

Author's Response: Reviewer 1

Thank you very much for taking the time to review our manuscript, and we appreciate your constructive and positive feedback. We hereby provide a revised version of our manuscript and provide answers to all comments below. The answers to the comments are in blue. The lines refer to the revised, resubmitted manuscript (without tracking changes).

The paper deals with observations by two drifters instrumented in the top 150 cm in a very low wind environment during a few hours with heat flux estimates from a nearby ships. Comparisons are made with a one-D numerical model simulations.

Major comments:

1: The drifters are equipped with sensors first along the hull of the drifters (near 20 cm and 40 cm), thus influences by possible mixing/circulation along the hull, and then with sensors along a thin line. The T, C sensors are measured by RBR Brevio at 0.55 and 1.75 m. These sensors are designed to be used as a CTD during CTD casts (w close to 1 m/s) and not necessarily as is here. At 55 cm, the sensors are at a level of rather high vertical T gradient during the low wind morning period. I wonder what kind of noise, biases (due to differences between the measured T, compared to the ‘effective’ T in the volume of the inductive cell in which C is measured) this could induce when estimating S at such high frequency and strong vertical gradients (‘average’ gradients are up to $\sim 0.5^{\circ}\text{C}$ in the 15 cm between 40 and 55 cm, at time according to figure 4; thus a mismatch of 0.05°C is probably in the range of what is likely, and that could induce the kind of noise/variability in the 55 cm salinity, as well). Also, the high frequencies of S can certainly not be taken at face-value as the T measurements is not made at the same location (horizontally) as the C one, and one needs to know what frequency range can be considered as accurate in S.

We thank the reviewer for raising this important concern. We agree that a temporal mismatch between the temperature and conductivity measurements could have affected the derived salinity at high frequencies, particularly under the strong near-surface temperature gradients observed during the low-wind period. To assess whether such a mismatch is relevant in our data, we first analyzed the temporal relationship between the temperature and conductivity signals. As the conductivity sensor also responds to temperature fluctuations, this approach provides an empirical estimate of a potential lag between the temperature sensor and the “effective” temperature inside the conductive cell that the reviewer mentions above. A cross-correlation analysis consistently indicated a small temporal offset of approximately 0.5 s.

To further assess the timescales over which the relationship between temperature and conductivity can be considered robust (i.e., is not dominated by noise), we performed a magnitude-squared coherence analysis on the despiked raw temperature and conductivity records, without prior lag correction or temporal filtering. The coherence increased with increasing period, and was close to 1 across all four CTDs at periods larger than approximately 100 s (Fig. S1a–d). We therefore selected a 120 s moving median filter for our revised analysis. Please note that this does not imply that the variability at shorter timescales is necessarily artificial. Rather, it

avoids basing our interpretation on the frequency range that might have been effected by different sensor response times, different flushing times, different sensor locations, etc.

Based on these results, we then repeated the complete processing both with and without shifting the temperature record by +0.5 s relative to conductivity, using otherwise identical despiking and filtering before recalculating salinity. This allowed us to directly assess how strongly the observed T–C mismatch affects the derived salinity. Although the lag adjustment reduced part of the highest-frequency salinity variability, the resulting salinity structure at the 120 s timescale remained nearly unchanged (Fig. S1e–f). Over the full deployment period, the mean absolute differences between the unshifted and lag-adjusted 120 s ΔSA time series were only 0.0012 g kg^{−1} for Drifter 1 and 0.0008 g kg^{−1} for Drifter 2. Consistent with these small differences, direct correlations of the unshifted 120 s ΔSA time series with the corresponding lag-adjusted 120 s ΔSA time series yielded $r = 0.998$ for Drifter 1 and $r = 0.997$ for Drifter 2. Because the 0.5 s offset was empirically derived rather than representing a uniquely determined instrumental response time, we did not apply a fixed lag correction to the full dataset and instead used the lag-adjusted analysis as a sensitivity test.

