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Ocean Science Discussions

Interactive comment on "Barents Sea heat – transport, storage and surface fluxes" *by* L. H. Smedsrud et al.

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We would like to thank for the time, effort, and interest by Anonymous Referee #1 to review our work. Most of the suggestions on the 7 online pages have been followed. The few remaining places the text has been re-phrased to clarify the issue raised. We now hope that the new version has explained properly the many assumptions used to get the column model set up for the Barents Sea, and that the paper has sufficient quality for the Ocean Science journal. Original text from the reviewer is cited as Rev 1: Our response is cited as Reply:

Rev 1 Summary: This paper attempts to provide an updated synthesis of the mean state of the Barents Sea including budgets for the heat and freshwater. The synthesis is used to establish the forcing for a 1-D column model, in which the Barents Sea is represented in two simplified boxes. The vertical profiles in the two model boxes is initialized as the area mean of an impressive number of profiles. The model is forced by monthly ocean transport and by area averaged monthly mean atmospheric parameters derived from climatologies. This relatively simple model is used to resemble the mean state of the Barents Sea and to explore the sensitivity of the region to variation in ocean transport and surface fluxes. I find the paper very interesting, in particular the sensitivity studies, and agree with the authors that the applied model may be a useful tool to increase the understanding of the sensitivity of the region. However, using a simple model implies many assumptions, which in turn raise several questions to the realism of the model. I do not find that many of these questions are properly addressed in the paper and in addition I have some remarks to the budget synthesis. Based on this, I am sorry to say that I can not recommend publication of this manuscript in its present form.

Reply: We think that all the questions have been answered in a proper manner now. The issues raised have been discussed and our view explained below the original questions below.

Rev1: The budgets: For the volume and heat budgets the discussion is mainly on the AW-flow through the BSO, which is, as mentioned, the best measured section discussed in the paper. However, some information is available for the other water masses entering or leaving the Barents Sea, both on its characteristics and its variability, which I would like to be included in the synthesis.

Rev1: In the discussion of the AW-flow through the BSO I miss the results by O'Dyver et al. (2001) to be mentioned, in addition to recent model estimates (e.g. the ROMS model mentioned in Gammelsrød et al (2009), Maslowski (2004), and Zhang and Zhang (2001), which already are included in the manuscript), and also the NOASIM and MICOM models given in Drange et al (2005).

Reply: We agree. Proper reference has now been given for more of the 3D models applied to the Barents Sea. Our model approach is dependant on forcing at the boundaries, and 3 D models are a valuable source of these boundary conditions, in addition to the (few) moorings available.

Rev 1: It would strengthen the synthesis, if it could result in a heat budget based on the metohology suggested by Schauer and Beszcynska-Moller (2009), rather a budget based on the traditional (uncorrect) method, where the heat flux through a section is calculated relative to an arbitrary reference temperature. (see additional comments related to the model forcing later)

Reply: Our heat budget is fully in line with the Montgomery (1974) demand that the volume budget is closed. This is now stated in the volume budget section, before the heat budget is discussed. We now also clearly state why the 0°C reference is a good choice for the Barents Sea, and that it is the Âńmean temperatureÂż of the outflow water. We also now write that the heat transport is dependant on the outflow temperature, as much as it is on the inflow temperature. There is some data to support that the outflow temperature has changed, and we also discuss this now. The new Âństream tubeÂż model of Schauer and Beszcynska-Moller (2009) has not been applied. It should not change the estimates significantly as the volume budget we have is closed, and the observations of the outflow region is so few.

Rev 1: Column modeling: I have to admit, that I am generally very skeptical to the concept of using a column model for an ocean, where the horizontal dynamics are important for the circulation (e.g. Pfirman et al, 1994; Schauer et al, 2002), - but this may be a matter of opinion. However, as a minimum I would expect an argumentation, which justify the choice of the actual model for the area (beyond that the model is computational cheap and quick, P. 1454, l.'s 19-20).

Reply: Point taken. The remark has been deleted, and a section has been added to the introduction stating why the column model is applied. Limitations (the internal horizontal gradients) and benefits (evaluation of the measured heat content and observationally based boundary conditions) of our approach is also included.

Rev 1: Below are some issues, which I find questionable or not properly addressed: In the introduction, p 1440, l. 24-25, it is stated that the ’inflow of Atlantic Water...... is higher during winter related to the stronger winds’, but on p. 1448, l. 7-11, describing the model forcing, it is said that ’stronger wind forcing increases vertical heat fluxes and (increases) turbulent entrainment, but is not driving the ocean transport’. As I understand the physics in the applied model, then the contribution driven directly by the wind is undertaking by increased vertical diffusion. I find this procedure questionable.

