Response to all referee and editor comments

We like to extend our thanks to Angelika Renner and the second referee for their positive feedback and constructive criticism. We reviewed the manuscript to address the comments and suggestions in order to improve the manuscript. Following, we address the comments and highlight the changes to the manuscript (excerpt from manuscript with changes in *italics*). The lines where text was changed or inserted refers to the preprint version of the manuscript. The changes in the manuscript show the updated version of the manuscript after both reviews. If both referees addressed similar points, the changes in the manuscript for Angelika Renner's comments were updated to show the changes after both reviews. As we conducted changes in all Figures due to multiple independent comments, we show all changed Figures with captions at the end of this response, and not as a change for every comment regarding the Figures.

Comments first referee (Angelika Renner)

Referee comment 1:

Line 96 and following lines were discrete samples are mentioned: how did you take these samples?

Response:

The SML samples were collected using the glass plate method (Harvey and Burzell, 1972). The ULW samples were collected using a syringe with a tube attached. Subsamples of these samples were then analysed for different parameters. More details about the discrete sampling procedure can also be found in Bibi et al. (2025a). At line 93, a brief paragraph was inserted to describe the sampling procedure.

Changes in manuscript:

Line 93: […] 2 cm and 40 cm below the surface. *Discrete water samples from the SML and ULW were collected daily. To account for diurnal changes, sampling alternated between one hour after sunrise and ten hours after sunrise (Bibi et al., 2025a). SML samples were collected using the glass plate method of Harvey and Burzell (1972), while ULW samples at 40 cm depth were collected with a syringe attached to a tube (Bibi et al., 2025a). Subsamples of these samples were subsequently analysed for different parameters.* Surfactant concentrations in the SML and ULW […]

Referee comment 2:

Line 99: I immediately wondered about calibration samples for oxygen and only discovered the answer to that when I got to the appendix – add a reference to the relevant section in the appendix somewhere here?

Response:

A short sentence about the correction of the FerryBox oxygen measurements was inserted in the method section (line 100) and in Appendix B.

Changes in manuscript:

Line 100: […] measured oxygen concentration, temperature, and salinity. *The FerryBox oxygen concentration was corrected with daily discrete samples analysed with the Winkler method (see Appendix B).* Chlorophyll *a* measurements […]

Referee comment 3:

Line 114: did you allow for the water to settle again after moving the microsensors to a new depth? With the entire plate that the sensors are mounted on moving through the water, that generates quite a disturbance?

Response:

The water settled during the measurements, which took about 30 s per vertical step. With a small step size of 125 µm, minimal turbulence during sensor movement can be assumed, and enough time for settling is ensured. The sentence in line 115 was changed accordingly.

Furthermore, since the sensors were mounted as far away as possible from the centre of the holder and the tips of the microsensors were positioned a few centimetres above the holder, any micro-scale turbulence generated by the holder's movement should not reach the sensors and therefore not influence the measurements.

The holder for the microsensors shown in the photo was used during the pre-experiment phase but was replaced during the main experiment with another holder with a smaller surface area, reducing turbulence. To avoid confusion, the photo was replaced with the correct holder, and a second photo showing a detailed view of the microsensors in the water was added. Furthermore, the microsensor measurement rate of 50 Hz was added in line 114.

Changes in manuscript:

Line 114: […] the microsensors took three measurements *with 50 Hz,* each lasting 10 seconds, […]

Line 115: […] The slow movement *of the microprofiler ensured minimal artificial water flow and turbulence in proximity to* the microsensors. […]

For changes to Figure 1 see end of this manuscript.

Referee comment 4:

Lines 119-121: was the room kept dark over night as well?

Response:

No, as SURF is located outside and has a transparent roof, it was exposed to natural diurnal variations in light. Artificial light or moonlight at night could not be excluded entirely. A short sentence was added in line 117. More pictures of the mesocosm facility can also be found in Bibi et al. (2025a).

Changes in manuscript:

Line 117: […] from 22 May onward were further analysed. *The transparent roof of SURF allowed the water to be exposed to natural diurnal variations in solar radiation.* To *reduce* the effects of solar radiation and diurnal heating […]

Referee comment 5:

Section 2.2: what are accuracy and precision of the microsensors?

Response:

According to the company, no official accuracy or precision for the microsensor can be provided, as these depend on many factors, including calibration frequency, lab electrical noise, and temperature stability. However, the sensors are fast-responding, exhibit minor drift over time, and have a low stirring sensitivity (Unisense.com). For the oxygen microsensor, only a detection limit of 0.3 µmol L^{-1} can be given, which is well exceeded in our study. The temperature microsensor has a resolution of 0.1 °C. These specifications of the microsensors were added line 110. In other literature, no accuracy or precision data for the Unisense microsensors could be found. Accuracies for other, larger Clark-type oxygen sensors are provided as ± 3 % (e.g. https://www.sea-sun-tech.com/product/clark-oxygen/).

In general, our focus does not lie on the absolute values of oxygen concentration or temperature, but on the profiles and differences across the DBL/TBL. These differences were clearly identifiable in of the majority of the 141 profiles. Due to large differences being observed in our study, slightly larger inaccuracies – even if assumed doubles compared to other larger Clark-type sensors – are of less concern.

Changes in manuscript:

Line 110: […] on a MicroProfiling System (UNISENSE, Denmark) (Fig. 1). *The microsensors were fast responding, exhibited minor drift over time and had a low stirring sensitivity. The oxygen microsensor possessed a detection limit of is 0.3 µmol L^{-1}, and the resolution of the temperature microsensor was 0.1 °C.* The microsensors were mounted […]

Referee comment 6:

Line 140: do you have an estimate how much of an offset there was?

Response:

The offsets in microsensor height relative to the water surface differed slightly between the nights and laid between –2500 µm and 3375 µm (mean –371 ± 1292 µm). The estimate was inserted in line 140. The microsensors were mounted by hand as close as possible to the water surface. Together with changes in the water level through evaporation, this manual sensor adjustment introduced the offsets. These effects could not be completely avoided. For most profiles, the offsets did not affect the analysis, as a complete profile from the air into the water was present. Only on 26 May for temperature sensor 2 and 30 May for temperature sensor 1, did they prevent the profiles from being analysed.

Changes in manuscript:

Line 140: […] introduced minor vertical offsets *(–371 ± 1292 µm)*, which were corrected […]

Referee comment 7:

Lines 146-149: I can't quite follow here – you used only the measured values at points P1-6 for the regressions, not the measurements above/below/in between? Why?

Response:

No, for the air regression line, all measurements between P1 and P2 were used; the same for all measurements between P3 and P4 for the DBL regression line, and all measurements between P5 and P6 for the ULW regression line. Line 146 was changed for clarification.

Changes in manuscript:

Line 146: […] three linear regressions were computed *with all measurements between the pairs* P1 and P2, P3 and P4, and P5 and P6. […]

Referee comment 8:

Figures 2 & 3 and respective parts in the main text: What you call «oxygen gradient» and «temperature gradient» is just a difference as you actually also say in the captions. The gradient would be the slope of the curve. To derive the gradient (with depth), you need to divide this difference by the vertical distance between the two points as well, i.e. by the DBL and TBL thickness, respectively. Or refer to the differences in the text, not gradients.

Response:

As the focus lies on absolute air–sea differences, e.g., to compare the cool skin effect, the wording was changed throughout the text from oxygen/temperature gradients to differences. The gradient, as a difference per layer thickness, was not calculated. Changes from oxygen/temperature gradient to oxygen/temperature difference were made in Figures 2, 3, 5, and 8 and throughout the manuscript when not explicitly referring to a gradient.

Changes in manuscript:

Changes from 'oxygen gradient' to 'oxygen difference' throughout the text and Figures. For changes to Figure 2, 3, 5 and 8 see end of this manuscript.

Referee comment 9:

Line 197-198: I don’t understand what you mean by «the smallest T gradients could be detected»? Do you mean that the differences in temperature were greater than the noise floor of 35 mK?

Response:

Yes, the temperature differences exceeded 35 mK. But the sentence means that temperature differences as low as 35 mK were detectable due to the slight noise-equivalent temperature difference. The sentence in line 198 was rephrased for clarification.

Changes in manuscript:

Line 197: […] While the absolute value had an accuracy of ±1 °C, *horizontal temperature differences larger than 35 mK* could be detected, as the noise equivalent temperature difference was greater than 35 mK. […]

Referee comment 10:

Line 213: how did you observe/asses the «slick-like properties»?

Response:

The slick-like properties were not only visually apparent but also evident in the surfactant concentration, which exceeded the threshold for slick conditions of 1000 µg L–1 (Bibi et al., 2025; Wurl et al., 2011) in large parts of the bloom and post-bloom phases. A sentence was added in line 213.

Changes in manuscript:

Line 213: […] During the bloom and post-bloom phases, *a slick was visually observed at the water surface. The presence of a slick was confirmed, as the surfactant concentration exceeded the slick threshold of 1000 µg L^{-1} during the bloom and post-bloom phases (Bibi et al., 2025a; Wurl et al., 2011).* For the oxygen microprofiles […]

Referee comment 11:

Line 215-216: How much of the decrease with depth is due to the time it took to complete the profiles? Looking at the temporal progression between profiles, this could have been a major factor, not that there was such a pronounced vertical gradient if you could have measured each profile instantaneously?

Response:

We calculated the vertical rate similarly to the gas exchange rate (Eq. 1), with C_0 and C_t representing the oxygen concentrations in the ULW at P5 and P6, respectively, and Δt the time difference between those measurements. The results show that this vertical rate is typically similar to the gas exchange rate, with differences between the two rates around 0.038 ± 0.157 cm h^{-1} for most of our observations. This means that the vertical gradients observed are, to a large extent, caused by a decrease in oxygen over the time between the measurements. Only after 07 June, in the post-bloom phase, did the vertical rate consistently exceed the temporal gas exchange rate (mean difference of 0.538 ± 0.375 cm h^{-1}). This meant that, in this phase, the vertical rate was not entirely due to the decrease in oxygen over time, but also due to a vertical gradient in the ULW. A paragraph describing this was added in Section 3.1. A sentence in the discussion in line 355 was also changed to account for this.

Changes in manuscript:

Line 217: […] over a few millimetres. *In addition, the oxygen concentration decreased with depth in the ULW. To test whether this decrease was due to depth or time, vertical rates were calculated, similar to the gas exchange rate in Sect. 2.5. Equation (1) was used, now with C_0 being the oxygen concentration at P5 and C_t the concentration at P6. Δt was the time difference between the measurements at P5 and P6. Until 06 June, the mean vertical rate for each night was similar to the overall gas exchange rate (Sect. 3.4) (differences of 0.038 ± 0.157 cm h^{-1}), indicating that a decline over time mainly caused the decrease in oxygen concentration with depth in the ULW. However, in the post-bloom phase from 07 June onward, the vertical rate consistently exceeded the gas exchange rate (difference of 0.538 ± 0.375 cm h^{-1}), indicating a vertical oxygen gradient in the ULW that was not solely due to temporal decreases.* On 22 May, before the bloom, […]

Line 355: […] two millimetre-thick layers in the air and ULW with notable oxygen gradients *in the air*, and one sharp diffusion boundary layer […]

Referee comment 12:

Line 216: did you do a reference measurement in the air in the room at large?

Response:

No, there was no reference measurement in the air. The oxygen microsensor measured the partial pressure of oxygen and then calculated the oxygen concentration based on its solubility in water. Consequently, the measurements in the air did not have the correct concentration because the solubility in water was assumed. Thus, no reference measurement in air was needed, as no accurate oxygen concentrations in the air portion of the profiles were measured; only partial pressure was measured. A second x-axis was added to Figures 2 and 4 to show the partial pressure measurement. This represents accurate values across all parts of the profiles, while the oxygen concentration values are correct for the water (DBL and ULW). Since the concentrations in the DBL are later used to calculate oxygen differences, we decided to include both units on the axis.

With the measurement in partial pressure, the shape of the profiles did not change, as the conversion from concentration to partial pressure is a division by the solubility of oxygen in water. Thus, H_0 and H_{DBL} can still be accurately determined, and the method for determining the DBL thickness and oxygen differences from Section 2.3 remains accurate. The descriptions in Section 3.1 of the profile in the air were changed from describing the changes in oxygen concentration to changes in partial pressure, and we expanded Section 2.2. Furthermore, a sentence in the discussion was changed accordingly.

