Author’s response to RC2

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.

RC2: J. H. Reißmann, 02 Aug 2021

> general comments:

> The authors configured a numerical model to investigate currents and resulting volume transports in the Åland Sea located between the Baltic proper and the Gulf of Bothnia in the northern Baltic Sea. This work is based on former model configurations with the aim to improve the performance of the the simulations in that challenging region with the focus on volume transports in unprecedented detail.

> Large parts of the manuscript are dedicated to model description and validation even outside the validation section in the results chapter. Also the results sections on currents and transports descripe the simulations more or less in comparison with data and general knowlege in large parts and, consequently, do not contain much new facts. However, some new insight about seasonal current patterns, the surface transport and the pathways of deep water is given. Nevertheless, also the discussion focuses on the plausibility of the results, possible causes for deviations from existing findings, and ways to improve the model setup. Motivated by e.g. the importance of nutrient fluxes through the Åland Sea for the eutrophication status of the Gulf of Bothnia, unfortunately, neither implications of the findings on this nor the role of any relevant physical phenomena or driver for them and implications from that are discussed in detail and the manuscript remains more technical in this way. For this reason, the manuscript just has to be seen as a basework to develop modelling skills for the investigated region as the authors note themselves.

> The language is good and clear. The content has some repetions from the introduction throughout the intermediate chapters all the way to the conclusions. The figures are of good quality and easy to understand.

We appreciate the referee for providing this useful review and thank the referee. We are happy to note that the referee has found the language and the figures accessible and that the overall tone of the review is so positive.

These insightful comments from the reviewer made us realize that we had not articulated clearly enough the goals and scope of the manuscript in the introduction. We have made a number of revisions to the manuscript to clarify the objectives and to explain how this study

fits in the larger research plan we have for the coming years. We have also highlighted what new facts were expected.

Furthermore, to address these comments we extended the analysis of wind conditions. We have revised how modelled currents are analyzed. We have extended the discussion of transport rates, for instance. Also, a more comprehensive explanation of model validation was implemented.

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 that this work is basework in a sense that it is an important first step to address the big questions regarding water exchange in the study area. We do not feel this is a bad thing, but rather think that this is necessary for our efforts to provide in our future works the kind of in-depth analysis of water exchange in the target area that is currently missing.

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, as we do. Previous efforts lack the kind of detail that is required for further study of this subject and we feel this study is a step that needs to be taken before further questions can be explored. We agree that the model validation is somewhat more comprehensive than would have been the bare minimum. We feel it is important to discuss the quality of the model results in detail here to build confidence in further studies we have planned.

We hope these changes, along with the ones detailed later, address the concerns raised by the referee. Thank you once again for these comments that help us improve this manuscript. We believe the manuscript has notably improved in this process. Please find our detailed answers to the specific comments made by the reviewer below.

specific comments:

> line 102: I think it would be good to briefly explain the physical meaning of the Samagorinsky parameter.

We note that also referee #3 commented on how this parameter is presented here, and we agree that this section needed further polish. Based on their feedback, as well as this comment, we decided it is better to modify this section to remove the more technical information that is better available elsewhere, and to improve readability for the readers. Hopefully these changes address this concern.

> line 107: Maybe some examples from literature should be given here.

We have added some references.

> line 163: Is there some literature proving this and the opposite for Föglö Degerby?

We are not aware of any specific references for this, but have extended this paragraph to explain the situation better.

> line 188: I agree that timing of the events seems good for which Figure 2 is appropriate. However, absolute differences are in the order of 0.1 m over most of the time shown. This would be much easier to see in a difference plot which is by definition much more appropriate to show differences. For me the following questions arise here: Is 0.1 m difference really quite small or what is to be called small here and why? And what is about the reference levels? Are they compareable at all? Are they determined in a comparable way? Can a target difference of 0 be expected from this point of view? Depending on your answers to these questions, it may be better to skip SSH differences as a mean for validtion, explain why a difference of 0 cannot be expected (mainly different reference levels I would assume) and focus validation on the timing and magnitude of the shown variations. Else, it should be explained why 0.1 m can be considered as small here and what implications these differences have on the dynamics and transports.

Thank you for this comment. It made us understand that we had not explained fully how we had interpreted the SSH comparison, or what could be concluded from it. We have modified this subsection to explain which aspects of the SSH validation are the most relevant for the study at hand and to explain what kind of differences can be expected when comparing model vs. observation. Hopefully the subsection is better now.

> line 198: Similar problem than before with SSH: What is the expected accuracy and why? Which accuracy is needed for the planned investigation to be reasonable?

Thank you. Here too we understood that we had not explained the aim of this comparison in detail. We have modified this subsection to provide context on profile validation. Hopefully it has improved now.

> line 206: Why (see comment on line 198))? Which implications does this have for the investigations?

We do not believe this has significant implications for the time scales investigated in this paper, but thought it would be worthwhile to clearly indicate the limits of the applicability for the configuration. This enables the reader to evaluate the results and their applicability more realistically.

> line 238 and before: How large is the error or accuracy of the numbers given in this paragraph and that one before?

The challenge with this subsection was to find the right balance of accurate description of currents, and easy readability. After reviewing the paragraph in question and discussing this as well as other reviewer comments concerning this section, we came to the conclusion that this paragraph and Fig. 6 (now Fig. 5 in the new revision) were not the most easily accessible way of describing the essential information the reader needs to know about the accuracy of modelled currents. With this in mind, we revised Fig. 6 to present also the RMSE errors and modified this paragraph so that it more clearly describes the situation. Hopefully, taken together, this paragraph and Fig. 6 are now more accessible and relevant.

