

Review of Salm et al, *Forcing-dependent submesoscale variability and subduction in a coastal sea area (Gulf of Finland, Baltic Sea)*

This paper aims to observe and understand variability in submesoscale processes within the Gulf of Finland, using a combination of in-situ observations from a glider platform taken in 2018 and 2019, and model simulations using GETM. They found that the depth at which submesoscale processes occurred varied between spring and summer, and linked this with changes in stratification and atmospheric forcing. Salm et al also highlight the importance of a coastal current for stimulating submesoscale subduction and explore the connections with topography.

They predominantly use *spice* as a tracer to identify variability in the water column in both the model and observations but describe other parameters that can be used to identify submesoscale variability (such as horizontal buoyancy gradients) for the model results.

The authors present a valuable dataset with glider observations and model simulations, and this is of importance to publish, especially for further understanding of submesoscales in the Baltic Sea. I feel that the presentation and discussion of the results is lacking in structure and some content, and my recommendation is for major revisions.

General remarks

- Throughout, language and grammar need to be double checked. Specifically, the word “the” is often either missing, or unnecessarily added in to sentences.
- The structure and motivation for the paper needs to be clarified. It feels that the glider observations have a minor (only validation) role to play and the bulk of the critical analysis is reliant purely on the model simulations.
- Some of the analysis is very reliant on descriptive or more qualitative assessment of figures, or include reference to parameters not presented. The paper could benefit from identifying some more specific or quantitative metrics (such as those used in several of the papers cited and referred to).

Specific comments

Abstract

- After all other corrections, I would review the abstract to tighten it up, be more specific when referring to dates, review language such as “suggest”, “likely” – can you be more specific? What is the implication and impact from your findings?

Introduction

- Throughout the introduction; double check grammar and sentence structures. Especially in Lines 20-40 there are lots of additional (or missing) “the”s which can make it a bit trickier to read through.
- Lines 20-25; I would include the importance and impact of SMS flows here (you talk about the links with carbon, heat etc later on).
- Line 45; “In contrast to the open ocean...” ; this is not necessarily true, salinity has a significant stratifying role in many parts of the ocean. Please rephrase.

- Line 57: “captured” – remove this word, it doesn’t add to the sentence.
- Line 57: “supports the prevalence” – the number of studies doesn’t necessarily mean there are lots of SMS flows/variability, but they do highlight the importance. Maybe rephrase?
- Line 60: this suggests that since observational data are limited, your glider data is of high importance? What are the limitations of relying on the model simulations? Can you include some review of literature/other modelling papers in the Baltic that support the usage of high-resolution models for this study.
- Line 65: you introduce the gliders here; could this be mentioned earlier when talking about SMS observations, are there benefits to specifically using gliders when trying to observe SMS flows?
- Line 65: what are the dates for the missions (what does spring-summer mean?)
- Line 75: I’d be interested to see a bit more introduction of spice here for readers who aren’t so familiar. Why use spice over other submesoscale parameters also reported in the papers you cite throughout (e.g. horizontal buoyancy gradients, parameterisations of SMS flows through Ekman and Mixed Layer Eddies)?
- Line 86: you refer to a section after section 4/Discussion, but there is no final section.

Data and Methods

- Line 90-94: state explicitly the dates, was it the same transect (if not, why not, how are they different), water depths covered. Maybe a table could be useful for that?
- Line 94: “YOs” is a very specific term, can you find an alternative word to describe the half profile (upcast / downcast)?
- Line 96: Is there a reason to interpolate on time rather than space?
- Figure 1: very hard to see the magenta cross for the wind data
- Line 140: put the equation for spice into a separate line to accentuate it.
- Line 142: citation needed for TEOS-10?
- Line 143: how does the choice of 4km impact results? Did you test with higher/lower scales?
- Line 146: You refer to spice variance throughout the paper, but it is predominantly spice that is plotted in the figures. Is it a qualitative assessment of the figures that results in your analysis of the variance, or do you numerically calculate the variance? If so, can you state that here?
- Line 149/150 (and fig 2): Is it possible to plot spice on the same y-axis? Hard to compare between the panels when T and S are plotted against depth and spice is plotted against sigma.
- Line 155: This reads as a result? Maybe rephrase this to introduce this paragraph.
- Line 161: you calculate several SMS characteristics from the model data. Is it possible to calculate some of these from the glider data too (as is done in papers you cite, such as Thompson, du Plessis et al)
- Line 170: Not sure what you mean by “Central scheme...”
- Line 171: Were these gradients used for N2 as well?
- Line 174: Why do you smooth over 6 hours? Is there a motivation for choosing this timescale?