Taken together, the coherence and lag-sensitivity analyses show that second-scale salinity fluctuations represent the most uncertain part of the record and are therefore no longer used as primary evidence in the revised interpretation. At the same time, the salinity structure at the 120 s timescale, including the vertical salinity differences relevant to our interpretation, is only weakly affected by the observed T–C offset. We revised the Methods to explain the use and selection of the 120 s moving median (lines 145–148), and the corresponding coherence and lag-sensitivity analyses were added to Sect. S1 and Fig. S1 of the Supplement.

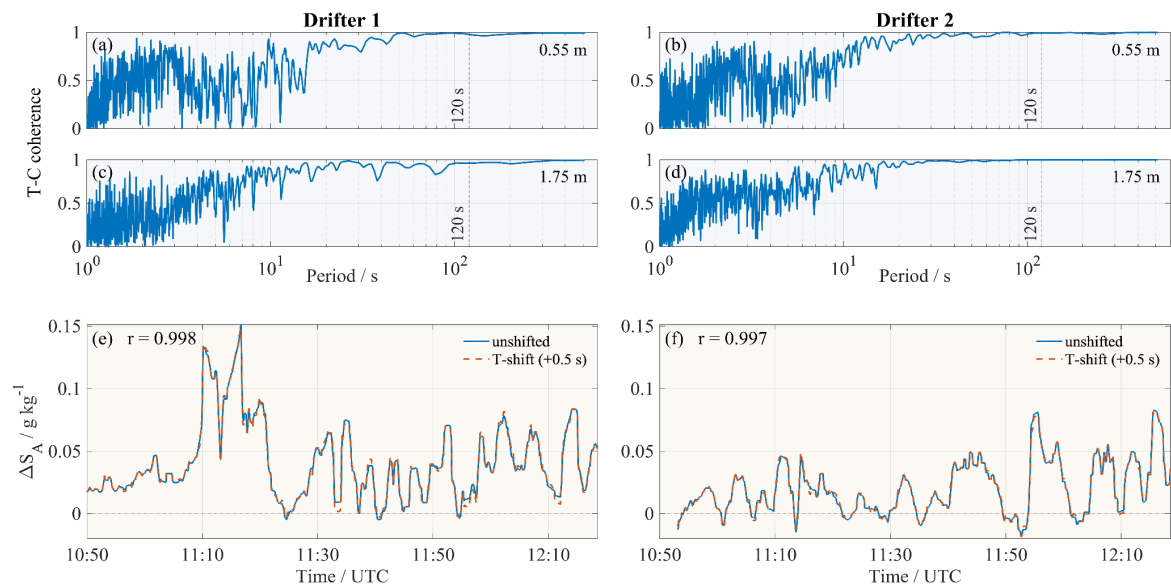


Figure S1: Panels (a)–(d) present the magnitude-squared coherence between temperature and conductivity for both CTDs of Drifter 1 and Drifter 2. The dashed line marks 120 s, where the T–C relationship becomes consistently more robust. Panels (e)–(f) show the comparison of 120 s moving median ΔSA calculated with and without a +0.5 s temperature shift.

I strongly believe that to be more convincing on the double diffusive regime, there needs to be tests on what the RBRbrevio (used in the same vertical configuration as shown here for the 55 cm sensor) measures in such an environment (against maybe salinity samples from a pumping line at the same depth). This is typically the kind of environment where micro T-C cells (such as on some turbulence sensors) are used in profiling mode -not such a macro-instrument! This is not to say that there cannot be higher near surface salinity due to differential advection, as suggested here, but I don't find the evidence supported enough, unfortunately.

We agree that an independent reference measurement would have been useful for an additional validation of the salinity signal. Such an independent reference measurement was not available during this deployment. We therefore cannot independently validate the small vertical salinity differences measured by the RBRbrevio sensors. We will consider the reviewer's suggestion of an additional reference measurement for future developments of the drifter system and field campaigns.

Please note, however, that the sensitivity analysis described above lends some credibility to our salinity measurements on timescales longer than 1–2 minutes. We have nevertheless revised our interpretation to distinguish more clearly between what is directly supported by the measurements and what remains a possible process explanation. As described above, we no longer use the second-scale salinity fluctuations as primary evidence. However, the inverse vertical salinity structure remains clearly detectable in the 120 s moving median records, and the temporal variability is still present after this filtering. We therefore retain the stability diagnostics, but interpret them as evidence for thermohaline conditions favorable for double-diffusive processes rather than as direct proof that salt fingering occurred (Sect. 4.3, lines 475–501). The generally large density ratios provide an additional constraint: most salt-finger-classified values exceed $R_\rho = 5$, indicating that associated growth rates and vertical fluxes were likely small (Large et al., 1994; St. Laurent and Schmitt, 1999).