Changes in manuscript:

Line 110: […] *The oxygen microsensor measured the oxygen partial pressure in the air and water and converted it to an oxygen concentration which was accurate for the water without changing the shape of the microprofiles.* The microsensors were mounted a few centimetres apart, […]

Line 214: […] For the oxygen microprofiles during the nights of all bloom phases, the oxygen *partial pressure* in both the air and the water decreased from one profile to the next, with the larger decrease in the water (Fig. 4). During all nights, a gradient in *oxygen partial pressure* existed in the air, with higher *partial pressures* near the water surface. Typically, *air gradients exceeded 0.02 atm* over a few millimetres. […]

Line 358: […] Our measurements indicate that significant gradients in *partial pressure* are present in these layers. […]

Referee comment 13:

Figure 4: the colours are neither colour-blind nor greyscale friendly, please edit.

Response:

The colours in Figure 4, A1, B1 and E1 were changed to a more colour-blind and grey-scale-friendly palette.

Changes in manuscript:

For changes to Figure 4, A1, B1 and E1 see end of this manuscript.

Referee comment 14:

Line 243: what do you mean by «them»?

Response:

We meant the correlation between the oxygen differences and the surfactant concentration. Line 243 was rephrased for clarification.

Changes in manuscript:

Line 243: […] The *oxygen differences were not significantly correlated with the surfactant concentration ($r = 0.215$, $p = 0.300$)*, which started to increase at the bloom's peak and reached its maximum after the chlorophyll *a* and oxygen *difference* maxima. […]

Referee comment 15:

Line 256: did you test lagged correlations? Wouldn't that be something to consider?

Response:

We now tested for lagged correlations. For the oxygen difference, a 3-day lag increased the correlation with surfactant concentration to a strong one ($r = 0.703$, $p < 0.001$). The same was true for the diffusive rates; with a 3-day lag, the correlation with surfactant concentration increased to a moderate level ($r = 0.668$, $p < 0.001$). The gas exchange rate showed an increased correlation with the surfactant concentration, with a 2-day lag to a strong correlation ($r = 0.788$, $p < 0.001$), and it showed an increased correlation with the zooplankton abundance, with a 4-day lag to a strong correlation ($r = 0.857$, $p < 0.001$). All other parameters showed no significant increase in correlation with other parameters when calculated with a time lag. We added these information in Sections 3.2, 3.4, and Appendix D.

It is, however, unlikely that these stronger lagged correlations, mostly with surfactants, have a direct causal relationship with the parameters, since the surfactant maxima occur after the peaks, e.g., in oxygen differences. The higher correlation is somewhat due to the relationship between chlorophyll *a* and surfactants, and to the correlations of the different parameters with chlorophyll *a*. When the chlorophyll *a* concentration starts to decrease towards the end of the bloom, surfactants are released by the phytoplankton. Thus, the surfactant peak follows the chlorophyll *a* peak. Parameters strongly correlated with chlorophyll *a* also show a strong correlation with surfactants when the lag between the end of the bloom and the release of surfactants is accounted for. It is, however, doubtful, that a peak in oxygen differences or gas exchange rate is the cause for the peak in surfactant concentration.

Changes in manuscript:

Line 243: […] *When a lagged correlation was calculated between oxygen differences and surfactant concentration with a 3-day lag, the correlation strengthened significantly ($r = 0.703$, $p < 0.001$). Unless otherwise noted, other correlations did not significantly increase when lagged.* […]

Line 282: […] The gas exchange rate showed a moderate correlation with the phytoplankton abundance ($r = 0.559$, $p = 0.004$) *and the surfactant concentration ($r = 0.588$, $p = 0.002$)*, but not with zooplankton abundance ($r = 0.324$, $p = 0.114$) or chlorophyll *a* concentration ($r = 0.384$, $p = 0.059$). *With 2-day lag, the correlation with the surfactant concentration increased to a strong and significant correlation ($r = 0.788$, $p < 0.001$), while with a 4-day lag, the correlation with the zooplankton abundance increased to a strong and significant correlation ($r = 0.857$, $p < 0.001$).* The oxygen saturation followed a similar trend […]

Line 537: […] but not with the surfactant concentration ($r = 0.239$, $p = 0.250$) (Fig. D1). *With a 3-day lag, the correlation between the diffusive rate and surfactant concentration increased to a moderate and significant level ($r = 0.668$, $p < 0.001$).* The mean diffusive rate […]

Referee comment 16:

Figure 7: what are the units for plankton and bacterial abundance? It's interesting that the increase in bacterial abundance in the SML seems delayed compared to the increase in the ULW. Any suggestions as to why?

Response:

The units for plankton and bacterial abundance are cells L^{-1}. This was added accordingly in Figure 7 and Figure C1. The increase in bacterial abundance in the post-bloom phase might be due to the release of DOM at the end of the phytoplankton bloom (see also Bibi et al., 2025a). Because most phytoplankton reside in the ULW, more DOM is released there, and the increase in bacteria taking up the DOM occurs first in the ULW.

Other colleagues are focusing on bacterial processes during the mesocosm experiment. Their first results show, that factors affecting the increase in cell abundance are different in SML and ULW, as the presence and proportion of the bacterial groups are different (Athale et al., in preparation). The increase of ULW bacteria from 06 to 09 June was due to an increase in bacterial groups already prevalent during the bloom phase. The increase in SML bacteria from 09 to 12 June was due to the stark increase in an order of Gammaproteobacteria, which was related to the biofilm-like nature of the SML in the post-bloom phase (Athale et al., in preparation; Rahlff et al., 2023). A short explanation was added in line 274, and the references were added accordingly.

Changes in manuscript:

Line 274: […] and 1.68 x 109 cells L^{-1} (ULW). *The earlier increase in the ULW was partly caused by the release of dissolved organic matter in the ULW at the end of the phytoplankton bloom, which was consumed by the bacteria (Bibi et al. 2025a). The shift between SML and ULW can be explained by the presence and proportion of different bacterial groups. In the ULW, bacterial communities were already prevalent during the bloom phase and increased in abundance. In contrast, in the SML Gammaproteobacteria associated with the biofilm-like properties of the SML lead to a later increase in bacterial abundance (Athale et al., in preparation; Rahlff et al., 2023).* Before the bloom phase […]

Line 605: *Athale, I., Spriahailo, D., Bowen, S., Singh, G., Poehlein, A., Daniel, R., Reinthaler, T., and Brinkhoff, T.: Microbial community dynamics in the sea-surface microlayer of a mesocosm during an induced phytoplankton bloom, in preparation.*

Line 706: *Rahlff, J., Wietz, M., Giebel, H.-A., Bayfield, O., Nilsson, E., Bergström, K., Kieft, K., Anantharaman, K., Ribas-Ribas, M., Schweitzer, H. D., Wurl, O., Hoetzinger, M., Antson, A., and Holmfeldt, K.: Ecogenomics and cultivation reveal distinctive viral-bacterial communities in the surface microlayer of a Baltic Sea slick, ISME Commun., 3, 1, https://doi.org/10.1038/s43705-023-00307-8, 2023.*

For changes to Figure 7 and C1 see end of this manuscript.

Referee comment 17:

Section 3.5: see comment above regarding gradient versus difference.

Response:

Temperature gradients were also changed to temperature differences throughout the text.

Changes in manuscript:

Changes from 'temperature gradient' to 'temperature difference' throughout the text and Figures.

Referee comment 18:

Line 298: a difference in temperatures between sensors of 0.142 deg C is quite a lot – or is this actually the rate? Any comment on that?

Response:

The deviations between the sensors were not due to differences between the two sensors measuring the temperature of the same water mass. They were the deviation between the measured temperature differences across the TBL. The 0.142 °C deviation was an extreme case, possibly due to outliers, while the mean deviation in temperature differences between the two sensors was only 0.048 ± 0.036 °C.

There were several reasons for the deviations, other than measurement inaccuracies. The sensors were positioned a few centimetres apart, and, as explored in Section 3.7, over this distance, large surface temperature gradients could occur, which were well within the range observed between the sensors. Small-scale processes such as buoyancy fluxes could change the shape of the profile very locally and thus the measured temperature differences. To investigate these processes, our group is currently conducting further experiments with an optical schlieren system. Such a system shows small-scale convection near the surface and demonstrates on which scales they occur.

The microsensors are handmade due to their size and fragility. While they do not match the accuracy of conventional temperature sensors used in oceanography, they are very suited for studying micro-scale surface temperature profiles. Our study would not have been possible with larger, possibly more accurate temperature sensors. We added a sentence in line 299 for clarification.

Changes in manuscript:

Line 298: […] The *deviations* in mean temperature *differences* between both sensors ranged from 0.001 °C to 0.142 °C, with a mean of 0.048 ± 0.036 °C. *These deviations were not caused by measurement inaccuracies, but by small scale processes such as buoyancy fluxes. These processes alter the shape of the profiles locally and leading to a larger variability, as has also been observed in temperature microprofiles by Ward and Donelan (2006). High medians and interquartile ranges, such as on 29 May and 02 June, were caused by two outliers with a high* *temperature difference per night respectively, but do not indicate a general trend for higher temperature differences during the bloom phase.* The mean temperature [...]

Referee comment 19:

Figure 8: see comment above regarding gradient versus difference. I find the difference between the sensors quite large. How do you assess the reliability of the sensors when they actually diverge so much?

Response:

The deviation between the sensors was not necessarily due to sensor offsets; it could have been caused by small-scale processes that influenced the profiles locally, as the sensors were positioned few centimetres apart. The use of multiple sensors provided more representative measurements over a larger area of several cm^2. It ensured continuous data acquisition if a sensor failed or was mounted too deep to reach the water surface. Minor deviations between sensors are not among our criteria for assessing sensor reliability; instead, we focus on relatively stable measurements, few outliers, and a well-positioned sensor tip close to the water surface. Of course, all microsensors were tested for functionality before use in our study. At the observed microscales, we measure significant deviations that cannot be explained by the lack of sensor performance, but due to dynamic processed across the air–sea boundary layer.

Changes in manuscript:

No changes in text.

Referee comment 20:

Section 3.7 and Figure 10: This pattern is at the surface. Do you expect similar patterns and horizontal gradients of similar magnitude at «depth», e.g. within the surface microlayer, at the bottom of the TBL, or in the ULW?

Response:

The occurrence of these patterns is more likely at the surface, as material accumulated, broke up, and lead to distinct patterns in surface temperatures. These patterns likely occur throughout the SML/TBL. Going deeper, it becomes less likely that these sharp patterns and strong gradients existed because the water flow by the pumps lead to a more uniform water mass in the ULW. However, temperature gradients in the ULW across a larger area within the mesocosm facility cannot be ruled out entirely. Temperature measurements with a similarly high horizontal resolution are currently only possible at the surface, not at deeper depths, so this remains speculation. A sentence concerning this uncertainty was added in the discussion in line 388.

Changes in manuscript:

Line 388: […] but on the mean from a larger area. *The extent, to which these distinct horizontal temperature distributions continue beyond the sea surface into the ULW remains unclear, as high-resolution thermal images are limited to the upper 10–20 µm of the water surface.* […]

Referee comment 21:

Line 348: The dissolved oxygen concentration profiles in Figure 4 and the temperature profiles in Figure E1 suggest that you did not fully exclude diurnal warming.

Response:

Yes, as the facility is outside, diurnal warming could not be entirely excluded, but using night-time profiles at least reduced this effect. Only cooling down throughout the night is included in the profiles; heating up from solar radiation is not. Line 348 was rephrased accordingly.

Changes in manuscript:

Line 348: […] night-time microprofiles were analysed. *This excluded atmospheric influences, such as solar radiation and wind forcing at the water surface, and reduced the effect of diurnal warming to a minimum.* […]

Referee comment 22:

Line 364: add «that» before «Rahlff et al».

Response:

Line 364 was changed accordingly.

Changes in manuscript:

Line 364: […] 1100 µm thick DBL, *that* Rahlff et al. (2019) […]

Referee comment 23:

Line 378: you did not show/present anything about skin layer temperatures before this sentence, this statement comes a bit out of the blue.

Response:

The sentence was rephrased to introduce the cool skin before the comparison with our data.

