

Interactive
Comment

Interactive comment on “Problems with estimating oceanic heat transport – conceptual remarks for the case of Fram Strait in the Arctic Ocean” by U. Schauer and A. Beszczynska-Möller

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We are very grateful for the very helpful comments of both referees.

Both referees suggest using the salt budget in addition to the temperature to better define or control the stream tube. We agree that the salt budget would be a constraint, although it is not unambiguous since the partner for exchange through the lateral boundaries of the stream tube, the Barents Sea Atlantic Water, has salinities that are not very different from the Fram Strait water. Referee1 points out correctly that we cannot exclude the possibility of a leaking stream tube. Yet we noted this in the discussion. Our point is not to state that the stream tube concept as presented here

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is the optimal way, but rather that with the existing observations it is probably the only way.

But the main reason for not considering the salt budget here is that we do not have appropriate measurements available. While we have temperature observations at all locations in Fig. 3, lower panel, we have salinity observations at very few dots only and of these the quality is sometimes poor. Constraining budgets with the help of salinity is possible with CTD data (Rudels et al., 2008), but CTD data are typically taken only once per year, and there are usually no velocity data with them. We mention the need for measuring constraining parameters now in chapter 6 dealing with consequences for observations

In the following we go through the comments point by point:

Referee 1:

We have improved the writing; at least we hope so.

We clarified the terms heat/temperature transport. We don't use the term temperature transport but only the term heat transport which we put in quotes if the "heat transport" is computed along a wrong method. We state this at the end of the introduction.

The issue about the latent heat gained through sea ice export was indeed missing, and we feel that this was a very important comment. It makes the whole exercise of assessing heat transports from observations in polar oceans even less feasible. We added ice transport in the tables, actually we included even a third table to make the point more clear (if the editor/reviewer feels this not necessary we can skip it). We discussed the implications in chapters 3, 4 and 5. While the referee wrote that it would be OK to exclude the sea ice, but then explicitly to state so, we found that his comment pointed in fact to another severe complication.

We understand the complaint about table 2 and the extensive discussion (actually it is only one paragraph) and we also find it somehow embarrassing. But the motivation

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for this table are endless discussions during conferences and meetings with many colleagues who accept (again only after some discussion) that for absolute heat fluxes a closed volume is necessary but who insist that for temporal comparisons “heat fluxes” through partial sections would be OK.

Ironically, the justification for the necessity of such detailed discussion came right in time: An OSD paper was submitted only some weeks ago, <http://www.ocean-sci-discuss.net/6/1437/2009/osd-6-1437-2009.pdf>. The entire article is based on an erroneous heat transport concept. Fig. 3 shows the temporal variability of “Atlantic heat transport through the Barents Sea Opening” together with the varying volume flux through that same section. Reference temperature is always 0°C. Here we are again. (Another example, is: The North Atlantic inflow to the Arctic Ocean: High-resolution model study, Yevgeny Aksenov, Sheldon Bacon, Andrew C. Coward, A.J. George Nurser. In press in Journal of Marine Systems. One can hardly read as fast as wrong heat flow publications keep popping up!)

We added a phrase in the caption of Figure 4 to make clear that we re-defined the inflow.

Minor comments:

Pg 1010 In 2 rephrased

Pg 1010, In 22: rephrased to include turbulent fluxes by mesoscale eddies.

Pg 1012, In 9: We weakened the phrase. Again, I am (and was) not stating that our results are correct (see the new discussion in chapters 5 and 6), but I think that the probability to get a right result if one starts already with a wrong method is very low.

M. Maltrud, Referee 2:

General comments

We modified the text and tried to better explain which other “stream tubes” end in Fram

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Strait. We are not sure in which other regions this concept can be applied; I guess that the Mediterranean does not need this concept because inflow through the Strait of Gibraltar must be balanced if loss of mass through evaporation can be ignored. But in the Arctic, Nordic Seas, American Mediterranean etc. one might use this concept. For the suggestion to constrain the stream tube by salinity see above.

Specific scientific comments

Abstract changed Page 1010 changed Page 1015 changed Page 1017: We would not mind monthly changes of 1-2 Sv, but the changes are up to 10 Sv, the standard deviation decreasing from 5.6 Sv before 2002 to 2.6 Sv after 2002, because of more instrumentation. Also there remains the argument about water property mismatch that was discussed by Rudels et al., causing trouble with several Sv monthly imbalance.

All technical comments have been taken into account. Thanks for the help.

Interactive comment on Ocean Sci. Discuss., 6, 1007, 2009.

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