

Author's Response: Reviewer 2

Thank you very much for reading our manuscript and for the constructive feedback, which helps us to improve the quality of the manuscript. We addressed all the points raised in the review. We will provide a point-by-point response to each comment below. The responses to the comments are in blue. The lines refer to the revised, resubmitted manuscript (without tracking changes).

This is a clear and very well written paper describing observations of the upper ocean in tidally dominated region of the Baltic for a few hours under conditions of weak and then stronger winds. The results are interesting, limited, but worth publishing. I can suggest a few improvements.

The main thing missing here is a quantitative comparison between the data and the predictions of the 1-D model. We are decades past of point of saying that my favorite model mixes about right when the wind blows. The issue these days is how well it does that in terms of actual numbers. The T/S structure is complex, and laterally dominated, so the best comparison should be with density, rather than T or S, at the six CTDs on the two drifters.

We thank the reviewer for this suggestion. We agree that the model-observation comparison should be quantified rather than based only on the qualitative agreement of the temperature evolution. We therefore added a direct comparison of the observed and modeled thermohaline structure at the CTD sensor depths (new Fig. 7), with particular emphasis on the potential density anomaly. This direct model-data comparison is now described in Section 3.4, and a detailed discussion of the physical processes associated with the delayed NSL response following the increase in wind forcing is provided in Section 4.1.

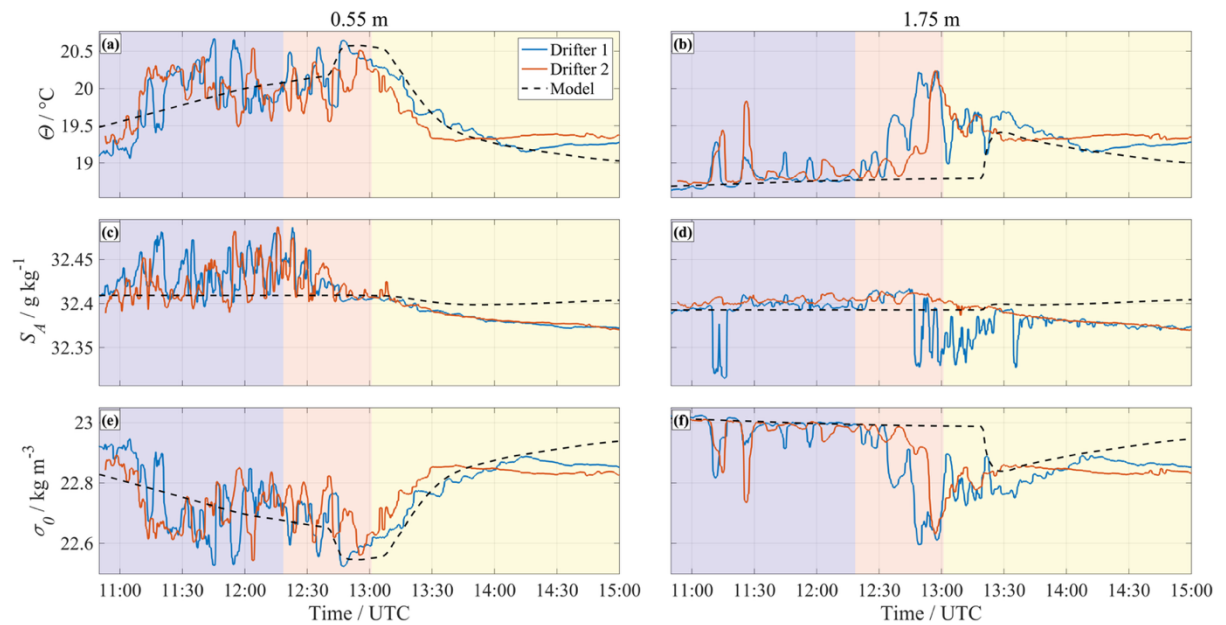


Figure 7: Comparison of Conservative Temperature Θ , Absolute Salinity S_A , and potential density anomaly σ_0 between Drifter 1, Drifter 2, and the 1-D model at 0.55 m (left) and 1.75 m (right). Background colors indicate the three wind-forcing periods defined throughout the study.