Changes in manuscript:

Line 377: […] *A colder water surface compared to subsurface water, called "cool skin layer", is commonly observed with temperature differences between –0.1 °C and –0.2 °C (Donlon et al., 1999; Murray et al., 2000; Donlon et al., 2002; Minnett et al., 2011; Jaeger et al, 2025).* Our in situ measurements of *the mean temperature difference* of –0.133 ± 0.079 °C *are consistent with these measurements.* Most of these *common* observations, […]

Referee comment 24:

Line 381: what kind of values?

Response:

The temperature difference across the TBL. The sentence was changed for clarification.

Changes in manuscript:

Line 381: […] Our measurements indicate that *a cool skin layer can also be present at night, consistently reaching a temperature difference comparable to field conditions* due to net heat loss, *even under mostly calm conditions.* […]

Referee comment 25:

Line 394: Such an impact would not be expected, would it?

Response:

No, a direct impact of the bloom on the temperature was not expected. However, these data show that there is also no indirect effect of the bloom on the temperature, e.g., through surfactant production. A sentence was added in Line 395.

Changes in manuscript:

Line 395: […] no direct impact of the bloom. *While such a direct impact of the bloom on the temperature was not expected, these results show that there was no indirect impact of the bloom on the water temperature, e.g., through surfactant production.* We suggest that the temperature *differences* were limited […]

Referee comment 26:

Line 408: Suggest to delete «energy exchanges, like» since you suggested in the introduction that surfactants do have an effect on momentum exchange.

Response:

Here, energy exchanges were used to underscore the contrast with gas exchanges, which are exchanges of matter, not energy. It was deleted to simplify the sentence.

Changes in manuscript:

Line 408: […] but less *effective for heat exchange*. […]

Referee comment 27:

Line 410: faster than what?

Response:

Faster than oxygen. Sentence changed accordingly.

Changes in manuscript:

Line 410: *[…] Heat, however, is exchanged* much faster *than oxygen*, because the thermal diffusion coefficient in water is by two magnitudes greater than the oxygen diffusion coefficient (Bindhu et al., 1998; Ambari et al., 2022). […]

Referee comment 28:

Lines 411-413: But the temperature gradient did not change with surfactant concentrations – I'm not sure what you are trying to argue here?

Response:

We try to explain why heat transfer is less affected by surfactants than oxygen transfer. The reason is the higher heat-diffusion coefficient in air compared to water, which leads to faster exchange between air and water. Due to these much faster exchanges, the effect of the surfactants in reducing heat transfer is much less apparent than that in reducing oxygen transfer. We rephrased lines 410-413 for more clarity.

Changes in manuscript:

Line 410: […] along concentration gradients (Goldman et al., 1988; Wurl et al., 2011; Rahlff et al., 2019). *Heat, however, is exchanged* much faster than oxygen, because the thermal diffusion coefficient in water is by two magnitudes greater than the oxygen diffusion coefficient (Bindhu et al., 1998; Ambari et al., 2022). *Due to the faster heat exchanges, surfactants might be less effective in reducing air–sea heat exchanges than oxygen exchanges.* […]

Referee comment 29:

Figure A1: There is quite a large spread in values for each sensor on short and slightly longer timescales. It's quite difficult to see and assess how they compare with all these different patterns. Maybe plot the deviation from the Ferrybox measurements to at least reduce the daily cycle?

Response:

The microsensor data from this experiment were now filtered using a Hampel filter with a 10-minute window, which replaces outlier values that differ from the median in that window by more than three standard deviations with the median. This way, the data appear smoother, making comparisons with the FerryBox data easier. We decided against plotting the differences from the FerryBox to show better the jumps in oxygen concentration that occurred during calibration. Figure A1 was updated, including a new more grey-scale-friendly colour scale, and a brief description of the filter was added in line 476.

Changes in manuscript:

Line 476: […] and water temperature. *The microsensor data were filtered for clarity using a Hampel filter, replacing outliers within a 10-minute window that differed from the median by more than three standard deviations with the median value.* The results showed […]

For changes to Figure A1 see end of this manuscript.

Referee comment 30:

Lines 489-490: how often did you take the discrete samples? Would they resolve changes due to diurnal variations?

Response:

The discrete samples were collected daily, alternating between early morning and afternoon, to account for diurnal changes. Thus, water was either exposed to darkness or to light before sampling. A more frequent sampling was not performed to keep the integrity of the SML. For this reason, no diurnal changes on single days were resolved. However, the alternation between early morning one day and late afternoon the next day helped to resolve general diurnal changes between these two conditions. A sentence was inserted in line 490.

Changes in manuscript:

Line 490: […] analysed with the Winkler method. *These discrete samples were taken daily at alternating times (one day early morning, the next day afternoon) to account for diurnal changes in oxygen concentration.* The inlet of the FerryBox […]

Referee comment 31:

Line 491: Which depth range in the ULW?

Response:

The mean of the data from H_{DBL} to the end of the profile was taken, so between approximately 1 mm and 7 mm (depending on sensor positioning and profile depth).

Changes in manuscript:

Line 491: […] The comparison of the mean microsensor measurements from the ULW *(ranging approximately between 1 mm and 7 mm)* and FerryBox shows […]

Referee comment 32:

Line 504: did you take samples for calibration of the salinity sensor? Could evaporation affect the accuracy of your salinity measurements?

Response:

The evaporation increased the water's salinity by 3.1 g kg^{-1} throughout the experiment (Bibi et al., 2025a). This increase, however, had no direct effect on the measurement's accuracy. No salinometer was available, but conductivity sensors were calibrated by the manufacturer according to the specifications prior the mesocosm experiment.

Changes in manuscript:

No changes in text.

Referee comment 33:

Line 511: Yes, but should bacterial abundance lead to a decrease in oxygen saturation?

Response:

An increase in heterotrophic bacteria would lead to a decrease in oxygen saturation; an increase in phototrophic bacteria like cyanobacteria, however, would lead to an increase in oxygen saturation. The later increase in oxygen saturation was likely due to the formation of algal and bacterial biofilms on the floor and walls of SURF and on the submerged equipment during the later post-bloom phase. These biofilms likely contained a lot of phototrophic organisms, which lead to an increase in oxygen production, even after the main phytoplankton bloom in the ULW declined. While we did not further analyse these biofilms, other research showed a strong oxygen production in biofilms by e.g. cyanobacteria (Kühl et al., 1996; Pringault and Garcia-Pichel, 2000). The sentence in line 511 was changed and references added accordingly.

Changes in manuscript:

Line 511: […] This also coincided with the post-bloom increase in bacterial abundance. *The increased post-bloom oxygen saturation can be attributed to the formation of biofilms on the floor, walls, and submerged equipment of SURF. These biofilms have been shown to contain* *cyanobacteria, which contribute to increased oxygen production (Kühl et al., 1996; Pringault and Garcia-Pichel, 2000).* The mean oxygen saturation […]

Line 683: *Kühl, M., Glud, R. N., Ploug, H., and Ramsing, N. B.: Microenvironmental control of photosynthesis and photosynthesis-coupled respiration in an epilithic cyanobacterial biofilm, J. Phycol., 32, 799-812, https://doi.org/10.1111/j.0022-3646.1996.00799.x, 1996.*

Line 703: *Pringault, O., and Garcia-Pichel, F.: Monitoring of oxygenic and anoxygenic photosynthesis in a unicyanobacterial biofilm, grown in benthic gradient chamber, FEMS Microbiol. Ecol., 33, 251-258, https://doi.org/10.1111/j.1574-6941.2000.tb00747.x, 2000.*

Referee comment 34:

Appendix E: with the above noted discrepancies between the temperature sensors, how do the profiles with depth compare?

Response:

In Figure E1, a subplot showing the temperature sensor 2 profiles was added. Overall, the profiles of both sensors appeared very similar. The only notable deviations were a slightly different mounting height and approximately 0.1 °C difference in the measured temperature, which was within the resolution of the sensors. Appendix E was expanded, and Figure E1 was changed to show the profiles of both sensors.

Changes in manuscript:

Line 551: The temperature microprofiles on 22 May give a typical example of the profiles *from both temperature sensors* during all nights (Fig. E1). The warmest profile was the first of the night, and all the subsequent profiles were approximately *0.02 °C to* 0.05 °C cooler than the preceding profile. The air temperature decreased as it moved *farther* from the water surface; it also decreased from profile to profile during the night and was always lower than the water temperature. A cooler thermal boundary layer was present in all profiles. *Sensor 2 measured up to 0.1 °C higher temperatures and was mounted approximately 750 µm deeper than sensor 1. These slight deviations in mounting height and measured temperature were also observed in profiles on other nights, while the overall shape of the profiles remained very similar between both sensors.* The only significant *differences between nights for each sensor were the mounting height and* the absolute temperature, which typically rose from night to night, starting at approximately 18.9 °C on 22 May and reaching around 22.6 °C on 15 June.

For changes to Figure E1 see end of this manuscript.

Referee comment 35:

Figure E1: as for Figure 4, please choose a better colour scheme.

Response:

The colours in Figure E1 were changed to a more colour-blind and grey-scale-friendly palette.

Changes in manuscript:

For changes to Figure E1 see end of this manuscript.

Comments second referee

Referee comment 1:

First of all, I am surprised that the authors would describe the difference between the measured parameters at the top and the bottom of the relevant BL as the gradient. Based on the units, they are showing the difference, not the gradient. This needs to be addressed and fixed.

Response:

We changed the word "gradient" to "difference", when describing the differences across the DBL/TBL. The word "gradient" was left only when specifically referring to a difference over a distance.

Changes in manuscript:

Changes from "oxygen/temperature gradient" to "oxygen/temperature difference" throughout the text, Figures and title of the manuscript.

Referee comment 2:

My main concern is with the comparison of the TBL findings to corresponding published values for the bulk-skin temperature difference (dT) and TBL thickness based on field experiments. The results are presented as calm night-time conditions but the apparent characteristics of the facility and the IR imagery suggests otherwise.

Response:

The water mass was not stagnant, and constantly but slowly moved with operation of small flow pumps inside SURF's basin to avoid settling of plankton. However, the construction of SURF allows to exclude wind. Because of the slow flow of the water and particle aggregations at the surface, the IR camera images may not be comparable to images from entirely calm water masses. As the mesocosm study was an interdisciplinary study, combining many different needs and methods, it was not possible to constantly simulate absolutely calm conditions, as would be possible with laboratory experiments. Completely stagnant conditions were not a goal of this study, as they are very unlikely to be found in the ocean. The mesocosm setup provided a combination between conditions nearer to the field and measurements otherwise only possible in the lab.

The goal of this study was to investigate the influence of a phytoplankton bloom on the surface water under controlled conditions to obtain a mechanistic understanding of the DBL and TBL. The capillary waves observed during the day made an analysis of the microprofiles at presented vertical scales challenging. This study focusses primarily on the analysis of the night time profiles, when no wind and capillary waves were present due to the closure of the basin by the roof and walls. The design of the study was not entirely dedicated to study the TBL under absolutely calm conditions. The study was furthermore conducted under much larger temporal and spatial scales as other studies of the TBL which makes a one-to-one comparison with those studies in different settings difficult. Our results might be closer to field conditions, where natural plankton blooms occur and surface currents might also lead to a flow of the surface water, even under low-wind conditions and such conditions cannot be represented in small-scale laboratory studies.

To better account for the limitations of the setup compared to field studies or dedicated laboratory studies, we have revised parts of the discussion and conclusion to more accurately reflect the non-calm conditions and have tempered comparisons with other studies.