We have also broadened the discussion of the short-term variability. The differences between the two nearby drifters demonstrate pronounced lateral thermohaline heterogeneity on scales of a few hundred meters. At the same time, sustained advection across the lateral gradients resolved at the drifter-separation scale cannot explain the observed rapid local fluctuations. The revised discussion therefore considers unresolved smaller-scale horizontal variability, intermittent vertical motions, or a combination of both, while emphasizing that their individual contributions cannot be fully resolved from the available observations (Sect. 4.4, lines 503–530).

1. 1. 180: instead of 'provide indications...' 'could suggest the occurrence of double-diffusive...'

I think that the statement as is reported here is too strongly worded, based on what remaining uncertainties there are on the fluxes and the model formulation which could induce deviations from the observed profile... (even if it is just sheared horizontal advection, as is argued for the source of S variability, but could also influence T). In particular I am not fully convinced by the fit of the observed T profile to the shortwave penetration parameterization on line 185, which I find a rather rough estimate, as we don't know what the real absorption is (pigments, etc...) and, as this is just based on fitting the temperature profile (which could be affected by the mixing processes, including double diffusion, or horizontal advection).

We agree that the original wording was too strong given the remaining uncertainties in the observations and model formulation. In Sect. 2.5, we therefore clarified the limitations of the model and now explicitly state that GOTM does not represent lateral variability or double-diffusive mixing (lines 186–189).

The discussion was revised accordingly. We now emphasize that GOTM reproduces the main characteristics and bulk evolution of the diurnal warm layer, but does not reproduce all observed features. In particular, the model does not fully capture the inverse salinity stratification, the pronounced short-term thermohaline variability, or the rapid breakdown of near-surface stratification following the increase in wind forcing (Sect. 4.1, lines 408–452). We therefore avoid attributing deviations between observations and model uniquely to double-diffusive processes.

Also, notice the uncertainty of not having the T-profile in top 20 cm, and thus difficulties of having the proper surface boundary conditions, and the whole profile. I was actually wondering whether the top drifter structure in the presence of horizontal current shear would not induce a little bit more of mixing. I am wondering about that to some extent because of figure 4, and the rather homogeneous signals at 20 and 40 cm depth until 12:30 ‘and the strong gradient with the next level at 55 cm (and with S) which is below the surface buoy (same for Figure S3). Also, this make me wonder what are the parameters that really enter in the COARE algorithm, which is not that well-tuned for strong near surface T-gradients with very weak winds (I am thinking to the warm layer, more than the skin effect, albeit if there are slicks, this could also be non-obvious...), but that’s probably a minor effect, compared to the huge incoming short-waves during the early low wind period.

Do you have some idea of what is the resulting uncertainty associated with it. I am aware that this is certainly not easy to assess, but maybe some assumptions can be made to give a range of its effects.

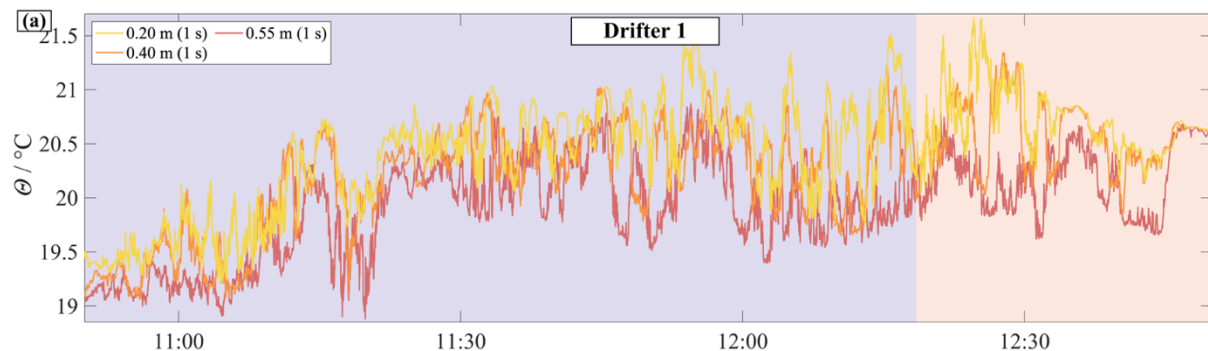


Figure R1: Conservative temperature at 0.20, 0.40, and 0.55 m depth for Drifter 1. Background colors indicate the wind-forcing periods used throughout the manuscript.