Reply: The model forcing sentences have been re-written to; ÂżThe model ocean volume transport is prescribed as a boundary condition, and does not use the wind forcing in any way. Âń In nature it is the winds that drives the increased winter inflow. In the model this increased inflow during winter is prescribed, and there is no increased vertical diffusion opposing the stronger inflow.

Rev1: P. 1440, l.’s 11-15 describes a fairly large gradient in the sea surface temperature from the BSO to the northern part of the Barents Sea, and assuming that the water in the northern Barents Sea is close to freezing also during summer, this gradient may be even larger according to the given summer temperatures in the BSO.

Rev1: On P 1447, l. 14, it is stated that ’the air temperatures decrease strongly northwards in the Barents Sea’ . Using horizontally averaged values masks these significant gradients, as pointed out by the authors on p. 1446, l.’s 19-20 and also noted on P 1454, l.’s 15-18. I find the use of averaged parameters as input in the sea surface heat budget calculations questionable for an area with relatively large gradients in the input parameters, and I speculate if this can explain that the found balance between the surface heat budget terms is different from the balance obtained by Zhang and Zhang (2001) and Simonsen and Haugan (1996) (P. 1454, l.’s 9-11)

Reply: The reviewer is in his full right to speculate here. The balance obtained by the earlier work does resolve the horizontal gradients, and we do not. We aim to describe

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the mean steady state balance, and it is evaluated by the large number of CTD stations. The 3D models evaluate their results in the BSO, and at the surface (SST's and ice cover), but this often reflects the boundary conditions and forcing more than the model itself. A typical example is the ROMS model applied by Budgell (2005) that generally show good agreement for the variability, but still has a constant offset of 0.6 °C for the Kola section (Budgell 2005, Figure 11). We think our approach is valid and has some benefits, that do support the new mean heat loss in Table 2. The fact that our values are often in the middle of Zhang and Zhang (2001) and Simonsen and Haugan (1996), and is consistent with the measured mean heat content suggest that they are a good estimates.

Rev1: About the model it is stated that it 'calculates the horizontally averaged ice thicknesses' (P 1445, l.'s 20-21) and have options for 'ice export' (P 1445, l. 23). The ice cover isolates the underlying ocean from a cooling atmosphere as stated in the paper, but since a large part of the ocean heat loss are through openings in the ice (e.g. Martin and Cavalieri, 1989; Ivanov and Shapiro, 2005; Harms et al., 2005; Gammelsrød et al. 2009), and since each of the boxes represents large areas where a total ice coverage is rare, I would like to see a description of how the ice extent is represented in the model. (From the discussion part of the paper I understand that the 'cooling area' may be related to the ice extent, but still I miss a description)

Reply: Agreed. A description of the sea-ice model used is missing. It was not included because the south box produces very little sea ice, and we did not discuss the northern box results to a large extent. A few sentence have been added now; "One sea ice class is used here, meaning that if first year sea ice develops during winter it has the same thickness inside each box. Very little observations of Barents sea ice thickness is available, and no attempt has been done to apply ridging processes, and an ice thickness distribution. A more detailed model description may be found in Björk (1997)."

Rev1: I note that import of ice from the Arctic is described in the discussion of the budgets. This ice does also affect the sea surface heat budget in the Barents Sea (e.g. Gerdes et al., 2003), and melting of the imported ice influence the stability of the water column in a couple years after an import event (e. g. Budgell, 2005; Schrum et al, 2005), and those signals from the imported ice are very likely included in the profile data used for initialization and comparison. I would prefer to see the import of ice included in the model, or at least be included in the discussion of the model results.

Reply: The ice import is included, at least partly. It is included as a part of the fresh water flux (Page 144 line 11) stating V=5.5 mSv based on Pavlov et al (2004). This is used in the model freshwater boundary forcing. It was also mentioned in the heat budget calculation as -1.3 TW (Page 1443, line 17), but the contribution was too small to include it in light of the other uncertainties. The year to year variability is large, and anything below 5 TW I guess is not counting for the mean situation. We have thus not done any changes here.

Rev1: The model is forced by the monthly mean flow of AW through the BSO with an additional 1.3 Sv representing the other inflows with a mean temperature of 5.6 C, and the volume budget is balanced by a simlar outflow, but with a temperature of 0 C, - if I have got this right. Reply: Yes - correct !

Rev1: First I miss a more comprehensive justification of this simplification, both because of the different temperatures of the outflow waters as pointed out by Dr. Shauer in her comment, and also because I miss a discussion of the seasonal variation in the outflowing volume (e.g. Fig. 4 in Schauer et al (2002) and Fig. 8 in Gammelsrød et al (2009)) and its relation to the variation in the BSO inflow. Second, the freshwater forcing is mimicking the vertical salinity (P 1448, l. 23-24) , but I have not found the implementation of the vertical distribution of the 'heat' and volume forcing mentioned.