Changes in manuscript:

Line 349: […] *Despite calm conditions without wind, the flow pumps generated light currents, which prevented a fully stagnant water column.* Both oxygen and temperature microprofiles […]

Line 351: […] and gas exchange rates, which were *typically measured indirectly in previous studies.* […]

Line 375: […] assume a TBL thickness of around 1 mm (Donlon et al., 2002; Jaeger et al., 2025). *Our mean TBL thickness of 1299.5 ± 391.8 µm is consistent with these recent measurements, but significantly thicker than the 300 µm thick thermal surface layer reported by GESAMP (1995). It has been shown, that increasing wind speeds above 2 m s^{-1} lead to a decrease in TBL thickness (Ginzburg et al., 1977; Sromovsky et al., 1999; Murray et al., 2000; Wong and Minnett, 2018). As our measurements are conducted without wind, a slightly thicker TBL is expected than those reported in the literature. For example, Ward and Donelan (2006) implemented temperature microprofiles under changing wind speeds and air–water temperature differences. They measured a strong decrease in TBL thickness at increasing low wind speeds, from 2.16 mm at 1 m s^{-1} to 0.55 mm at 3 m s^{-1} for a warm TBL, and a 0.44 mm thick colder TBL with a wind speed of 4 m s^{-1}. While our TBL thickness is as expected higher than their thicknesses under increasing wind speeds, it is lower than their warm TBL thickness at the lowest wind speed. While Ward and Donelan used freshwater without surfactants and a different microprofile analysis method, their results indicate for our experiment not only a strong thinning of the TBL with the onset of low winds, but also differences between warm and cold TBL thicknesses at similar conditions.*

A colder water surface compared to subsurface water, called "cool skin layer", is commonly observed with temperature differences between –0.1 °C and –0.2 °C (Donlon et al., 1999; Murray et al., 2000; Donlon et al., 2002; Minnett et al., 2011; Jaeger et al, 2025). Our in situ measurements of *the mean temperature difference* of –0.133 ± 0.079 °C *are consistent with these measurements*. Most of these *common* observations, however, were made in field conditions, where wind enhances the cool skin effect. Our measurements indicate *that a cool skin layer can also be present at night, consistently reaching a temperature difference comparable to field conditions due to net heat loss, even under mostly calm conditions. A temperature equilibrium between air and water was not achieved due to the continuing diurnal changes in air temperature which affected the water surface temperature even with a closed roof*. Common parametrisations of the cool skin effect, solely based on wind speed, *are often not defined for low wind regimes due to a lack of observational* (Donlon et al., 2002; Minnett et al., 2011). Other parameters influencing the net heat flux, such as buoyancy fluxes, *water surface temperature* or air temperature and humidity, must be *additionally* considered when calculating cool skin effects at very low wind speeds *(Soloviev and Schüssel, 1994; Fairall et al., 2003; Minnett et al., 2011).* The heterogeneous temperature distribution we detected with the IR camera under slick conditions, which also occurs under rain conditions (Wurl et al., 2019), highlights the need to base these calculations not only on measurements from a single point, but on the mean from a larger area. *The extent, to which these distinct horizontal temperature distributions continue beyond the sea surface into the ULW remains unclear, as high-resolution thermal images are limited to the upper 10–20 µm of the water surface.* […]

Line 448: […] This mesocosm study further shows the need to *expand* cool skin parametrisations *to low wind conditions*. Uncertainties in these parametrisations […]

Line 469: […] valuable for gathering in situ data on previously *scarcely measured* properties of the SML […]

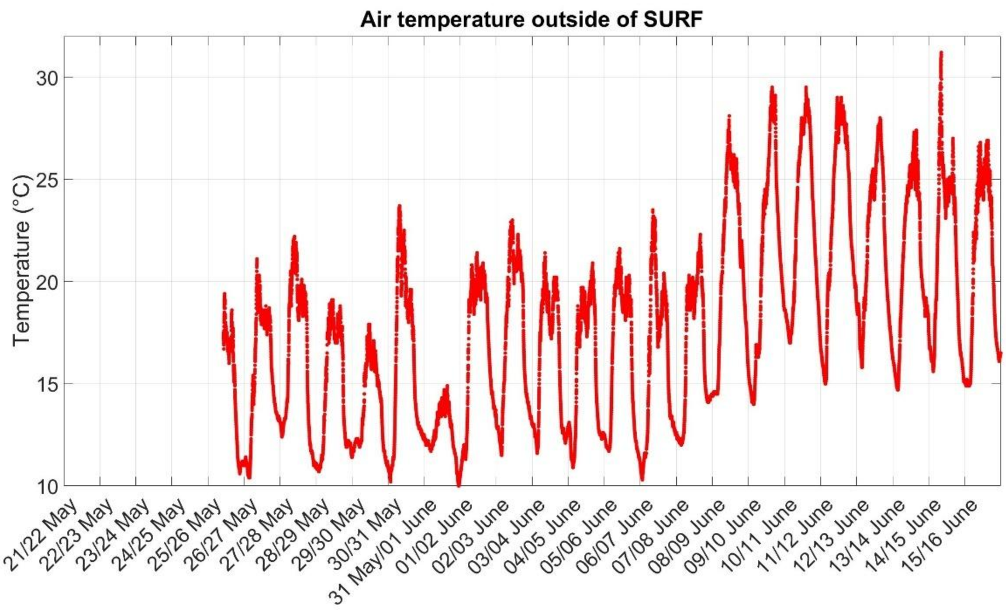
Referee comment 3:

The mesocosm appears to be an outdoor wave tank with a retractable roof which was used to exclude wind and rain effects. The photograph in Fig. 1 is an inadequate representation of the setup and the reader should not have to go to other publications to figure out the facility details. From the photos in the referenced publications of Bibi et al. and Gassen et al., it appears that the "roof" does not block off the openings at the ends of the tank. If that is the case, then there may still be some effect of air movement over the tank. I suspect the ends are open since a dT was observed – if it was sealed then the water and air would eventually come into equilibrium and there would be no dT.

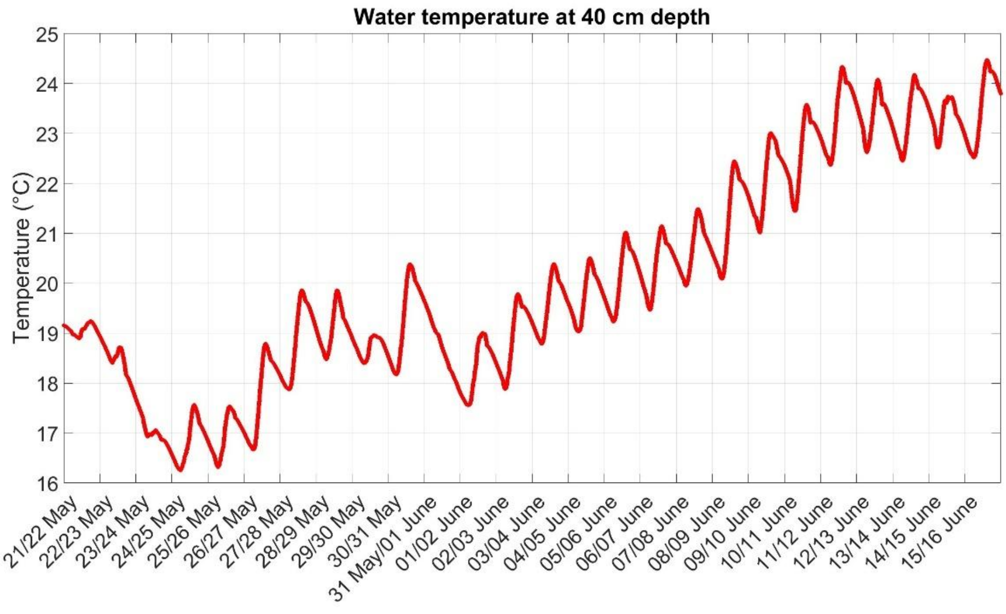
Response:

We added additional photos to Figure 1 to give a better overview of the mesocosm facility with the roof closed. The front and rear of the facility have walls, which were removed, when the roof was opened. When the roof was closed, the walls on both sides were installed to close the facility and, therefore, prevented wind from freely blowing across the facility and a wind speed of close to zero could be assumed for the analysed profiles.

An equilibrium in dT was not achieved due to the diurnal air temperature changes. While atmospheric measurements inside the mesocosm facility were not available due to sensor failure, weather station measurements outside the facility showed diurnal air temperature changes from 5 °C to 15 °C with continuing temperatures changes during the nights. Due to these constantly changing air temperatures, no equilibrium with the water temperatures was achieved and a dT was observed. This is also apparent in the continuous CTD temperature measurements in the ULW at 40 cm depth, which show consistent changes without plateaus. Data of the air temperature were not included in the manuscript, as the air temperature from outside of SURF was not exactly matching the air temperature inside of SURF. Plots of the air temperature outside of SURF and continuous CTD water temperature measurements at 40 cm depth are added below. A sentence in line 381 was included to describe the absence of a temperature equilibrium and the closure of SURF.



Air temperature outside of SURF during the mesocosm study, gap at the start due to missing data.



Water temperature at 40 cm depth measured by a CTD during the mesocosm study.

Changes in manuscript:

For changes to Figure 1, see end of this manuscript.

Line 119: […] The roof of SURF was closed at night to exclude wind and rain effects, allowing examination of only the phytoplankton bloom's influence on oxygen concentration and temperature, and their *differences* across the SML. *While the closed roof and walls prevented wind from freely blowing across the water surface, the interior of SURF continued to be affected by fluctuations in outside temperature due to heat transfer through the roof.* […]

Line 348: […] night-time microprofiles were analysed. *This excluded* atmospheric influences, such as solar radiation and wind forcing at the water surface, *and reduced the effect of diurnal warming to a minimum.* […]

Line 381: […] more than –0.3 °C due to net heat loss. *A temperature equilibrium between air and water was not achieved due to the continuing diurnal changes in air temperature which affected the water surface temperature even with a closed roof.* Common parametrisations […]

Referee comment 4:

The mesocosm included the use of an array of pumps at the bottom of the tank that continuously circulated water. From Bibi et al., it appears that the circulation was horizontal, but I suspect that this subsurface forcing had some impact on the surface. IR imagery of a calm surface with a net upward heat flux should clearly show convection cells – see publications by Saylor (web examples at https://cecas.clemson.edu/~jsaylor/saylor.rsch.irHiRes.html), which also address the issue of slicks. The IR image in Fig 10 looks more like measurements in a wind flume at low wind speed such as in publications by Jahne and colleagues, Veron, Marmorino and Smith, etc. The authors suggest that the appearance is due to slicks but they do not look like published images of slicks.

Response:

The pumps generated a slow constant flow of the surface water. This flow might have prevented the appearance of convective cells. The distinct pattern in Figure 10 is unlikely caused by wind, as wind was absent, but by accumulations of organic material forming at the surface. These accumulations formed during the natural progression of the phytoplankton bloom and were strongest during the post-bloom phase. They changed the surface temperature very locally and may also have overlaid possible convection cells. A photo of one of these accumulations from the 09 June (during the day with an open roof) is added below.

Slicks in laboratory experiments are often formed by artificial monolayers like oleyl alcohol (Saylor et al., 2000; Flack et al., 2001; Saylor et al. 2001). Natural slicks however, consist of a complex gelatinous matrix rich in particulate material (Wurl et al., 2016). For this reason, it is likely that the natural slick observed in the mesocosm appeared different on the IR camera image than previously published artificial slicks. Convection cells might not be as clearly formed in natural slicks or might be masked by other processes.

Similarities to wind flume measurements might be caused by the slow flow of the water. Significant wind inside the mesocosm facility can be excluded, not only due to the closed roof and walls of SURF, but also due to a low wind speed of approx. 2 m s^{-1} measured outside of SURF during the time the IR image was taken, which makes wind entering the facility through any gaps (as not completely sealed) in the structure unlikely.

The focus of this manuscript lies on the microsensor measurements, and the IR camera image provides supporting information about the heterogenous surface temperature distribution, which helps to explain deviations between the temperature sensors. A detailed analysis of the IR camera images during the whole mesocosm study, showing the effects of the formation of the natural slick on the temperature distribution, effects on possible convection cells or wind effects on the slick would require a dedicated extensive analysis, which cannot be provided within the scope of this manuscript. We included in the discussion a short comparison of the IR camera image with surface temperature images from other studies and added the references accordingly.

Water surface on the 09 June with visible accumulations of material at the surface.