Results

- Line 183: “over which ten profiles were gathered”: is that per transect of the glider, so in total you have 10x the number of repeats the glider did?
- By averaging the profiles you lose a lot of the benefits and advantages of the glider data – it would be interesting to see the variability in spice (from glider data) over the transect/mission timeline, and to compare that variability to the seasonal variability that the paper is focused on (i.e. is the shorter timescale comparable to the seasonal?).
- Line 187-193: I struggle to understand what magnitude of difference between model and observations is significant. How much can it impact the spice calculations and the final results? From the figures it is clear that the model does not perfectly replicate the observations, what level is acceptable? (e.g. “slight differences” – what does that. Mean?)
- For quite a few of the figures (4, 8, 9, 10 , 11), it could be beneficial to point towards or highlight features that you discuss in the text (e.g. with a small triangle/arrow/line).
- Line 199: “UP” – if you have enough space, I recommend typing this out in full, “upper pycnocline”. Try to remove unnecessary acronyms as much as possible.
- Figure 4: can you interpolate the glider data across time? It is hard to pick up the features you discuss due to the individual profiles.
- Missing more quantified discussion of submesoscale characteristics (e.g. line 245: can you calculate how much the wind changes could impact wind induced SMS flows?). I think this links to the somewhat qualitative or descriptive discussion of the spice tracer; adding quantification where possible will strengthen your conclusions.
- Line 278-285: can you put lines/mark out these events on the figure?
- Line 290: highlight on the figure when these three events occur, help the reader out as much as possible by making it easy to follow your arguments and analysis.
- Line 294: you talk about horizontal buoyancy gradients, but there are none shown in Figures 7 or 8) that you refer to. In addition, you are talking about an event in the second half of May 2018, but you refer to figure 7j,m which shows an event in June.
- From line 335; is this intended to be a new subsection? It is somewhat detached from the previous paragraph (maybe this is just the formatting in the draft version though).
- Paragraph around line 2345: Are there any calculations or analysis that can be done to make this more certain? Or rephrasing of your results (a lot of “probable”, “suggests” etc).
- Figure 11: can you colour in the bathymetry in black or similar? It took me a while to spot the features that you refer to in the text. And is there space along the bottom of the figure to add a panel showing the overall topography so we have context of what the rest of the bathymetry is like away from the <40 m peaks?

Discussion

- Paragraph 367-374: Feels more like a paragraph for the introduction? Or link it to your discussion/results a lot earlier on.
- Line 391: what do you mean by elongated regions? Vertically or horizontally?
- Lines 396-403: the discussion here is comparing to studies that include winter SMS flows / full annual cycles of submesoscale variability (seasonality). The paper presents itself as looking at seasonal variability, but you only look at spring-summer and not the full seasonal cycle. It seems that this is done to only use the model for periods when the observational data is available to validate it, but this results in a compromise on both of

the data sources: we lose the high resolution variability that could be interesting to look at in the glider data, and also lose the full seasonal cycle that the model could present us. Do you trust the model simulations enough to gain a small insight into other seasons?

- Line 404: Can you give some insight into how much this could impact your conclusions? Is this a significant limitation in using only spice as a tracer for submesoscale flows?

I feel that the paper is missing a final conclusion statement or section, to give a summary of the main results and findings. Also a clear comment on the impact/implications of the study, and any limitations or future aspects to explore.