Reply: Agreed. We have now added a note on the monthly observations of outflow, which are similar to the observations of inflow, and confirm that the main inflow is in the BSO, and the main exit is in the BSX; "The main outflow of $\sim$ 2.0 Sv from the Barents Sea takes place between Novaya Zemlya and Frans Josef Land (Gammelsrød

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et al 2009), termed the Barents Sea eXit (BSX, Fig. 1). The yearly cycle is similar to that in the BSO, with the largest outflow during winter above 2.0Sv (Schauer et al 2002). We have also added a description on the vertical implementation of heat in the model forcing section; " The advected heat is evenly distributed vertically, reflecting the homogeneous layer of Atlantic water in the BSO (Ingvaldsen 2004). "

Rev1: Specific comments. P. 1439, l. 19-29: I think the definition of units and reference values should be moved to the representative paragraphs.

Reply: The section has been moved to the representative paragraphs as suggested.

Rev1: P 1439, l. 19 & 24: misprints in definition of the units TW and Sv.

Reply: This was caused by the Ocean Science Latex => HTML transition. We will make sure it does not happen again.

Rev1: P 1439, l. 21: The given ratio between heat transport and heat fluxes (pr area unit) includes several unexplained assumptions, - recommend to remove these lines.

Reply: OK. The part with "similar magnitude" between W/mˆ2 and TW has been removed.

Rev1: P 1440, l. 3: I do not agree that 'such a synthesis has not been found elsewhere', but rather that this is an attempt to make an updated synthesis, - as stated by the authors on p. 1460, l. 16.

Reply: Yes - fine. Allthough no classical "synthesis" was found, several papers do discuss the overall budgets, and this has been re-phrased accordingly.

Rev1: p 1440: Barents Sea mean state: I appreciate the short description of the mean state, but since this region experience large annual and year to year changes, I would also expect some comments on the variability in the region.

Reply: This section is just an introduction to the other sections describing the volume, heat and salt budgets. In these we at least include the monthly variability, and some of

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the recent changes since the 1990's. We have therefore added this short sentence ; "The Barents Sea variability is substantial, and will be shortly described in the sections below." There is also a new paragraph on the previous and ongoing 3D model efforts, where the variability is at the centre of attention.

Rev1: On p 1440, l. 24- to p 1441, l. 1, it is stated that 'Inflow of Atlantic Water.... is measured since 1997. New data included here compliments the 1997-2001 series up to 2007'. On p 1442, l. 8 the year span is '1998-2007', and in the caption to Figure 3 (p 1470) it is referred to 'the 1997-2008 data'. I guess the difference in the year spans is due to misprints.

Reply: Thanks. This is all the same 1997-2007 data set. A few months of 2008 was available, but were not included in the end. Corrections made. The inclusion of some months in 1997 increased the Atlantic Water mean to 2.0 Sv.

Rev1: And on P 1442, l. 12 the period is '1965-2005', - is this referring to the same data?

Reply: No. Temperature has been measured back to 1965 (Skagseth et al 2008). Now made clear.

Rev1: P. 1442, l. 12: I guess a mistype is responsible for the negative mean temperatures

Reply: This was also caused by the Ocean Science Latex => HTML transition, the $\sim$ symbol should have been there, not the - . We will make sure it does not happen again.

Rev1: P. 1443, l, 8-9 'When comparing this result with model results', - I miss some references here.

Reply: The original Budgell (2005) and Maslowski et al (2004) citations have now been added, and the section re-written for clarity.

Rev1: P 1446, l's 5-10: The albedo and snow model is also explained in some more

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details on p 1447-48.

Reply: Yes. The sentence on snow on page 1446 has been deleted as it was somewhat repeated later.

Rev1: P 1447, l.'s 26-28: Comment: On islands an increased precipitation compared to the surrounding ocean is widely seen due to orographic processes.

Reply: Thanks. Now added.

Rev1: P. 1450, l. 16 and P 1451, l.'s 11-12.: I would prefer to see the area of the two boxes as part of the model description together with a justification of the chosen size of the areas.

Reply: OK. This has been moved to 3.2 Initialisation, with some new text added.

Rev1: P 1450, l.'s. 20-21. As seen on Fig???

Reply: Figure 7 added here.

Rev1: P 1452, l. 14: I miss an justification of the flow entering the northern box, maybe as part of the synthesis section.