Changes in manuscript:

Line 413: […] *The natural surfactants produced during and after the bloom, lead to the formation of a slick, which altered the horizontal surface temperature distribution. Material floating on the slick-like water surface lead to very heterogenous surface temperatures. Our approach differs from laboratory studies that use artificial surfactants to generate surface slicks via monolayers. These studies showed that under calm conditions, slicks lead to more homogeneous surface temperatures (Saylor et al., 2000; Flack et al., 2001; Saylor et al., 2001). These different approaches lead to inconsistent results between laboratory and field studies, which mesocosm studies can close by bringing closer-to-field conditions into laboratory-like experiments.* […]

Line 653: *Flack, K. A., Saylor, J., R., and Smith, G. B.: Near-surface turbulence for evaporative convection at an air/water interface, Phys. Fluids, 13, 11, 3338-3345, https://doi.org/10.1063/1.1410126, 2001.*

Line 722: *Saylor, J. R., Smith, G. B., and Flack, K. A.: The effect of a surfactant monolayer on the temperature field of a water surface undergoing evaporation, Int. J. Heat Mass Transfer, 43, 3073-3086, https://doi.org/10.1016/S0017-9310(99)00356-7, 2000.*

Saylor, J. R., Smith, G. B., and Flack, K. A.: An experimental investigation of the surface temperature field during evaporative convection, Phys. Fluids, 13, 2, 428-439, https://doi.org/10.1063/1.1337064, 2001.

Referee comment 5:

There seems to be a significant difference between the measurements of the two temperature sensors. Sensor 2 shows a much larger quartile range in Fig. 8 that seems to vary with time (smaller pre- and post-bloom, larger during bloom.) The maximum difference of 0.142 C seems to be significant and should be addressed. The authors indicate that the horizontal differences in the IR imagery can explain this but the IR image is a snapshot of spatial features that presumably are varying in time while the temperature probes are average over time at a point. If the horizontal features change then it seems like they would be averaged out in time for the T sensors. I also don't think you can assume that the surface IR features are representative of the temperature in the TBL since the IR optical depth is O(10 microns).

Response:

Typically, five to seven profiles were measured per night, and outliers could have led to a larger interquartile range and deviations in the median and mean. While the deviation of 0.142 °C was large, it represented an outlier in the measurements as the overall mean deviation between both sensors was significantly smaller (0.048 ± 0.036 °C). On 02 June, the night with the large deviation of 0.142 °C, two of the five measured profiles of Sensor 2 had exceptionally high values, leading to a large overall mean. In the same night, the measurements of Sensor 1 fell in the lowest range during the whole study. Both of these factors lead to an unusually high deviation of both sensors.

The larger interquartile range of Sensor 2 during the bloom phase was mainly caused by two outliers on 29 May and 02 June, and overall there was no consistent trend for larger interquartile ranges of Sensor 2. This is apparent in the means of the interquartile ranges of Sensor 2, which were higher during the bloom phase (0.095 ± 0.080 °C) compared to the pre-bloom (0.069 ± 0.056 °C) and post-bloom phases (0.081 ± 0.054 °C). However, the mean for the bloom phase reduced to 0.057 ± 0.021 °C by excluding the two outliers. We addressed the higher interquartile range in Section 3.5.

The deviations between both temperature sensors are not differences in the measured temperature of the same water mass. They are temperature differences derived from temperature microprofiles, which were measured a few centimetres apart. Small scale processes, like convection, buoyancy fluxes or slick edges can occur on these small spatial and temporal scales leading to locally different shapes of the profiles and absolute values of both sensors. The influence of convection, leading to a significant variability in temperature microprofiles has also been reported in Ward and Donelan (2006). Our group is currently conducting an optical schlieren system experiment, visualising convective processes on small scales of few millimetres, which will be published elsewhere.

The IR camera measured the upper 10–20 µm of the water surface. These measurements were therefore not representative for the temperature across the TBL. The temperature of the TBL, however, was not constant, but characterized by a gradient from the temperature at the uppermost surface to the temperature in the ULW. The surface temperature, which was measured by the IR camera, contributed to the shape of the temperature profile and the measured temperature difference. Spatial features in surface temperature observed by the IR camera might thus have contributed to locally changing the shape of the microprofiles and the temperature differences. And while the whole microprofiles took 40–50 minutes to complete, the time the sensors measured near the surface were in the range of 4–5 minutes (for measurements across 1000 µm) or around 30 s for one vertical step. Even short-lived spatial features at the surface could significantly alter the measured profiles on these timescales of approximately five minutes. We expanded Sections 3.7 to account for this.

Changes in manuscript:

Line 298: […] The *deviations* in mean temperature *differences* between both sensors ranged from 0.001 °C to 0.142 °C, with a mean of 0.048 ± 0.036 °C. *These deviations were not caused by measurement inaccuracies, but by small scale processes such as buoyancy fluxes. These processes alter the shape of the profiles locally and leading to a larger variability, as has also been observed in temperature microprofiles by Ward and Donelan (2006). High medians and interquartile ranges, such as on 29 May and 02 June, were caused by two outliers with a high temperature difference per night respectively, but do not indicate a general trend for higher temperature differences during the bloom phase.* The mean temperature [...]

Line 332: […] of a few centimetres. *While the IR camera only measured the temperature at the uppermost water surface (10–20 µm thickness) and not the temperature across the whole TBL, the water surface temperature still determined the shape of the microprofiles and thus the measured temperature differences across the TBL. The* horizontal surface temperature differences support the observed temperature *deviations* measured by both temperature microsensors *through this very local changing of the temperature microprofiles.* On 11 June, […]

Referee comment 6:

I think the comparisons of dT and the TBL thickness in sec. 4.1 to field results need to be less definitive. The authors state that their measurements confirm the TBL thickness of O(1 mm) reported by Donlon et al. and Jaeger et al., but both of those papers address field conditions. The authors also state that their measurements confirm the commonly cited -0.1 to -0.2 C dT but that result is for wind speeds above 6 m/s or so. I think it is not justified to state that the results show that the cool skin effect in the absence of wind are overestimated by the field measurements. I looked at Donlon et al. and Minnett et al. and could not find the implied values of -0.44 C and -0.77 C under calm conditions. If they came from scatter in the dT vs U plots in those refs that is not an appropriate comparison. The authors are correct that other factors besides wind affect dT, esp. heat flux. But unfortunately there is no information about the heat flux for the mesocosm. Was relative humidity measured? If so, you could compute the specific humidity difference and look at the dT vs dq correlation as in Yan et al. [2024 DOI: 10.1175/JPO-D-23-0103.1] since the latent heat flux likely dominated the dT forcing. The dT treatment would have been significantly improved by having heat flux measurements. I suggest using language like “consistent with” or “differs from” and avoid stating that this one simple, limited experiment under unquantified heat flux conditions confirms or shows errors in published field studies.

Response:

We agree, that the comparisons with the literature were too definitive. The comparisons with field measurements were not meant to directly compare with measurements under the same conditions, as field measurements without wind are very sparse. The comparisons were rather described to generally compare our in situ measurements of the TBL thickness and dT with the field measurements which often rely on IR surface temperature measurements and measurements from the ULW to compute the TBL thickness and dT. So, while we cannot confirm field measurements at similar conditions, we show that these often indirect measurements deliver results that are in the same order of magnitude to our in situ measurements in the mesocosm. It provides some general overall confidence in the results. To more directly compare our measurements to field measurements, we would need to repeat the experiment under differing wind conditions, which makes the profile analysis due to dynamics at the surface very challenging.

We agree with the reviewer, that the comparison with Donlon et al. and Minnett et al. might be misleading. This comparison should in no way state that their work is inaccurate. The dT of –0.44 °C and –0.77 °C for no wind, while not explicitly stated in their manuscript, is the result when inserting a wind speed U of zero in their formulae for dT depending on the wind speed (Donlon: $\Delta T_{depth} = -0.14 - 0.30\exp(-\frac{U}{3.7})$, Minnett: $\Delta T = -0.133 - 0.637\exp(-0.305U)$). However, Donlon et al. state themselves, that their function is only well defined for night-time conditions with wind speeds above 2 m s^{-1}, while Minnett et al. build on that function and adapt it to their data, which included more data from lower wind regimes and daytime conditions. Because their formulae are defined for higher wind speed than in our data, and are based on entirely different measurement techniques, we do not expect a direct agreement with our data. We rather make the comparison to point out the need for a dT parametrisation under a low wind regime, in which dT is not solely based on the wind speed, but also on the surface temperature and other parameters influencing dT (Minnett et al., 2011). We removed the supposed dT from Donlon and Minnett for no wind from the discussion and described the comparison in a more general context.

We performed heat flux measurements during the mesocosm study and successfully measured shortwave solar radiation and longwave irradiance (Bibi et al., 2025a). However, no data of air temperature and relative humidity inside of SURF are available due to sensor failure. While we also performed these measurements outside of SURF, they were not representative for the conditions inside of SURF, especially with the roof closed, as exchanges with the surrounding air were limited. Thus, calculations of latent and sensible heat flux with parameters outside of SURF were considered being highly uncertain. As the scope of this manuscript includes both the analysis of oxygen and temperature microprofiles and an assessment, how a phytoplankton bloom influences both, we decided to keep the focus on the profile analysis and not introduce heat flux measurements with significant uncertainties. A dedicated analysis of temperature microprofiles and heat fluxes would require a separate study, with a different setup. However, our group conducted a separate experiment analysing the surface heat flux inside of SURF depending on the wind speed, which is currently under review (Jaeger et al.: Interplay of Latent and Shortwave Heat Flux as Forces on the Sea Surface Temperature – a Mesocosm Approach).

We rewrote parts of the discussion in Section 4.1 to account for these limitations of our study and described the comparisons with field studies from the literature in a more general context.

Changes in manuscript:

For changes in the discussion, see changes shown in reply to referee comment 2 from the second referee.

Referee comment 7:

Ward and Donelan (2006) measured the TBL thickness in the laboratory in a similar fashion and should be cited and compared with the results

Response:

We included a comparison of the TBL with the thermal sublayer thickness from Ward and Donelan (2006) in the discussion and added the reference accordingly.

Changes in manuscript:

Line 68: […] Reported TBL thicknesses can reach 8 mm (Ginzburg et al., 1977), but generally refer to the upper 1 mm (Donlon et al., 2002) *and are highly dependent on the wind speed (Ward and Donelan, 2006).* […]

For changes in the discussion, see changes shown in reply to referee comment 2 from the second referee.

Line 749: *Ward, B., and Donelan, M. A.: Thermometric measurements of the molecular sublayer at the air-water interface, Geophys. Res. Lett., 33, L07605, https://doi.org/10.1029/2005GL024769, 2006.*

Referee comment 8:

Zero lines on Figs 2-5 would be helpful

Response:

We added zero lines in Figures 2, 3, 4, 5, D1 and E1.

Changes in manuscript:

For changes to Figure 2, 3, 4, 5, D1 and E1 see end of this manuscript.

Referee comment 9:

How were the pre-, bloom, and post- periods determined? Vertical lines on Figs 5-9 showing these periods would be helpful

Response:

In Bibi et al. (2025a), the different bloom phases for all sub-experiments of the mesocosm study were determined based on the chlorophyll *a* dynamic, which is a proxy for the phytoplankton biomass. The pre-bloom phase was set to last until 26 May, as on 27 May nutrients were added to induce the phytoplankton bloom. The 05 June was set as the end of the bloom phase, as the chlorophyll *a* then reached the pre-bloom level of 5 µg L^{-1} from prior the addition of the nutrients. This was added to the text at the beginning of Section 3.1 describing the results and lines indicating the start and end of the bloom were added in Figures 5, 6, 7, 8, 9, B1, C1 and D1.

Changes in manuscript:

For changes to Figure 5, 6, 7, 8, 9, B1, C1 and D1 see end of this manuscript.

Line 212: […] the pre-bloom phase (18 May to 26 May), the bloom phase, *starting with the addition of nutrients until chlorophyll a reached levels to those prior to the addition* (27 May to 04 June), and the post-bloom phase (05 June to 16 June) (Bibi et al., 2025a). […]

Referee comment 10:

The first sentence of Sec 3.5 states that all observed night profiles were similar and refers to Appendix E, but that appendix only shows profiles for one night.

Response:

In Appendix E we added the profiles of the second temperature sensor for the same night to further compare both sensors and give more examples of the profile shapes. Unlike the oxygen microprofiles, the shape of the temperature profiles did not significantly change between the nights. As the temperature microprofiles did not exhibit distinct differences between the bloom phases, we decided it to be sufficient to show profiles from one night to give an example of the general shape of the profiles. We rephrased the first sentence in Sec. 3.5 and expanded Appendix E to include the comparison between the profiles of the different sensors. The data of all microprofiles are publicly available, as all microsensor data are published in PANGAEA as Rauch et al.: Microscale profiles of oxygen, pH and temperature through the sea surface microlayer in a mesocosm experiment during an algal bloom (DOI not available yet).

