

Interactive
Comment

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

Anonymous Referee #1

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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

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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.

My general and specific comments are given below:

General comments:

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.

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) .

It would strengthen the synthesis, if it could result in a heat budget based on the methodology suggested by Schauer and Beszczynska-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)

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

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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). Below are some issued, 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.

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. 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)

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

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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)

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.

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 similar outflow, but with a temperature of 0 C, - if I have got this right. 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.

Specific comments.

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

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

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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.

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.

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.

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.

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

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

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

P 1446, l's 5-10: The albedo and snow model is also explained in some more details on p 1447-48.

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

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.

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

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

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.

P. 1452, l. 4. 'Model 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.

P. 1452, l. 26: Repetition of information already given, - rephrasing needed.

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.

Some additional references:

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

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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 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 Barents 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.

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