> Figure 3: It is a bit a pitty that it is not possible for all profiles to unambiguously assign the corresponding NEMO profile to the monitoring profile. I thought about indicating

corresponding profiles from monitoring and model by colour, but for this the needed 12 to 32 different colours for all profiles is definitely too much and not manageable.

We completely agree. In fact, we experimented with a number of different ways of presenting this data before initial submission, including color coding the profiles. In the end, we came to the conclusion that the current approach was the most usable for the reader.

> Figure 5: General problem with these plots: They are not normalised to phase space. But here this seems not to be very relevant for the discussed findings.

Thank you for this comment. If we understood correctly, no specific change was requested here.

> line 266: A good measure to show that would be to present the magnitudes of the vector means normalised to the means of the vector magnitudes. The effect itself is trivial and simply geometric.

We considered including persistency plots in the manuscript, which we assume the reviewer is referencing here. In the end, we decided not to include them and to explain this qualitatively instead, as the number of plots is already high. These plots are quite often incorrectly interpreted and we expect including them would lead to more misunderstandings. We agree that analysis of current persistency would be interesting and are planning it for future studies.

> lines 341/343/348/355: The mesoscale is defined by the first baroclinic Rossby radius. It would be good to give a number of its absolute size in this region. I suspect it to be quite small in this region and the model showing only mesoscale structures. Is the model capable to simulate submesoscale structures reasonably at all?

We are aware of relatively few sources that have investigated the baroclinic Rossby radius in the target area. Fennel et al. (1991) is probably the most commonly used reference. They estimated the Rossby radius for the neighbouring basins. For the Gulf of Finland their estimate was 1.3-2.5 km and for the Baltic Proper around 5 km. Alenius et al. (2003) gave values between 2-4 km in the GoF. Westerlund and Tuomi (2016) gave a rough estimate in the Bothnian Sea of the order of 3-5 km. The technical report by Dargahi and Cvetkovic (2014) gave estimates based on a model calculation with mean values as small as 1 km (SD 0.7 km) in the Åland Sill area, but much higher values (7.3 km, SD 1.5 km) in the Åland Sea proper and even in the Southern Quark (3 km, SD 1.1 km). We have also performed some rough calculations based on CTD data in order to understand better how these values given in the literature relate to reality. In general, we got a range of values that were of the same order but somewhat smaller than Dargahi and Cvetkovic (2014).

Based on the information available, there certainly remains some uncertainty and it is possible that in some coastal and sill areas the model is unable to fully resolve the submesoscale. But, in the main basins of the target area, we do not expect this to be the case. Given that we are not aware of any peer reviewed sources directly answering this question, an absolute value for the radius in the manuscript would likely oversimplify the

issue. Therefore we would prefer to leave it for future work. A closer investigation of this would certainly be well placed in a future work more closely discussing the submesoscale dynamics in the area.

References:
Alenius, P., Nekrasov, A., Myrberg, K., 2003. Variability of the baroclinic Rossby radius in the Gulf of Finland. Cont. Shelf Res. 23 (6), 563–573.
Dargahi, B., & Cvetkovic, V. (2014). Hydrodynamic and Transport Characterization of the Baltic Sea 2000-2009.
https://www.diva-portal.org/smash/get/diva2:897280/FULLTEXT01.pdf
Fennel, W., Seifert, T., & Kayser, B. (1991). Rossby radii and phase speeds in the Baltic Sea. *Continental Shelf Research*, *11*(1), 23-36.

> line 457: This also raises the question how much bigger the model domain should be in relation to the investigated area. Would the model results be more robust against disturbances or biases in the boundary data if the model domain is somewhat larger than the investigated area? Why is the relation between both choosen as it is here?

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 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 fix 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 potential 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.

> technical corrections:

> line 41: Witting (1908) is missing in the references.

Fixed.

> line 45: Granquist (1938) is missing in the references.

Fixed.

> line 54: Is F64 correct? In Figure 1 station F69 is located in Lågskär Deep. If F64 is correct it should be emphasised somehow that a station quite far away was used to draw this conclusion. Suggestion: ‚He also concluded from data from station F64, although it is located in the Åland Sea proper, that at Lågskär Deep …‘

The reviewer is right, this was a typo. Palosuo used data from F69, not F64. This has been fixed.

> line 78: Suggestion: Change ‚big depth gradients‘ to ‚large depth gradients‘.

Fixed.

> lines 206/407/434: Suggestion: Replace ‚sensible‘ by ‚reasonable‘.

Fixed.

> line 268: Suggestion: ‚In the other winters‘ instead of ‚Of the other winters‘

This sentence no longer exists in the revised version.

> line 334: Suggestion: Better write ‚are plausible‘ than ‚were plausible‘. Check used times like this in the whole section! I would suggest to use present time to descripe your findings in general in the whole manuscript, especially also in section 3.

Fixed. We have also checked verb tenses throughout the manuscript and modified them where necessary. We decided to mostly keep using past tense in the results section 3, as it is recommended by several scientific writing manuals. We do recognize that recommendations vary, and that the present tense is often a very good choice for describing the results.

> line 352: Suggestion: Put ‚at our disposal‘ to the end of the sentence for better readability.

Fixed.

> line 361: Suggestion: ‚are‘ instead of ‚to be‘ to make it a correct sentence.

Fixed.

> line 372: Suggestion: ‚direction of the mean seasonal current‘ sounds more correct.

Fixed.

> line 386: Although the Knudsen theorem is commonly quite known, maybe is would be good to add the reference here.

Done.

> line 387: ‚note‘ instead of ‚notes‘

Fixed.

> line 422: Suggestion: Ommit ‚than‘, just ‚less ideal‘ reads better and maybe is more

correct.

Fixed.

> line 426: Suggestion: ‚a minor issue‘ instead ‚less of an issue‘.

Fixed.

> line 485: ‚flows‘ instead ‚flow‘

Fixed.