Review of manuscript *Storm-Modulated Submesoscale Dynamics over Sloping Topography in a Wind-Driven, Non-Tidal Basin*
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The manuscript presents a numerical study of wind-driven submesoscale dynamics at depth in the Baltic Sea. The major result is that the ocean dynamics at depth, and specifically above topographic slopes, responds to strong wind events in generating submesoscale instabilities and flows that are sheltered (uncorrelated) from the surface dynamics. The manuscript is rather descriptive but well-written and easy to follow. Most results are demonstrated with thorough diagnostics. Nonetheless, I have two major concerns that I think prevents the manuscript from being published in the present state. Major and minor comments are listed down below.

Major comments

1. **The link between winds and deep submesoscales is unclear.** I agree that deep submesoscales are transient, depend on the low-frequency current orientation with regard to the topographic slope, and that this changes over time (Figs. 3 and 4). However, the dynamical link between the wind and the circulation is not obvious. The response to similar wind events can be qualitatively different. For example, northwards winds could lead to different current patterns and orientations (Fig. 5). I think this might be because typical wind events are very short (~1-3 days) and the deep circulation does not have time to reach a steady state after the onset of such event. Hence, the circulation might always be in a transient, unbalanced state, with the specific response to local winds (probably nonlinearly) interacting with a background or residual circulation. Also, this ambiguity might come from how wind events are defined (Fig. 1c). The wind time series shows some gradual variability and I cannot pick specific "events" such as the ones highlighted in grey shadings. For example, on 21st Oct and 27th Oct, the winds are equally strong as in the first event. **So, I would recommend to first try to draw some qualitative and quantitative links between winds (direction, intensity) and the deep (alongslope) currents, and then elaborate on EPV, instabilities, etc.** The authors could lean on the literature on upwelling systems (e.g., Gilcoto et al., 2017) to infer time scales of the response.

2. Figures 5, 6, 7 and associated text: Oscillations are huge, they are of similar amplitude as time-averaged variables. **Are these near-inertial oscillations?** (2pi/f=14.3h at 57N and the manuscript title says that the basin is non-tidal) I guess they have a signature in velocity, which should impact vorticity. Given the large amplitude of these signals, they would deserve to be specifically studied. I suggest the authors to show time series of variables at time intervals allowing to resolve near-inertial frequency, and assess whether or not these near-inertial oscillations impact the dynamics.

Minor comments

- Line 4: remove “primarily” as surface processes are equally important
- L30: glider observations have routinely revealed submesoscales, e.g., Bosse et al. (2017).
- L34: the routes to dissipation are reviewed in a book chapter (Gula et al., 2022).
- L43: see global survey of submesoscale coherent vortices in McCoy et al. (2020).
- L46-49: this mostly repeats the end of the previous paragraph, it should be merged or simplified.
- L52 and elsewhere: I am confused with the use of “deep water” since the study does not go deeper than 100 m.
- L62: remove “region”
- L70: near-inertial waves are mostly generated by transient winds but in the sentence, they appear to be decorrelated.
- L72: I would remove the last sentence of the paragraph.
- L78: At this stage of the manuscript, I would use “hypothesis” instead of “finding”
- L127: Fig. 3b should not be referred to before Fig. 2.
- L129 (see also Major Comment): how are the wind events defined? I would recommend to drop the strategy of studying specifically these events and look more carefully at the continuity of the oceanic response.
- L163: remove “most”
- Figures 2 and 3: There is no projection on a map (the grid seems regularly spaced in lon,lat) and the fields appear very distorted. 1 degree of lon = 60 km but 1 degree of lat = 111 km, there is an aspect ratio of almost 2.
- Fig 2: extend the colorbar range to [-1,1] to better see where the instability criterion for zeta<-1 shows up.
- Fig 2: I do not see the point of showing maps at 80 m and 100 m as these depths are roughly within the same T,S,rho range (Fig 1b). Why not showing 150 m instead of 100 m ?
- L190: “ubiquitous at the peak of the storm” → how about non-stormy conditions?
- L192: On the coexistence of submesoscale “disconnected patterns” between layers, this is not very new, it has been shown in many open ocean studies (Bosse et al., 2017; Vic et al., 2018).
- L216: I do not understand the sentence starting with “Fig. 3 shows…”
- L228: remove “waters”
- L236: remove “vortically”
- Fig 4: extend a bit deeper? 150 m? it would be nice to see the bottom edge of typical submesoscale structures.
- L281: How do you define “interior”? away from topographic slopes? How far?
- L288: Watch out with the use of O(x). It defines orders of magnitude (0.1,1,10,…) and does not mean “approximately”. The threshold of 0.5 is very large. Quasi-geostrophy applies for Ro<<1, so in O(0.1). I would use a smaller threshold to distinguish between balanced mesoscales and unbalanced submesoscales.
- L298 and within the section. To me, there is no clear link between wind events and the prevalence of instabilities.
- L301: towards northerly directions → northward
- L332: This is not shown in the manuscript (Ekman transport, etc.)

- Fig 7: I think it would be nice to show a map of epsilon, and perhaps link it to EPV patterns.
- L374-375: I disagree with this statement, again, the link between winds and the oceanic response at depth is not obvious.

References

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