authors would like to thank Vladimir Byshev for very useful comments.

Interactive comment on Ocean Sci. Discuss., 9, 2121, 2012.

Reviewer #1:
The main shortcomings of the manuscript are a weak support of the model results by the observations and lack of a convincing explanation how the link between ENSO events and ocean temperature in the Barents Sea operates. On the other hand, the impact of the local forcing in the Barents Sea is clearly described and is an asset of the manuscript. The atmospheric teleconnections, frequently referred to in the manuscript, are not well explained. The study uses a forced OGCM with a strong restoring to climatology or with assimilation. This approach constrains the ocean model and does not allow it following freely the atmospheric forcing. Yet, the authors put an emphasis on the role of the atmosphere in ocean variability. A discussion on how the model setup affects the analysis is required.

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Specific comments
Title
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It is rewritten.

2. Introduction
Page 2123, lines 1-3. "This is because the southern Barents Sea remains open during all year while other Arctic Seas are covered by sea ice that prevents further cooling." The reviewer disagrees with the statement. First, the Siberian shelf seas, the Kara, Laptev and East-Siberian Seas were either partially or completely ice-free in summer during 1979-pres.; Chukchi Sea and Beaufort Sea were also ice-free. Since 2005 (except 2006) the Arctic Seas were summer ice-free. Please see the NSIDC website for the ice concentration fields ftp://sidads.colorado.edu/pub/DATASETS/nsidc0192_seaice_trends_climo/monthlymeans/browse/. The 1979-2010 multiannual ice concentrations show that most of the Arctic Seas were ice free in summer during this period. Secondly, continental runoff brings very fresh water in the Siberian shelf seas and the Arctic Ocean, increasing upper ocean stratification and limiting upward heat flux from the Atlantic layer towards the sea ice. In contrast, the river runoff in the Barents Sea is less but the Atlantic water dominates the inflow in the sea, as a result the Barents Sea is much less stratified than the rest of the Arctic Ocean. This allows Atlantic water heat reach the ocean surface.

Fig. 1.