While we agree that drifter-induced mixing could, in principle, be a factor in a vertically sheared flow, we do not believe that this type of mixing could have substantially affected our measurements. Drifter-induced mixing would occur in the wake of the drifters. However, our sensors are mounted directly at the drifters, so they would not be able to detect the delayed effect of mixing in the wake.

The reviewer is right that the uppermost sensors show only small temperature differences between 0.20 and 0.40 m during much of the weak-wind period, while a pronounced temperature gradient occurs toward 0.55 m (as shown in the figure above). We therefore interpret the observations as indicating a very shallow near-surface mixed layer, suggesting that our measurements at 0.2 m depth do represent the true SST that determines the air-sea fluxes in COARE. This interpretation is also supported by the fact that a similarly shallow mixed layer is

reproduced rather well by our turbulence model (see Fig. 6). A recent comparison of results from this turbulence model with LES, describing the structure of thin stratified near-surface layers under strong radiative forcing (diurnal warm layers, see Schmitt et al., 2024), suggests that the model provides a realistic representation of this kind of flow, supporting our argument.

The COARE algorithms do contain an option to correct for the effect of unresolved diurnal warm layers when estimating SST. However, as explained above, we believe that our uppermost sensor at 0.2 m depth provides a reliable representation of SST even during Period 1 with weak winds and strong solar radiation. As the internal structure of the surface layer does not affect the atmospheric fluxes derived from COARE as long as SST can be reliably estimated, we do not expect the unresolved, but well-mixed, uppermost 0.20 m to introduce a significant additional uncertainty in the derived atmospheric fluxes. In addition, as pointed out by the reviewer, this uncertainty is likely to be small compared with the strong incoming shortwave radiation during the weak-wind period. Therefore, even if some uncertainty remains in the flux estimates, it is unlikely to significantly affect the main conclusions of our analysis.

3: l. 213: mention is done on ‘pure surface current’ pathway. I find that ambiguous, as we are in certainly stratified near-surface. Thus, there is certainly a near-surface vertical shear, even with this low wind (diurnal jets, and wind is not that low, for it not to partially set, at least based on the S1 photographs and the wind values reported). Do you think that the buoy drift might be representative of a drift at a level close to 20-cm depth in this low wind environment, or something slightly deeper? That shear might be larger than the direct wind slip. (afterward, I have read the 4.1 discussion, but this does not fully answer this important question).

Thank you for making us aware of this. In the revised manuscript, we now distinguish more clearly between the small direct wind-slip contribution and the possible effects of vertically sheared currents. The small direct wind-slip contribution means that the drifter still provides a largely Lagrangian view of the near-surface flow. However, some vertical shear may cause the water sampled at different sensor depths to move at slightly different horizontal velocities relative to the drifter. This is now discussed explicitly in Sect. 4.4 (lines 510–517). The modelled near-surface velocity structure is shown in Fig. S6, while Fig. 6d shows the corresponding vertical shear. The modelled velocity differences across the vertical extent of the instrument chain are of order 10^{-2} m s^{-1} , comparable to the expected wind-slip velocities. We further assess whether such relative motion could explain the observed rapid density fluctuations by advection across the lateral gradients resolved at the drifter-separation scale. The required density gradients would be one to two orders of magnitude larger than observed, and the lateral density differences repeatedly fluctuate around zero (lines 518–524). We therefore conclude that sustained advection across the lateral gradients resolved at the drifter-separation scale is insufficient to explain the observed rapid variability. The variability may instead reflect unresolved smaller-scale horizontal thermohaline variability, intermittent vertical motions, or a combination of both (lines 525–530).

