

Review of the manuscript entitled “Barents Sea Heat – Transport, Storage and Surface Fluxes” by L. H. Smedsrud, R. Ingvaldsen, J. E. Ø. Nilsen, and Ø. Skagseth.

This manuscript presents interesting results synthesizing heat and freshwater budgets for the Barents Sea, in particular providing a new estimate of heat transport by the Norwegian Coastal Current (NCC), and employing a 1-D column model to estimate the Barents Sea heat budgets. Relevant model sensitivity experiments examined impacts of ocean heat transport and sub-basin areas on ocean temperature profiles and their annual cycle, suggesting an interesting transition from current state to a new Arctic state when ocean heat transport/sub-basin area changes over a threshold. The manuscript is generally well organized and written. I would recommend that this paper is acceptable in the *Ocean Science* after a minor revision as suggested below.

Specific minor comments:

1. In line 2 on p.1438, I would suggest to specify whose sensitivity of the Barents Sea, for example, heat budgets, hydrographic properties, sea ice mass balance.
2. In line 4 on p.1438, I would suggest to reword the sentence, for example, to “... are synthesized and are applied to force model simulations that ...”.
3. In line 5 on p.1438, I would suggest to remove “well”.
4. In line 12 on p.1438, I would suggest to remove “the major part of the area” because the southern Barents Sea is also a major part of the area.
5. In line 15 on p.1438, I would suggest to reword the sentence to “... by a heat loss through longwave radiation, removing ...”.
6. In line 13-16 on p.1438, I feel a conflict here. The authors states a significant sea ice production but there is little net surface heat loss. I would suggest the authors to reword or to clarify this statement.
7. In line 18-19 on p.1438, I could not understand the sentence “Despite ... remains robust, ... yearly cycle remains”. I guess the authors may want to say “Corresponding to the large changes of heat budgets in the Barents Sea, ocean temperature adjusted which retains climatological annual cycle”. I suggest the authors to further confirm this.
8. In line 13-15 on p.1439, from my understanding ocean heat transport and advection should be different. The former is defined as “ $v * T$ ”, while the latter is “ $v * \frac{\partial T}{\partial x}$ ”. It would be better to clarify this here and in the following discussions.
9. In line 16 on p.1439, the authors stated “Surface fluxes depend on vertical mixing and stratification”. It would be useful to clarify they are the mixing and stratification in atmosphere or ocean or both.

10. In line 8 on p.1441, it is stated that the Barents Sea area of $1.1 \times 10^6 \text{ km}^2$. This is not consistent with the number given in line 19 on p.1439.
11. In line 9-10 on p.1442, the sentence of “Distributed evenly ...” would cause a confusion. It is not that all heat gain via the Barents Sea Opening is lost at surface. Heat can also be lost by lateral transport via other passages.
12. In line 11-12 on p.1442, I could not understand why the mean inflow temperature is negative. If this is the case, why does the Barents Sea gain a net heat?
13. In line 18 on p.1442 and Figure 3, the authors use a reference temperature of 0°C to calculate heat transport but states here “using the constant cooling of 6.4°C . This causes confusion to me. I would suggest the authors to clarify.
14. In line 21 on p.1443, I think it would be a better estimate of surface heat loss here by using 73TW, rather than that described in line 10 on p.1442.
15. In line 15 on p.1444, I think the unit in “ $V=5.5 \text{ mSv}$ ” is a typo.
16. In line 26-27 on p.1446, the authors state “We have chosen to use the mean of the SRB and Polar ISCCP data”. I would like the authors to provide a brief discussion for a rational. By the way, both SRB and ISCCP are satellite-derived data. Discussions about why they are different would be useful.
17. In line 2-3 on p.1447, the authors state “We use the SRB fluxes as they are close to the mean”. I suggest adding some words to define the “mean” here.
18. In line 11-17 on p.1447, the authors described that the mean of two reanalysis data sets has a warmer bias compared with met station data. Have the authors got a chance to check if this warmer bias exists in each of the reanalysis data?
19. In line 8-9 on p.1448, the authors state that stronger wind forcing “is not driving the ocean transport”. This causes confusion to me. Does this mean wind-driven current does not contribute to transport? It would be good to clarify this.
20. In Section 3.4 and Figure 5, a large difference of the seasonal variation of temperature profile occurred in the Northern Barents Sea. The simulated temperature is too low compared to the observations. This would have a large impact on heat budget estimate. It would be useful to have some discussions on this.
21. In line 1 on p.1452, the authors state “... will be a heat sink ...”. From my consideration, this may depend on season. Why does ocean temperature fairly remains below zero in winter? If it is mainly caused by upward heat loss through sea ice, the ocean would be a heat source, rather than a heat sink, in physics.
22. Finally, I would like to suggest the author to double check English grammar to enhance readability of this paper.