Reply: The division into a northern and southern box largely follows the polar front, but we have not discussed this in detail. The movement of the front can only partly be adressed by a column model approach, through the area sensitivity. There is also little current meter data available along the polar front (or winter zero degree isotherm) , and we initially treated the whole Barents Sea as one big box when it comes to ocean transport. We have now changed this and stated clearly that we have an inflow of 3.2 Sv to the southern box, and an inflow of 2.0 Sv to the northern box; "The volume budget used as model forcing is a fixed yearly cycle around a mean of 3.2 Sv for the southern Barents Sea, and 2.0 Sv for the northern Barents Sea". The estimates for the northern box is based on the BSX data, and 3D model results by Maslowski (2004). Results changed only to a small degree for salinity . The heat transport is the major difference,

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73 TW in the mean to the south, and 2 TW to the northern box.

Rev1: P 1451, l. 23. '.. found over large areas of the Arctic Ocean'. Since not discussed otherwise here, I would like to see some references here.

Reply: Agreed ! Rudels et al (1994) and Björk et al (2002) added here.

Rev1: P. 1452, l. 4. 'Model salinity (changed from sensitivity) decreases more than suggested by observations'. Isn't this a little bit odd, since ice inflow from the north is not included, - if I have understood it correctly.

Reply: As mentioned before is the sea ice import included in the salinity budget, but it is diveded evenly over the Barents Sea. The "error" of the 1D model is probably related to the horizontal freshwater distribution. The freshwater tends to remain along the coast. There is, for instance, often a sea ice export close to the Svalbard coast exiting the Barents Sea in the Fram Strait. Such horizontal gradients cannot be re-produced by a column model. This has now been added to the text, in addition to the brine water likely escaping along the bottom.

Rev1: P. 1452, l. 26: Repetition of information already given, - rephrasing needed. Reply: Re phrased.

Rev1: P. 1458: One of the results from the sensitivity runs is that the 'cooling area' may be an important parameter, and this parameter may be related to the ice extent. The obtained surface heat budget match as expected the advective budget determining the ocean forcing of the model. The obtained surface heat budget is compared with one of the budgets in Simonsen and Haugan (1996). One of the main results in that paper was that the atmospheric forcing is very sensitive to the applied heat budget parametrization, which seems to be supported by the numerical model experiments by Budgell (2005) and Harms et al. (2005) and summarized in Gammelsrød et al. (2009). I agree with the authors that finding the exact area may not be the most important result (p. 1458, l.'s 4-13), but in the light of the known sensitivity of the surface heat budget parametrization just mentioned, and my earlier comments on the implementation of the model forcing, I am not confident with the obtained sensitivity results, - although I find them interesting.

Reply: The 1DICE model uses "standard" bulk formulas for the different heat fluxes. No attempt has been done to "tune" these. Our forcing section is rather long and detailed for a good reason; the radiative forcing and air temperatures are important. Most 3D models, and Simonsen and Haugan, used re-analysis to force their fluxes. As we have found they differ with as much as 20 W/mˆ2 in the yearly average (NCEP) or 20 W/mˆ2 during summer (ERA 40). A very likely candidate for the large sensitivity is therefore the forcing, which we to some extent have avoided here using the "synthesized" monthly means in Table 1. It is fair to state that one does not have confidence in our results, but on our side we do not have confidence in the re-analysis, and have done a decent job to work around them.

Rev1: Some additional references:

Reply: Citations to Gerdes 2003, Harms 2005, Budgell 2005, Rudels 1996, and Björk 2002 have been added to the article. The Gammelsrød 2009 and Skagseth 2009 ones have been updated.

Gerdes et al., 2003, GRL, 39(19), doi1980. DOI: 10.1029/2003GL018080.

O'Dwyer , J., et al., 2001, North Atlantic Water in the Barents Sea Opening, 1997 to 1999, Polar Research, p. 209-216.

Harms et al., 2005, JGR, 110, C06002,doi:10.1029/2004JC002559

Pfirman et al, 1994, In Johannesen et al (eds) , Geophysical Monograph Series, 85, AGU, pp. 77-94.

Martin and Cavalieri, 1989; JGR 94(c9) pp. 12725-12738.

Ivanov and Shapiro, 2005; DSR, 52(9), p. 1699-1717. Drange et al. (2005), In Drange

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et al (Eds), Geophysical Monograph Series, 158, AGU, pp. 199-219.

Gammelsrød et al. 2009, J. of Mar. Sys., 75, p. 56-69 (Paper version of Gammelsrød et al., 2008 in the manuscript)

Budgell, W. P., 2005, Numerical simulation of ice-ocean variability in the Barenst Sea region, Ocean Dynamics, vol. 55, 3-4, p. 1616-7228.

Schrum, C., et al, 2005, Modelling air-sea exchange in the Barents Sea by using a coupled regional ice-ocean model. Evaluation of modelling strategies, Meteorologische Zeitschrift, vol. 14, 16, p. 801-808.

Interactive comment on Ocean Sci. Discuss., 6, 1437, 2009.