4: l. 390-400: I am surprised that salt fingers extend over such a vertical scale exceeding 1-m in this kind of environment (what is mentioned is that this is supported by the temperature of Fig. 4, but it might seem less clear from figure 6e, 6f?). There is a large temperature diffusion that would need to occur, due to the high thermal stratification. I wonder what kind of horizontal scale would be involved (and how compatible it is with possible

vertical shear in the surface warm layer (NSL)? (to state it shortly, I doubt that this is possible over such a thick stratified and possibly sheared layer)

Partly based on this very helpful comment by the reviewer, we have now completely revised our interpretation of the observed short-term fluctuations, and, accordingly, the discussion in Sect. 4.3–4.4 (lines 475–530).

In the revised manuscript, we interpret the observed meter-scale fluctuations more cautiously and do not assign them to a single physical mechanism. The rapid variability may reflect unresolved smaller-scale horizontal thermohaline variability, intermittent vertical motions, or a combination of both, although their individual contributions cannot be fully resolved from the available observations. At the same time, we do not exclude that salt-finger-type double diffusion may occur locally under the observed thermohaline conditions. However, the observed meter-scale structures should not be interpreted as individual salt fingers, since the characteristic scales of individual fingers are much smaller than those resolved by our measurements. The Turner angle and density ratio are therefore retained only as indicators of conditions favorable for salt fingering, rather than as evidence that the observed meter-scale structures represent individual salt fingers.

Given this revised interpretation, we have also adjusted the manuscript title so that it no longer implies stronger evidence for salt fingering than supported by the observations. The revised title is “Fine-scale Thermohaline Stratification and Variability in the Near-Surface Layer Under Weak Wind Conditions”.

Minor comments:

l. 185: have other formulations of the shortwave penetration profile been tested, to check how sensitive the results are to this fit. The 0.6 m found suggests a fairly turbid near surface water (I fully understand that it is only the part of the profile near the surface which matters here). Has this been measured? Or could it be inferred from some of the photographs (for example of the jellyfish, which are at different depths on Fig. S2 (I am aware that this would be very qualitative; or on the top right of Fi. S1, which suggests some greenish color, but I am not sure what is to be seen there)

Our CTD system had a PAR sensor but this system couldn't be operated so close to the surface in any meaningful way. So we have unfortunately no reliable measurements of the underwater light field in the upper 2 m. We have tested different Jerlov types based on the 2-band expressions of Paulson and Simpson (1977) that are implemented in GOTM. Not surprisingly, it turned out that the second exponential attenuation term in these expressions, with absorption scales on the order of 10 m, did not play a significant role in the very shallow near-surface region (depth < 2 m) we study. This is the main reason why we only work with a single-band absorption model with one exponential light attenuation term. The vertical absorption scale we use was chosen by fitting the modeled increase of the NSL heat content to our observations, ignoring the short-term fluctuations that are triggered by processes other than surface heating. On a more qualitative level, our relatively small absorption scale seems consistent with the turbid waters seen on the surface images. This approach is now described explicitly in Sect. 2.5 (lines 190–200).

Interestingly figure S2 shows quite a lot of jellyfish. Do you think that these organisms (their density seems non-negligible) could induce vertical mixing (or heat transport) through their horizontal/vertical motion, or are they mostly passive at this time of day. I am aware that the photograph might not be the ‘average’ situation observed, but an extreme (is that the case?)

We agree that jellyfish could in principle cause local water displacement and mixing when passing close to the sensor chain, so occasional local disturbances cannot be excluded. However, the photograph in Fig. S3 represents only one moment during the deployment. Direct visual observations showed that jellyfish were present only intermittently and were not continuously present around the drifters or sensor chains. Moreover, comparable short-term thermohaline fluctuations were observed during other deployments of the same drifter system when no jellyfish were visible near the instruments (data not shown). Although these additional deployments are not analyzed here, this indicates that the observed variability is not contingent on the presence of jellyfish. We therefore consider jellyfish-induced disturbance unlikely to be the primary explanation for the variability reported here.

Is time in Figure 3, local sun time, or some other time (GMT?)

