Author’s response to RC3

Westerlund, A., Miettunen, E., Tuomi, L., and Alenius, P.: Refined estimates of water transport through the Åland Sea, Baltic Sea, Ocean Sci. Discuss. [preprint], https://doi.org/10.5194/os-2021-56, in review, 2021

Below, reviewer comments are displayed with a gray background, while author responses are without highlighting.

RC3: Anonymous Referee #3, 07 Aug 2021

> *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.

We would like to thank the referee for taking the time to review the manuscript and helping us improve it. These comments are highly appreciated. We are glad that the reviewer thinks the model works well, and are happy to hear that we were able to convey our understanding of its suitability for further studies.

After carefully considering the feedback given by this referee, we have implemented a number of modifications to the manuscript. We have clarified study focus and how it sits in the larger research plan we have for the coming years. Clearly, the explanation of objectives and scope of the manuscript needed polish. We have modified the introduction to articulate these more clearly.

Also, we added further analysis for meteorological conditions and the exceptionally occurring northward mean seasonal currents that the reviewer mentioned. Furthermore, we extended the analysis in the manuscript to include more discussion of e.g. transport rates. We have also revised how currents were analyzed. A more comprehensive explanation of model validation was included.

We see this study as the first in a series of studies, which ultimately aim to answer the big questions related to water exchange in the target area. We agree with the reviewer that there are still many questions that need answering that were not (fully) addressed in this paper. How these questions are split into different studies is of course somewhat subjective, and we certainly understand if the reviewer has a different point of view regarding this issue.

This particular paper lays groundwork for further studies we envision for the following years. We feel it is important to try to relieve concerns that are generally raised about modelling studies such as this one, and even more so in a region that is as challenging as the Åland Sea. Accordingly, some aspects of the manuscript seem, and in fact are, somewhat technical in nature, and the model validation is somewhat comprehensive. We note that the main point of the paper, as set out in the title, is to provide refined estimates of transports through the Åland Sea, and this is done.

We believe the manuscript has notably improved with the modifications we have implemented based on reviewer suggestions. Hopefully these changes, along with the changes made to answer the specific comment as detailed below, address the concerns raised by the reviewer.

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.

Thank you for this comment, which made us realize we clearly had not articulated clearly enough how this study relates to the important questions raised by the reviewer here. We expect to see an answer to these big questions in future years after further studies have been completed. We have modified the introduction to reflect this fact better.

> 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.

After consideration, we agree that the mention of this particular parameter is unnecessary and have condensed this paragraph accordingly.

> 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?

The most relevant difference for the study at hand is that the thermodynamic module of the ice model handles momentum transfer from the atmosphere differently than the full dynamic ice model. Atmospheric stresses are transferred directly to the ocean, when in reality ice cover should make these stresses smaller. On the other hand, the atmospheric forcing originates from models that typically take sea ice concentration as input. In atmospheric models, ice cover may increase surface roughness and induce a stable boundary layer lowering wind speeds. We mention mildness of winters, because severe ice winters would introduce an additional source of uncertainty to the analysis, the effect of which might be challenging to quantify.

> 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).

We have extended this paragraph so that this is explained better. In this particular case we would prefer not to include these two locations in Fig. 1, as we do not actually use this data and the Figure already has many labels.

> 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?

We noticed that we had not fully explained how the SSH validation had been conducted and which factors are most relevant for the study at hand. We have modified this subsection accordingly. Hopefully it now better explains the relevant factors and why we have not used bias to describe the data.

> 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.

We have expanded this paragraph with details about the halocline in the individual profiles.

> 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.

This fragment at the end of the sentence was likely a reference to earlier results in the literature concerning currents in the Åland Sea, which are not really relevant for this analysis. It seems that we have left this fragment in the submitted manuscript by mistake. We have therefore removed it. Thank you for pointing this out.

> 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?

Yes, the wind conditions differed. We modified the paragraph to reflect this.

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

We agree the wording in this paragraph was a bit unclear. We have rewritten the paragraph, hopefully it is more clear now what improvements were meant.

> 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?

The selection of the model area always involves several factors such as computational requirements of the configuration, physical and topographical features of the area, user requirements, data availability for the chosen area, financial resources, and so on. This in turn always involves some compromise. In this case, the presented configuration is intended to serve as a platform for future studies, which means that a wide array of requirements had to be taken into account. Furthermore, due to the iterative development cycle for these kinds of modelling configurations, any choice also includes some risks, as it is necessary to decide the modelling domain relatively early on in the development cycle and it is not possible to foresee all potential problems that could arise. Usually some problems do arise. Then it is necessary to weigh their impact against the impact, risks and potentially significant cost of further changes to the modelling configuration. In this particular case we determined that the benefits of this kind of a change are relatively small when compared to the risks and costs. For this reason, the model domain was not further expanded at this time.

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

We have now added this same information to the methods section.

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

Please see the answer regarding lines 267-269 above.

> 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.

Yes, the reviewer is absolutely correct. This has been fixed.

> line 78: “big depth gradients” should be changed to “large depth gradients”

Fixed.

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

Fixed.

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

Fixed.

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

This number is no longer present in the revised version.

> line 372: It must be “direction of the mean seasonal current”

Fixed.