

Author's reply to referee comments

Miettunen, E., Tuomi, L., Westerlund, A., Kanarik, H., and Myrberg, K.: Transport dynamics in a complex coastal archipelago, EGUsphere [preprint], <https://doi.org/10.5194/egusphere-2023-1547>, 2023.

Below, referee comments are displayed with italic font, highlighted with a grey background. Author replies are without highlighting. When listing the changes in the revised manuscript, line numbers refer to the "Author's tracked changes" file.

RC2, Anonymous Referee #2, 11 Oct 2023

> *General comments:*

> The manuscript is very well structured, of appropriate length, and has nice high-quality figures, in my opinion. Further, the text is very easy to read and contains relatively few grammatical or spelling mistakes. So, from this point of view I'm perfectly happy with the manuscript as it is. However, I do have a couple of specific comments regarding science; see below.

We would like to thank the referee for the encouraging feedback and for the comments on how to improve our work. Please find our replies to the specific comments below.

> *Specific comments:*

> You state in the Model Description (lines 78-80) that open boundary data are from a CMEMS physical reanalysis; can you elaborate on this? Which variables are prescribed at the boundary, and at what time resolution?

> Later in the manuscript you state (lines 273-274) that daily averages are used at the open boundary. Surely, SSH data must be available on hourly resolution? How about currents? I'm a little bit concerned that the barotropic transports are affected by the use of daily averages instead of e.g. hourly data. To address this issue, I propose you validate sealevels against tide gauge data inside the computational domain, preferably one station in the south and one in the north, and perhaps validate the differences between these two (model vs. observations in both cases). If the lack of hourly data at the boundary affects the performance of the model close to the boundaries, the modelled transports may still be okay in the inner domain due to the filtering effect of the archipelago, and you may be able to show this in case you have at least one tide gauge in the inner part of the domain (e.g. Turku?). This may be enough if there are no suitable tide gauges near the northern and southern boundaries. If this is not possible, there will be some lingering doubts about the validity of the model setup with the boundary conditions being used (daily averages), and in extension, the conclusions.

> As an alternative, would it be possible to rerun the model for a short time period using hourly resolution of the open boundary conditions, and compare with daily averages?

Thank you for pointing out that the open boundary data was not clearly enough described in the original manuscript. Sea surface height data at the open boundaries were indeed used at 1 h intervals, and barotropic velocities, temperature and salinity at 24 h intervals. We have clarified this in the model description in the revised manuscript (lines 100–101 in the "Author's tracked changes" file).

Modelled sea surface height was validated in our earlier paper (Westerlund et al., 2022) using tide gauge data from Föglö station which is located in the western Archipelago Sea. This is the only tide gauge station that can be regarded to be representative of the overall sea level variation in the model domain. The other two (Forsmark on the Swedish coast and Turku on the Finnish coast) represent local conditions. The validation showed that the model is able to reproduce the sea level variations quite well, indicating that barotropic dynamics are reliably reproduced.

We now realise that this earlier validation should have been referenced more clearly in this manuscript. In the revised version, we now summarise the SSH validation done in Westerlund et al. (2022) at the beginning of Section 3 (lines 140–143).

> *Technical corrections:*

> *Caption for Figure 7: Please add the info that these are northward volume transports (as I think they are).*

This figure no longer exists in the revised manuscript because it was replaced with another figure (as explained in the next reply).

> *Line 184: It seems to me that the mean transport in the lower layer in the central transect is close to zero..? (green curve, middle panel)*

The mean transport in the lower layer in the central transect is indeed close to zero, but nevertheless slightly northward (not southward, as it was written by mistake in the original manuscript). Northward net transport in the lower layer of the central transect is now more clear in the revised manuscript. To avoid duplication of figures (brought up by the Referee 1) and also to show the differences between the upper and lower layers and different transects more clearly, we have replaced Figs. 7 and 8 with new figures showing the monthly net transports and their northward and southward components separately for the upper and lower layers. The new Fig. 8b in the revised manuscript now shows that while the net transport in the lower layer in the central transect is small, it is nevertheless mostly northward.

> *Lines 253 (and other places): Change "Baltic Proper" -> "Baltic proper" (lower-case "p"). Throughout most of the manuscript you spell with lower-case "p", which is grammatically correct I think (though some authors use capital "P"). However, it is important to be consistent.*

Yes, it should be with lower-case "p". These are corrected in the revised text.

> *Line 313: Please check if this reference should perhaps be "Westerlund et al. (2021), as on github?"*

We have checked this. The reference is to the GitHub repository. The earlier paper is cited elsewhere in the manuscript.