The two drifters are very close, yet show significant differences. This is clear evidence of lateral gradients. The magnitude of these gradients should be quantified. My guess is that the density gradients are much less than the T/S gradients in density units (“Spice” might be useful here), which implies that the small-scale lateral density gradients are causing the interleaving, thereby erasing the lateral density gradients. You could go a long way down this path, which is not necessary in this paper, but you should at least point it out. This will strengthen your conclusions about lateral processes - the vertical T/S gradients are typically caused by the slumping of horizontal gradients.

Thank you for this insightful comment. We have now quantified the thermohaline differences between the two closely spaced drifters. During the observation period, their separation ranged from approximately 143 to 282 m, while lateral differences in potential density anomaly reached up to about 0.3 kg m^{-3} .

These results are now presented in Sect. 3.6 and Fig. 9. We have also expanded the discussion in Sect. 4.4 to assess whether relative motion through the resolved lateral gradients could account for the rapid local variability. This analysis shows that sustained advection across the gradients resolved at the drifter-separation scale is insufficient, while unresolved smaller-scale horizontal variability, intermittent vertical motions, or a combination of both remain possible (lines 503–530).

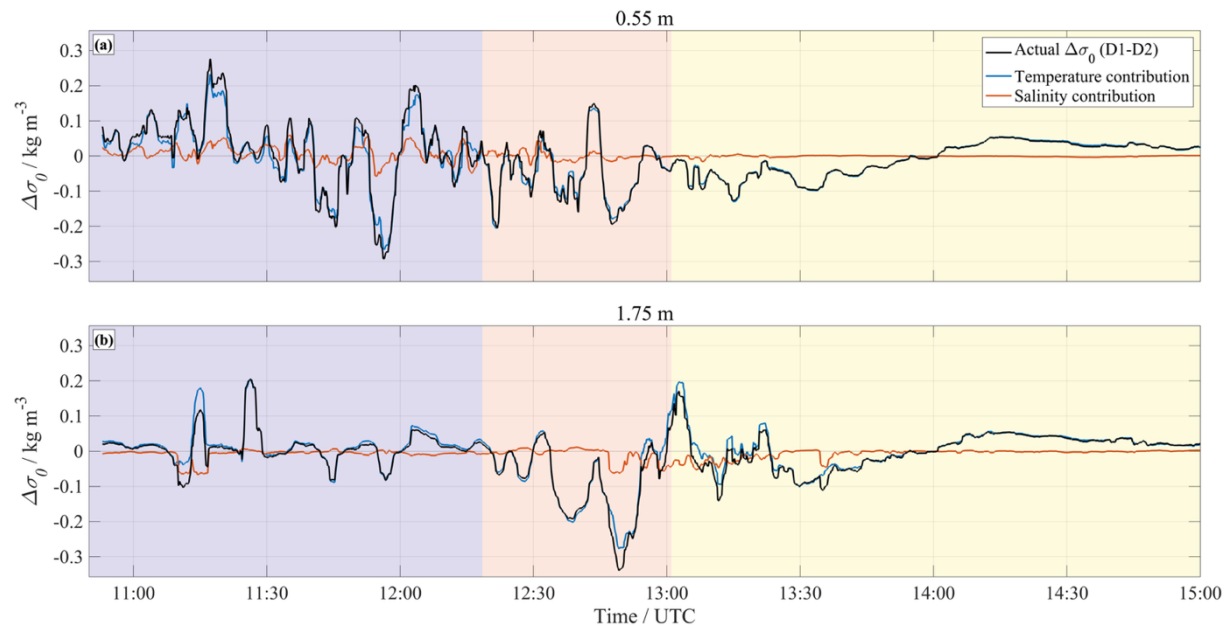


Figure 9: Lateral potential density anomaly difference $\Delta\sigma_0$ between Drifter 1 and Drifter 2 at 0.55 m (top) and 1.75 m (bottom). The temperature and salinity contributions to the observed density differences $\Delta\sigma_0$ are shown separately in blue and red. Background colors indicate the three wind regimes defined in Fig. 4.