Changes in manuscript:

Line 293: The temperature microprofiles observed during the night *appeared* similar across *all nights, regardless of the bloom phase, and an example of the profiles of both temperature microsensors on 22 May is given in Appendix E.* The temperature […]

Line 550: Appendix E: Temperature microprofiles during one night

The temperature microprofiles on 22 May give a typical example of the profiles *from both temperature sensors* during all nights (Fig. E1). The warmest profile was the first of the night, and all the subsequent profiles were approximately *0.02 °C to 0.05 °C* cooler than the preceding profile. The air temperature decreased as it moved *farther* from the water surface; it also decreased from profile to profile during the night, and it was always lower than the water temperature. A cooler thermal boundary layer was present in all profiles. *Sensor 2 measured up to 0.1 °C higher temperatures and was mounted approximately 750 µm deeper than Sensor 1. These slight deviations in mounting height and measured temperature were also observed on other nights, while the overall shape of the profiles remained very similar between both sensors.* The only significant differences between nights *for each sensor were the mounting height and* the absolute temperature, which typically rose from night to night, starting at approximately 18.9 °C on 22 May and reaching around 22.6 °C on 15 June.

For changes to Figure E1 see end of this manuscript.

Comment editor (Peter Liss)

Editor comment 1:

I have looked at the authors' replies to the referees' comments and in general they are well addressed. The use of the term gradient is criticised by both the referees. The authors propose to change 'gradient' to 'anomaly' throughout. In my opinion this is not a good step since anomaly needs to be defined, i.e. anomaly means different from the norm and here what is the normal is not defined. I do not see why 'difference' can't be used in place of 'gradient' since differences are what was actually measured.

Response:

Based on the editor's comment and referee comments, we decided to change "gradient" to "difference" and not "anomaly". The word "gradient" was left only when specifically referring to a difference over a distance.

Changes in manuscript:

Changes from "gradient" to "difference" were made throughout the manuscript, Figures and title of the manuscript.

Changed Figures

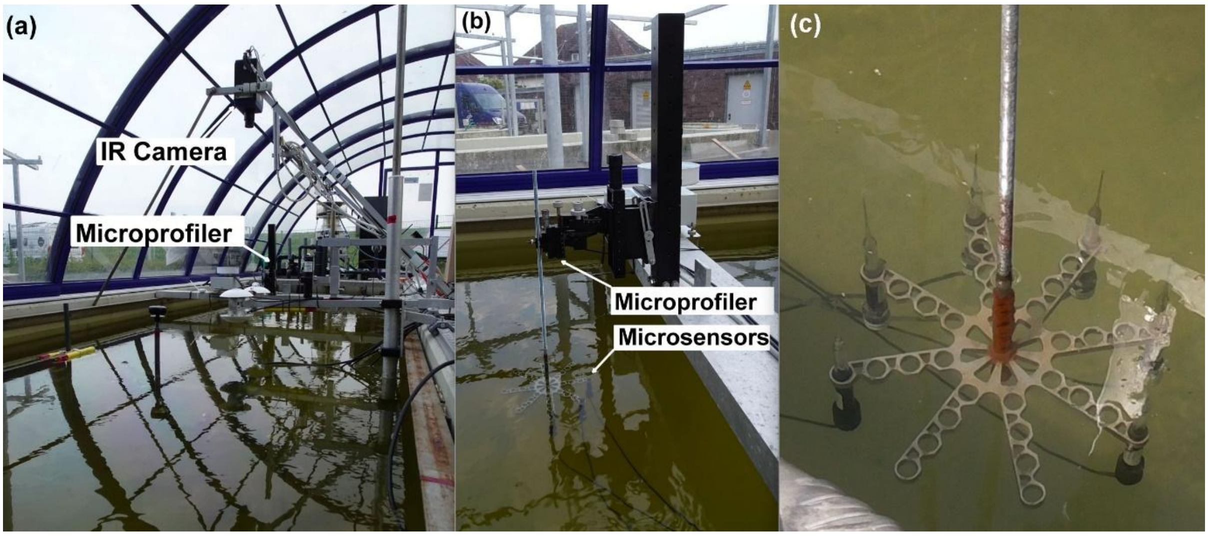


Figure 1 (a): Setup of different sensors inside SURF including the microprofiler and IR camera. (b): Microprofiler setup with microsensor array. (c): Close-up image of the microsensors and holder used during the mesocosm study.

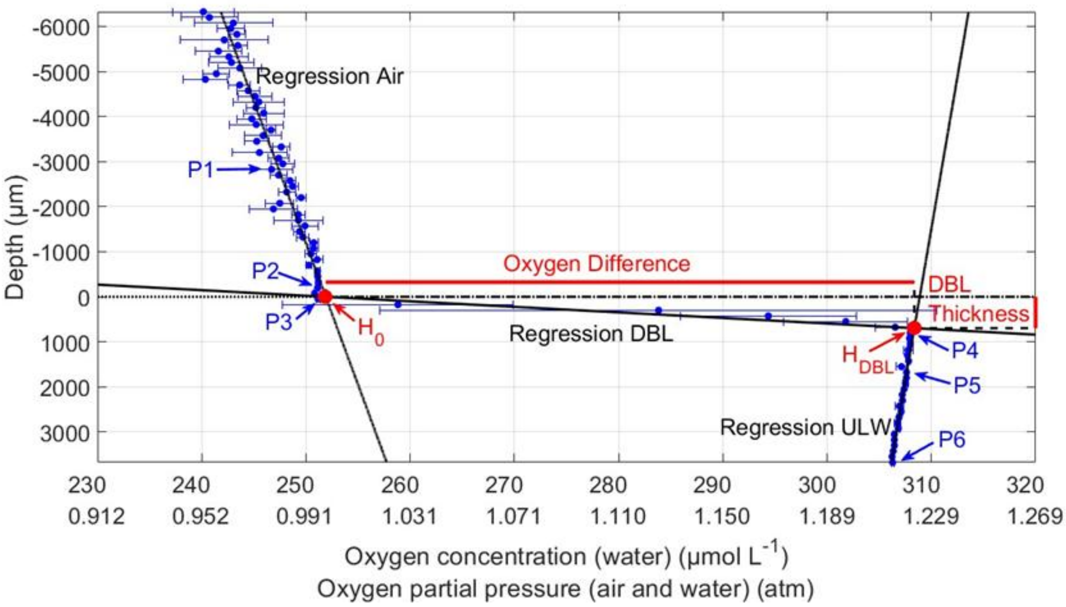


Figure 2: Microprofile of the oxygen concentration (31 May 2023, Profile 2). The oxygen concentration is accurate for the water profile but not for the air profile; the oxygen partial pressure is accurate for both profiles. Depth corrected for the proper sensor position; 0 µm indicates the air–water interface (dotted line); points P1–P6 were used to calculate regressions for air, DBL, and ULW. The intersection of air and DBL regression H_0 is the upper position of the water surface, and the intersection of DBL and ULW regression H_{DBL} is the lower DBL boundary. The vertical difference between H_{DBL} and H_0 represents the DBL thickness, and the horizontal difference represents the oxygen difference across the DBL.

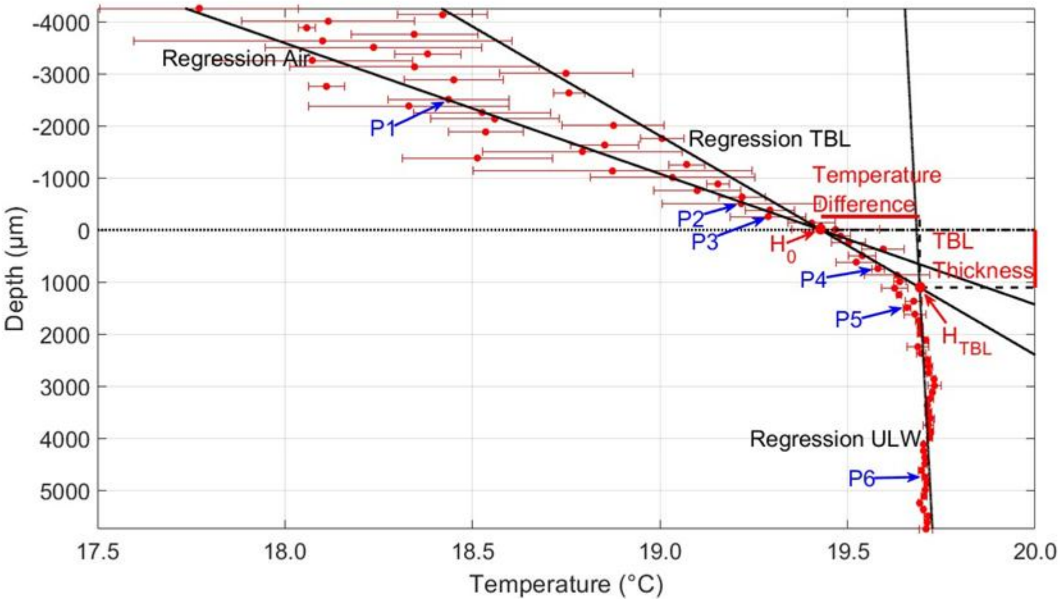


Figure 3: Microprofile of the temperature (31 May 2023, Profile 1). Depth corrected for the proper sensor position; 0 µm indicates air–water interface *(dotted line);* points P1–P6 were used to calculate the regressions for air, TBL, and ULW. The intersection of air and TBL regression H_0 is the upper position of the water surface, and the intersection of TBL and ULW regression H_{TBL} is the lower TBL boundary. The vertical difference between H_{TBL} and H_0 represents the TBL thickness, and the horizontal difference represents the temperature *difference across the TBL*.

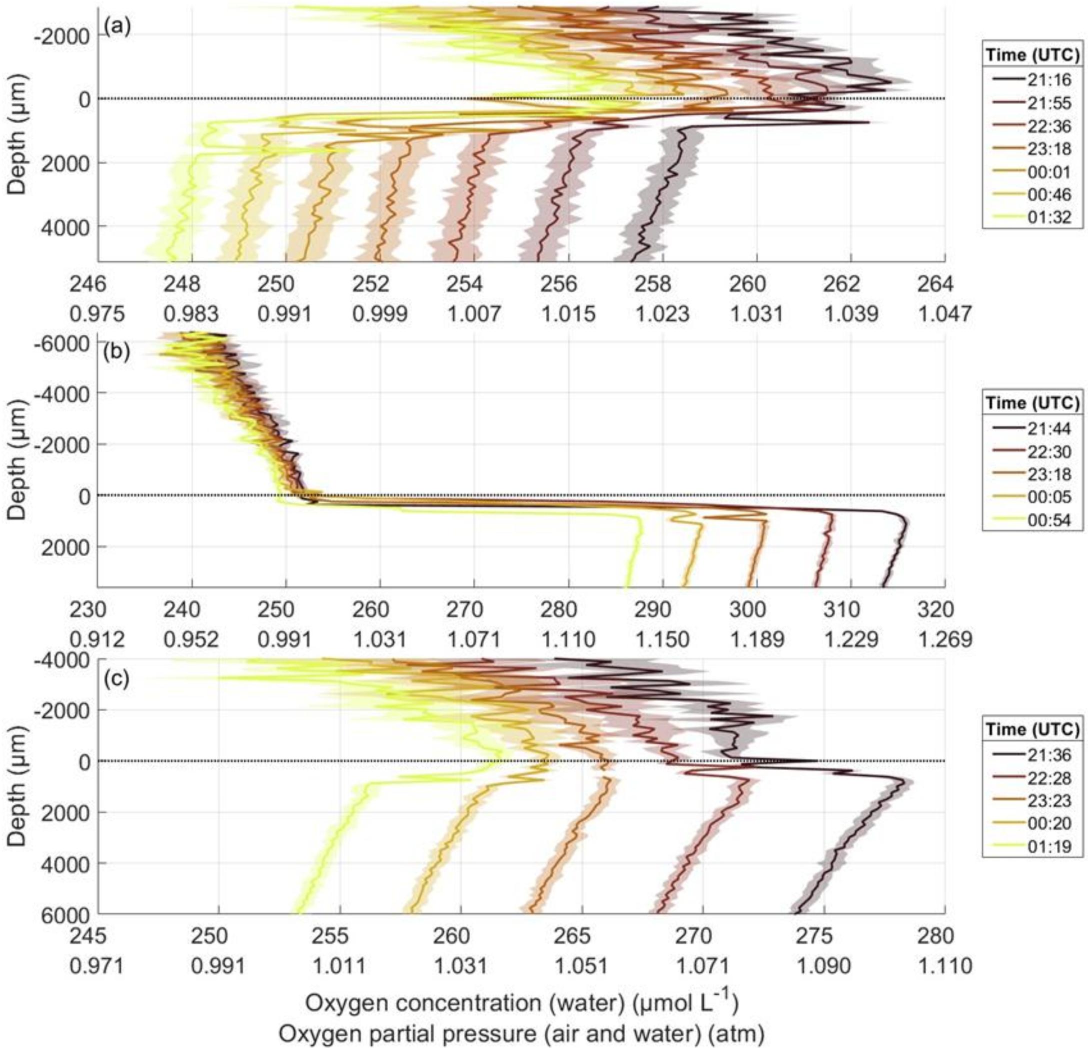


Figure 4: Oxygen microprofiles throughout the night of 22 May in the pre-bloom phase (a), 31
1045 May in the bloom phase (b), and 11 June in the post-bloom phase (c), depth corrected for the proper sensor position. *The oxygen concentration is accurate for the water profile but not for the air profile; the oxygen partial pressure is accurate for both profiles.* 0 µm indicates the air–water interface (dotted line). Times are the mean times of the profile; one profile took between 40 and 50 minutes to complete.

