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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

Anonymous Referee #1

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I believe this is a worthwhile paper that will merit publication after revision. It is not a particularly significant or original scientific contribution but rather a discussion about an issue which, one would hope, nearly everyone appreciates – or should appreciate upon reflection. That is, if there is a net volume transport through a section, then the stuff sometimes termed "heat transport" is ambiguous to within an arbitrary "reference temperature". Nevertheless the authors make a nice discussion of this problem, with one important oversight to be mentioned below. The authors suggest their own approach, admitting to its arbitrariness. I will suggest they could gain a valued constraint by considering also the implied salt budget.

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Specific comments:

The prose is generally good. However, the authors should ask a native-English writer to go over everything carefully for idiomatic and, sometimes, grammatical improvement. I'll not try to do this here. However, considering the abstract, see "known since long". Then see "temperature transports" .. "across sections" .. "unbalanced volume flow which". To what does "which" refer? Then see "the arising difficulties". At pg 1009, lines 6-7 see "consequences" .. "Consequently". Etc throughout. Let me say again that the prose is generally good. Still it could use some help.

The authors make extensive reference to "temperature transport" (often, but not always, in quotes). They also speak of heat transport, often in an adjacent sentence. It is unclear what distinction the authors are trying to make, especially if (as the authors themselves do) one simply multiplies "temperature transport" by volumetric specific heat capacity to obtain heat transport. One gets an impression the authors may mean something else, possibly using "temperature transport" as a label for "wrong". The paper needs clarity in terms and, where possible, fewer terms.

One key oversight which needs a comment is about sea ice. The authors only mention (once) sea ice in context that ocean heat transport can affect total ice. Unmentioned is the latent heat transport due to sea ice export. As illustrative numbers (not meant to be accurate), if the Arctic exports 0.1 Sv ice with latent heat of fusion 334 J/g/K, isn't that 33 TW? As large as, or larger than, many of the transports in Table 1? Certainly the authors can state that they don't choose to include this (which is OK), but they ought to so state. Effectively they hide it in Focsurf which is defined as ocean / atmosphere heat exchange. These would not be the first authors to define (unawares?) sea ice as part of "atmosphere". However they should add a line to clarify this.

Next please consider salt. The authors have made a valid point that some colleagues may sometimes overlook the role of reference temperature under circumstance of nonzero net volume transport. Such colleagues would make a mistake that deserves

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to be pointed out. However, the authors do not escape the resulting arbitrariness. Their streamtube conceptual model helps frame a rationale for assigning what is effectively a reference temperature. Still the streamtube is inexact as a physical depiction and the authors results retain a degree of arbitrariness (as they duly admit). I very strongly urge the authors to take a further step. Calculate a corresponding salt budget. When that budget fails to close, consider what may be acceptable bounds for the unclosed budget. Consider variations to the streamtube protocol that may bring salt within acceptable bounds. In this way, I hope, further restrict the arbitrariness in heat transport.

I wish to complain about Table 2 and the excessive discussion given to that. An on-looker might simply see that the authors "forgot" to write in 4 TW due to increased Fram outflow. The authors intended (I believe) a straightforward remark which gets lost by adding an entire table and excessive text to make their point. An inline comment on effect of assuming increased Fram inflow with required increased outflow would have sufficed and been more clear.

As a comment, when a reader gets to Fig 4 and sees Fram inflows varying around 6 Sv, the reader may be puzzled about earlier choices of 9 or 10 Sv. It may want to be clear that inflow has been redefined. I am supposing that 9 or 10 Sv is representative of the authors' views about total estimated inflow.

Here are some minor specific comments:

pg 1009, ln 22: When "f" is defined as turbulent fluxes, it is not clear if (or how much) the authors include unresolved mesoscale eddies as "turbulence". Then, after (2), the authors state that they omit lateral boundary transports due to "f" as negligible. Is this meant to include mesoscale eddy transports?

pg 1010, ln 2: "v" is "considered" to have no divergence. But it was already stated at pg 1009, ln 18 and is the basis for (1) that the flow is taken to be incompressible.

pg 1012, ln 9: Previously published estimates which ignored constraints aren't known

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to be "wrong". Within an arbitrariness which remains, also in the authors present treatment, one simply can't tell what is "right" or 'wrong'.

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