The rapid change in temperature at about 1230 is repeatedly described as a decrease in ‘stratification’, implying density stratification, but inferred from temperature. The case for this is weak- the change seems weaker in density than in temperature. The quantitative comparison suggested above should resolve this issue.

We agree that the original wording could be interpreted as inferring density stratification directly from temperature. We have therefore distinguished more clearly between temperature stratification and density stratification. The rapid change around 12:40–12:45 UTC is now described as a rapid weakening of the temperature stratification. The density-based diagnostics show a weaker response, with the NSL remaining statically stable during periods 1 and 2 and becoming nearly well mixed only during period 3.

It is claimed that salt finger fluxes might be important. What are the salt fingering fluxes? Are they big enough to be important?

Direct salt-finger fluxes cannot be quantified from our measurements because no microstructure measurements of scalar dissipation or vertical turbulent fluxes are available. Applying existing flux parameterizations to the bulk gradients between our two CTDs, separated by 1.2 m, would furthermore require assumptions about unresolved vertical structure that cannot be tested with our data.

We therefore additionally examined the magnitude of the density ratio within the observations classified as favorable for salt fingering. Previous observational and parameterization studies show that salt-finger mixing is strongest for density ratios close to unity and decreases substantially with increasing $R\rho$ (Large et al., 1994; St. Laurent and Schmitt, 1999). During period 1, only 1.1 % of the salt-finger-classified observations for Drifter 1 and none for Drifter 2 fall within $1 \leq R\rho < 2$, whereas the majority of observations for both drifters exceed $R\rho = 5$ (Large et al., 1994; St. Laurent and Schmitt, 1999). This indicates that, although the observed thermohaline configuration was formally favorable for salt fingering, the associated growth rates and vertical fluxes were likely weak. We have revised Sect. 4.3 accordingly and now explicitly state that conditions were favorable for relatively weak salt-finger-type double diffusion, but that the observations do not demonstrate its occurrence and it is not interpreted as a dominant vertical transport mechanism (lines 475–501).

To reflect this more cautious interpretation, we have also revised the manuscript title from “Fine-scale Thermohaline Stratification in the Near-Surface Layer Under Weak Wind Conditions with Indications of Salt Fingering” to “Fine-scale Thermohaline Stratification and Variability in the Near-Surface Layer Under Weak Wind Conditions”.

Section 4.3- “Slower larger large-scale advection” is unimportant. True, but advection does not have to either slow or large scale.

We have examined this question more explicitly in the revised manuscript. Because the drifters provide an approximately Lagrangian representation of the near-surface flow, their horizontal motion relative to the surrounding water is expected to be small but may arise from (i) wind slip and (ii) vertical current shear. In Sect. 4.4 (lines 510–524), we show that the resulting relative velocities are of order 10^{-2} m s^{-1} and are too small to explain the observed rapid density fluctuations by advection across the measured lateral gradients. The required

horizontal density gradients would have to be approximately one to two orders of magnitude larger than those observed at the drifter-separation scale. In addition, the lateral density differences fluctuate around zero on short timescales, indicating a largely reversible signal rather than the persistent change expected from sustained advection across a pre-existing gradient. We therefore conclude that this type of small-scale advection is unlikely to be the primary cause of the rapid fluctuations, while differential advection may still contribute to the slower development of the inverse salinity stratification.

Section 4.4 - Introducing slicks is a good idea. The most obvious source of such slicks is small-scale advection-their existence is thus strong evidence for such advection. Meter-scale fronts are common in the Baltic and should be also evident in the gradient calculations suggested above.

We agree that small-scale advection may contribute to the formation and persistence of surface slicks and that the observed slicks are consistent with a laterally heterogeneous near-surface flow. In the revised manuscript, we therefore discuss surface slicks together with the observed small-scale thermohaline variability and lateral gradients. However, because the available observations do not allow us to directly link individual slick features to specific advective structures, we do not interpret their presence as direct evidence for a particular small-scale frontal process.

Data availability - The next version needs a DOI.

Thank you for pointing this out. The DOIs for the dataset and the corresponding dataset publication series have now been added to the Data Availability section.