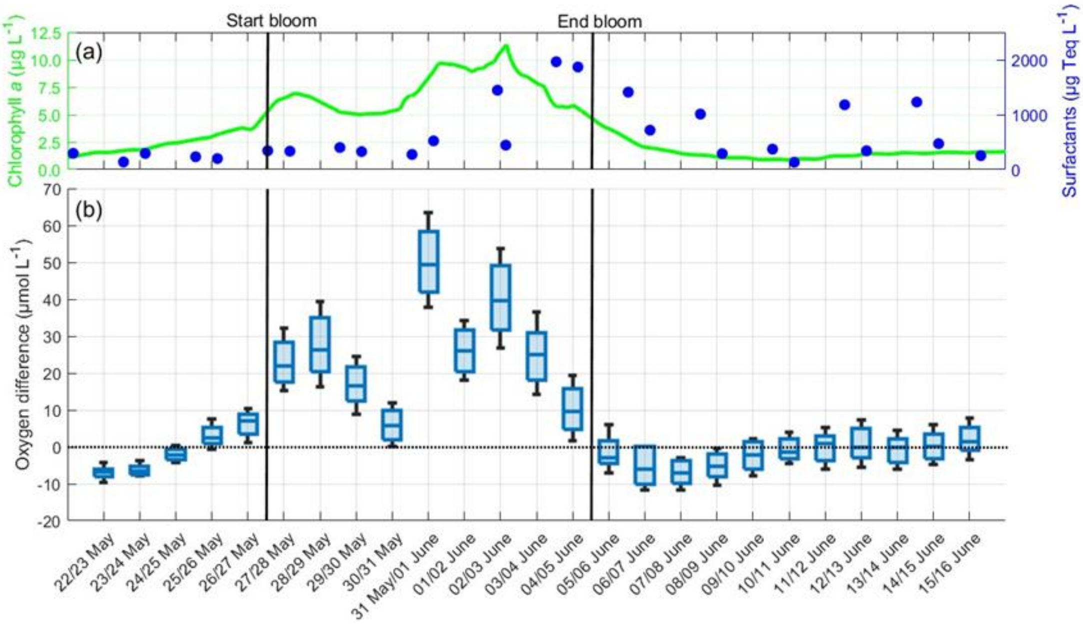


Figure 5 (a): ULW chlorophyll *a* concentration (green) and SML surfactant concentration (blue) between 22 May and 16 June, (b): Oxygen *differences* in the SML during the nights between 22 May and 16 June, 4–10 profiles per night, box: 25 % to 75 % quartile, horizontal line: median, whiskers: largest and smallest value. *The dotted line indicates the zero level and the solid black lines indicate the start and end points of the bloom phase.*

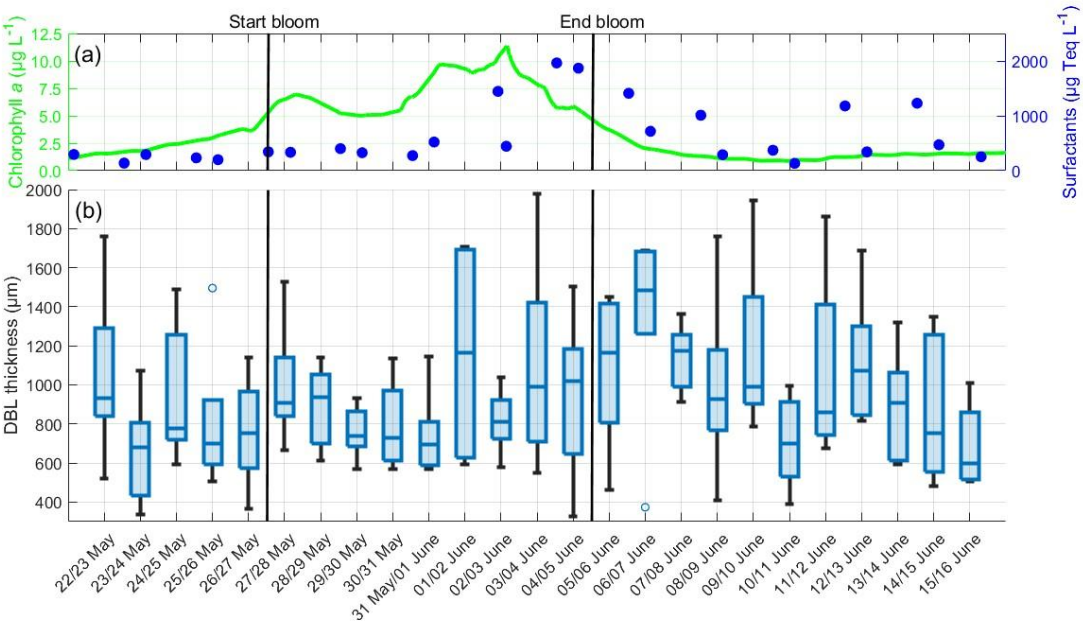


Figure 6 (a): ULW chlorophyll *a* concentration (green) and SML surfactant concentration (blue) between 22 May and 16 June, (b): Oxygen DBL thickness during the nights between 22 May and 16 June, 4–10 profiles per night, box: 25 % to 75 % quartile, horizontal line: median, whiskers: largest and smallest nonoutlier value, open circles: outliers (difference to next value > 1.5 times interquartile range). *The solid black lines indicate the start and end points of the bloom phase.*

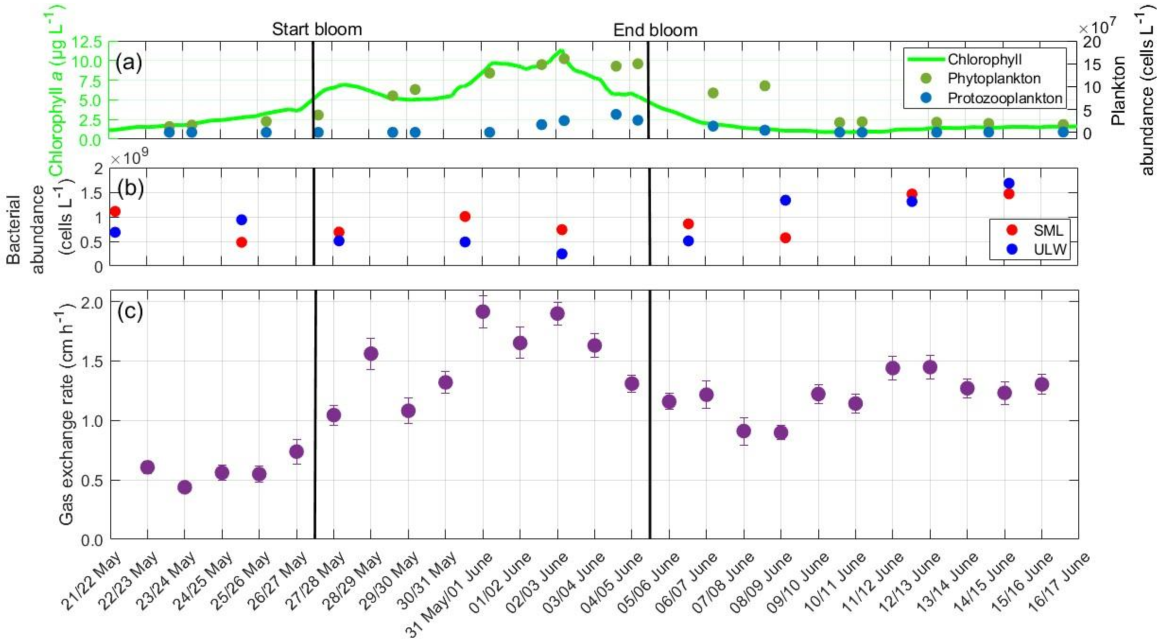


Figure 7 (a): ULW chlorophyll *a* concentration (green), phytoplankton abundance (dark-green), and protozooplankton abundance (blue) between 22 May and 16 June, (b): Bacterial abundance in the SML (red) and ULW (blue), (c): Mean gas exchange rate during the nights. *The solid black lines indicate the start and end points of the bloom phase.*

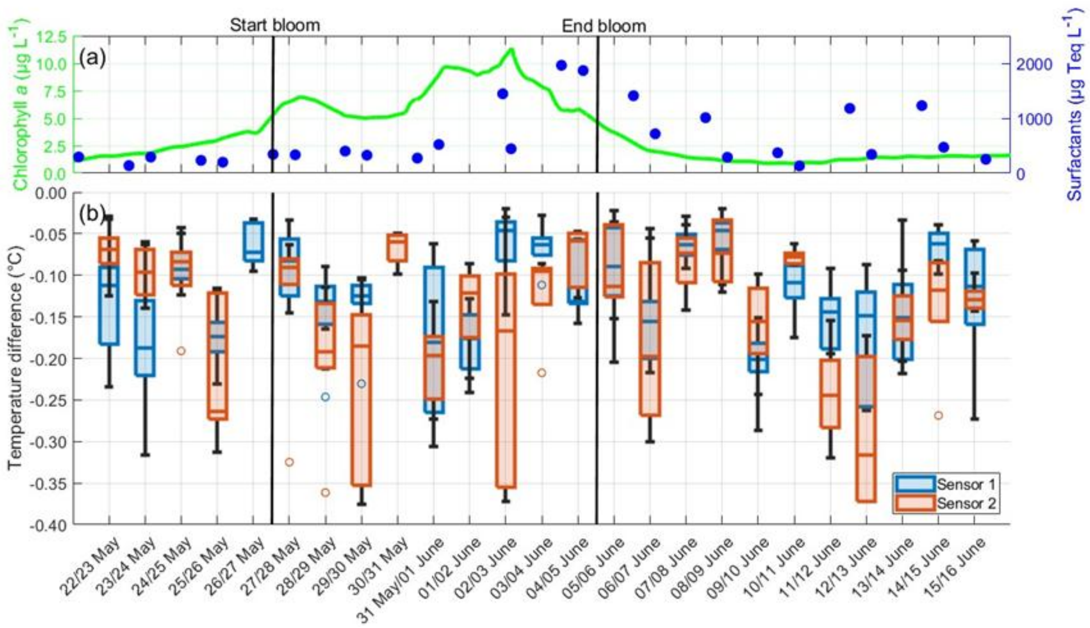


Figure 8 (a): ULW chlorophyll *a* concentration (green) and SML surfactant concentration (blue) between 22 May and 16 June, (b): Temperature *differences* from Sensor 1 (blue) and Sensor 2 (red) during the nights between 22 May and 16 June, 4–10 profiles per night, box: 25 % to 75 % quartile, horizontal line: median, whiskers: largest and smallest nonoutlier value, open circles: outliers (difference to next value > 1.5 times interquartile range). *The large interquartile range of Sensor 2 on 29 May and June 02 is caused by two outliers in these nights. The solid black lines indicate the start and end points of the bloom phase.*