We assume that the reviewer is referring to Fig. 4. All times shown in the figures and discussed in the manuscript are given in UTC. We have clarified this by adding “UTC” to the axis labels and by stating the corresponding local time once in the text (line 244).

l. 238. I would fully expect a delay in the response of enhanced vertical mixing to the surface wind increase: time to set the short surface waves, and their breaking or interaction with near surface currents. (I would roughly expect 15 minutes for that, and there are some papers on this non-stationary situation and time to set the wind sea that you could refer to). Notice that as far as I know, this is also something not included in GOTM.

We agree that a delayed response of enhanced vertical mixing to the increase in wind forcing is physically plausible. We have revised Sects. 3.4 and 4.1 accordingly and now quantify the observed delay using a bulk Richardson-number argument. Because no direct surface-wave observations are available, we do not further interpret the delay in terms of wind-wave development. Wave-related effects such as Langmuir turbulence are not included in the present GOTM setup (Sect. 2.5).

l. 353: I don't believe that the heat is vertically distributed by turbulent mixing across the entire thickness of the DWL in all other studies with instances of low wind. See for example, the study by W. Asher, A.T. Jessup and D. Clark, 2014, JGR: Oceans, DOI: 10.1002/2014JC009808

Although heat and temperature budget are not explicitly discussed in this paper, which was dedicated to salinity surface increase in a low wind high radiative heating situation, you can see the high T-stratification right to the surface (for example diff of C between 10 and 20 cm; and the fact that S daily increase is found at 10 cm, but very minimal at 50 cm).

We agree and have revised the wording accordingly. We no longer imply that turbulent mixing distributes heat throughout the entire DWL under all low-wind conditions. Instead, we now distinguish between the actively turbulent layer and heating below it by penetrative short-wave radiation, consistent with previous descriptions of strongly stratified diurnal warm layers under weak-wind conditions (e.g. Price et al., 1986; Schmitt et al., 2024; Sect. 4.1, lines 415–420).

l. 385: is there some evidence from the gradient between the two drifters of horizontal T-gradients indeed at short spatial scale. timing of peaks/intermittency is different on the two drifters (at the beginning near 11:15, the data (if one trusts them) would suggest a lower S(buoy 2) at 55 cm than at buoy 1 (but also a higher T at buoy 2; by the way, this is among the features which made me a little worried about the potential T mismatches with the ones in the conductivity cell). Wind at this initial time seems from the south or south-east. Would it bring saltier or fresher surface water? (any idea on that?)

We agree that the differences between the two drifters provide evidence for lateral thermohaline variability on spatial scales of a few hundred meters. We have therefore added a dedicated comparison of temperature, salinity, and density between the two drifters, including the thermal and haline contributions to the density differences (Sect. 3.6, Fig. 9). The results show that the lateral density differences are predominantly temperature-driven, while salinity contributes less overall. At the same time, the rapid short-term fluctuations observed at each drifter cannot be explained solely by advection across these resolved lateral gradients. This is now discussed in Sect. 4.4, where we assess the possible contributions of lateral and vertical processes to the observed short-term variability (lines 525–530).

l. 272: '... suggests that...'

Thank you. We have corrected this accordingly.

l. 332: 'in terms of double-diffusive parameters'.

Thank you for pointing this out. The wording has been corrected in the revised manuscript.

l. 434: I am not sure that the low modeled SST (that T at 20 or 40 cm on the buoys) is necessarily the result of the surface slicks for the reasons outlined on line 431-432 (we are not talking of the same levels, I fear...). I would assume that turbulence would be low in this low-wind morning situation even without slicks, and that if it happened, it would not reach below 20 or 40 cm (the surface heat loss is not that large in all cases)

We agree that the lower modeled near-surface temperatures cannot be uniquely attributed to the presence of surface slicks. We have therefore revised this part of the Discussion. Surface slicks are now discussed more generally as a potential factor modifying near-surface temperature structure, mixing, and stratification under calm conditions, without using them as a specific explanation for the model–observation difference.

l. 543: in the reference ‘and R. Carpenter, J.’ includes an extra initial?

Thanks for noting that. The ‘R.’ was misplaced, and has been corrected.

l. 555: the reference is incomplete.

Thank you for pointing this out. We have revised the reference.