

Ocean Sci. Discuss., referee comment RC3  
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## Comment on os-2021-56

Anonymous Referee #3

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Referee comment on "Refined estimates of water transport through the Åland Sea in the Baltic Sea" by Antti Westerlund et al., Ocean Sci. Discuss.,  
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### General comments:

The authors have adapted a high-resolution model, which has already been used in previous studies, to the Åland Sea area in order to investigate in unprecedented detail the water transports between the Åland Sea and the Bothnian Sea in the north and the Baltic Proper in the south. Previous modelling studies as well as observational data lack detail, leaving open questions about, for example, the changing eutrophication status of the Bothnian Sea. This study now aims - at least to my reading - to fill these gaps in understanding (at least partially).

After a clearly structured description of the methods and used data, the results chapter is unfortunately no longer clearly ordered, as the model validation is not limited to the model validation part, but comprises large parts of the rest of the chapter. Accordingly, this chapter unfortunately contains only a little new findings on water exchange in the Åland Sea. In the discussion section that follows, the focus is then on possible mainly model-related reasons for the deviation of the model results from measurements, again on validation against already known findings from older studies and on potential further improvements to the model setup. In conclusion, it does not seem so clear whether the focus of this study is on the water transport through the Åland Sea itself or on the demonstration that the model can reproduce this transport very well.

All in all, the manuscript reads very well to me as a comprehensive model introduction or validation paper, describing a model that works in my opinion very well in the challenging region of the Åland Sea, and is certainly suitable for numerous and diverse follow-up studies in the region, as the authors themselves write in the outlook. In contrast, the analysis of water transport in the Åland Sea is unfortunately somewhat superficial. For example, no reasons for exceptionally occurring northward mean seasonal currents are discussed, and the knowledge gap on the change in the eutrophication status of the Bothnian Sea mentioned in the introduction is not even addressed in the discussion.

I would therefore recommend the authors to carry out more in-depth and somewhat more comprehensive analyses on water transport on the basis of the already available data used for this study (possibly including the meteorological data) or to shift the focus/objective of this study more towards the applicability/quality of the chosen model setup.

specific comments:

line 21: Could the water transports through the Åland Sea explain the changes in eutrophication status of Bothnian Sea? If so, why? Are there changes in transports? It would be good to refer to this again in the discussion section.

line 103: Why is the Smagorinsky parameter "rn\_csmc" the only parameter explicitly mentioned here? What makes the selection of this parameter (as opposed to other parameters) so important? This selection would have to be explained in much more detail. Alternatively, one could list all selected parameters as an appendix or similar and not mention it here at all.

line 109-114: I think thermodynamic formulation means an ice model without ice drift. What effect on water transports can be expected from ice drift (both in mild and in harsher winters) that it is explicitly stated that only mild winters occurred in the modelling period?

line 162/163: Considering the location of Turku and Forsmark, it is easy to imagine that these gauges are not representative for the main study area. However, this should be explained in more detail (e.g. also by plotting the position of the gauges in Fig. 1).

Fig. 2: The time series obviously shows a high correlation, but there also seems to be a relatively large bias, so that I would like to see the numbers of the statistical parameters in addition to the figures. Can this bias be explained by a bias in the boundary conditions only or are there other reasons?

line 193 ff.: In contrast to the thermocline, the halocline is only mentioned in this subchapter in connection with a salinity bias above it. Since the halocline plays a very important role in the further course of the analysis, I would recommend describing the of the modelled halocline quality (location and gradient compared to the observations) in more detail. Due to the large number of individual profiles, this cannot be seen directly from Fig. 3 in my opinion.

line 260f.: Why is it to be expected that the current velocities on the western side are stronger than on the eastern side. I would like some words of explanation.

line 267-269: What is the reason for this special characteristic of the winter 2013/14 (and also of the winter 2016/17)? Were there exceptional wind conditions?

line 326 f.: It would be good to explicitly mention here the issues that have been discovered in previous publications.

line 360: Why did not use a slightly larger model area if you suspect errors in your main study area due to conditions at the (relatively) near open model boundary?

line 363 f.: I would have given the numbers of missing data in earlier chapters and not just in the discussion.

line 371: Same as line 267-269: What is/could be the reason for these two "special" cases?

technical corrections::

line 54: Is station F64 really meant here? According to Figure 1, station F69 (and not F64) is positioned in Lågskär deep, so in this context F69 is probably meant.

line 78: "big depth gradients" should be changed to "large depth gradients"

line 206/407: I would prefer the use of "reasonable" instead of "sensible"

line 234: Figure 6 is mentioned in the text before Figure 5 - I would recommend to change the numbering of these figures.

line 238: The overestimation of the U-component should be between 0.006 and 0.034 (and not 0.34) m s<sup>-1</sup>.

line 372: It must be "direction of the mean seasonal current"