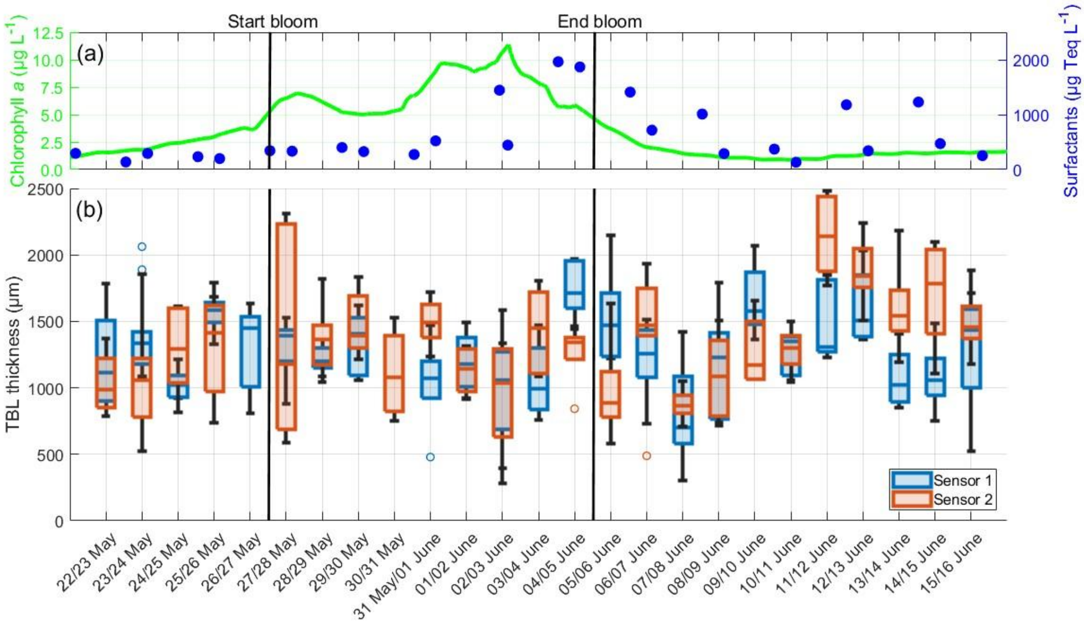


Figure 9 (a): ULW chlorophyll *a* concentration (green) and SML surfactant concentration (blue) between 22 May and 16 June, (b): TBL thickness from Sensor 1 (blue) and Sensor 2 (red) during the nights between 22 May and 16 June, 4–10 profiles per night, box: 25 % to 75 % quartile, horizontal line: median, whiskers: largest and smallest nonoutlier value, open circles: outliers (difference to next value > 1.5 times interquartile range). *The solid black lines indicate the start and end points of the bloom phase.*

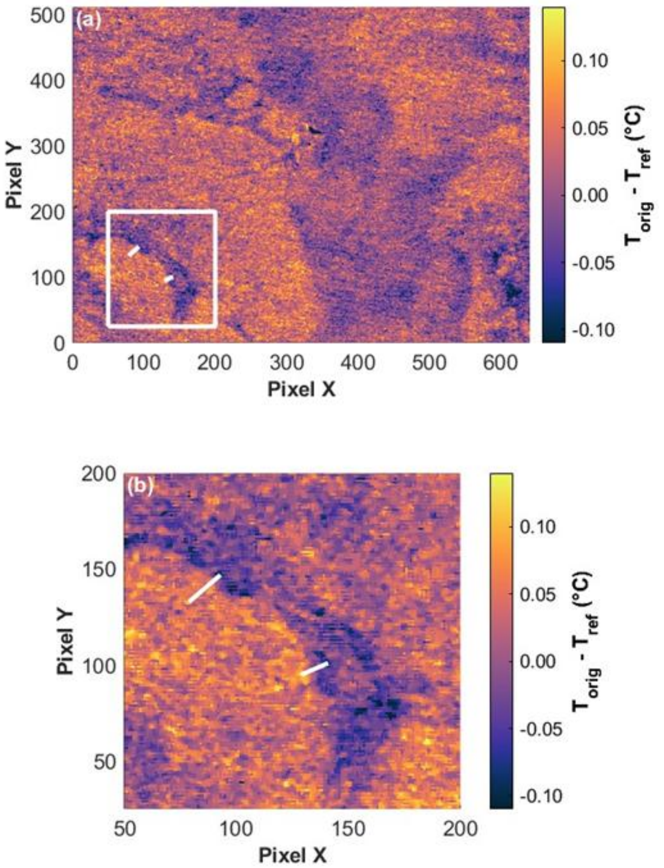


Figure 10 (a): Corrected IR camera image (T_{orig}–T_{ref}) from 11 June at 21:00:25 (field of view = 1.061 m x 0.812 m, 100 pixels X = 0.166 m, 100 pixels Y = 0.159 m). The white square in (a) shows the zoomed view in (b): The two lines demonstrate two examples where, despite a small distance of 32.181 mm (top left) and 22.087 mm (bottom right), high temperature differences of 0.232 °C (top left) and 0.224 °C (bottom right) were measured.

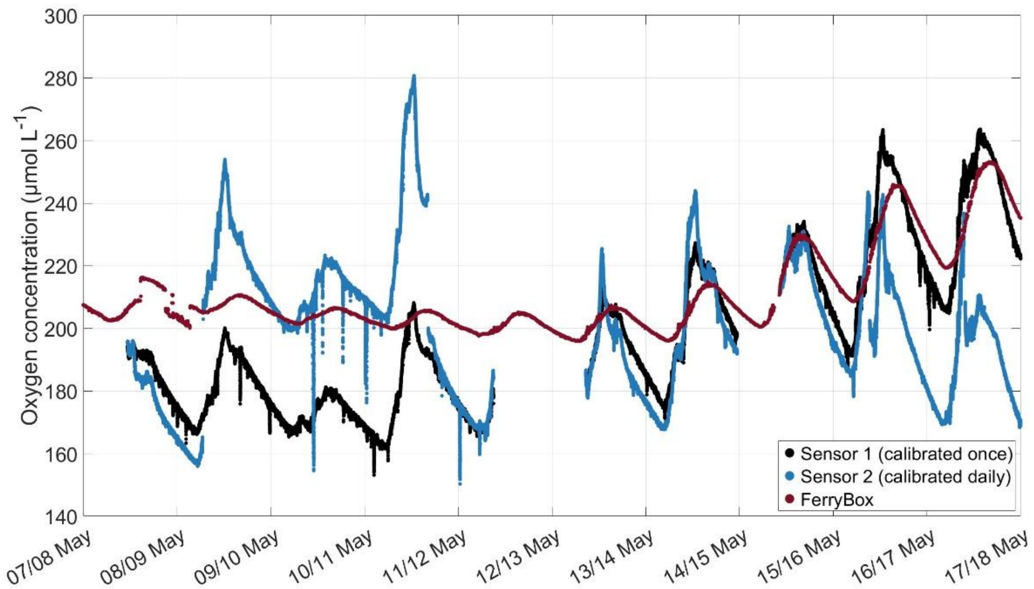


Figure A1: Comparison of the oxygen concentration measured by an oxygen microsensor calibrated at the start of the experiment and not corrected for temperature (black), one microsensor calibrated daily and corrected for temperature (blue), and corrected oxygen concentration measured by a FerryBox (red), experiment conducted in May 2024 in SURF. *Microsensor data are filtered using a Hampel filter with a 10-minute window.*

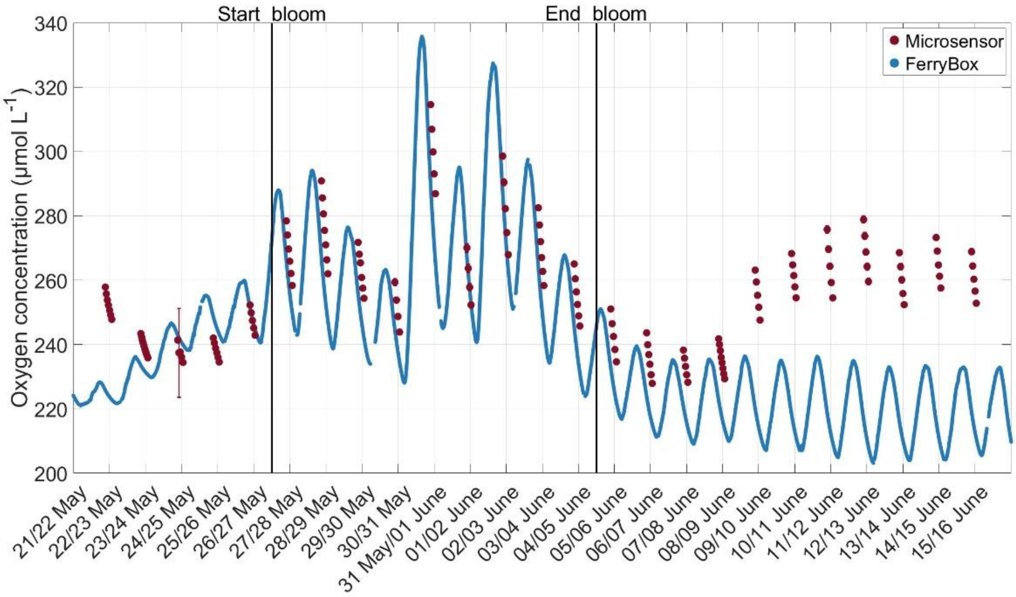


Figure B1: Corrected oxygen concentration measured by the FerryBox at 40 cm depth (blue) and by the oxygen microsensor (red), mean from the lower end of the DBL to the end of the profile. *The solid black lines indicate the start and end points of the bloom phase.*

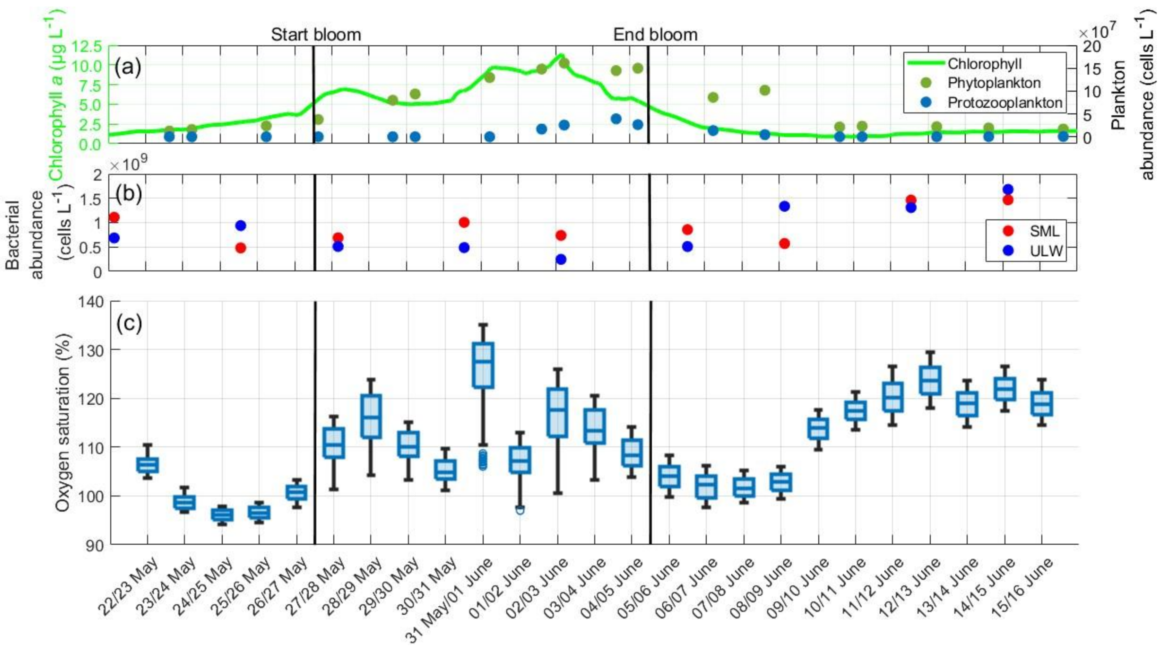


Figure C1 (a): ULW chlorophyll *a* concentration (green), phytoplankton abundance (dark-green) and protozooplankton abundance (blue) between 22 May and 16 June, (b): Bacterial abundance in the SML (red) and ULW (blue), (c): Oxygen saturation during the nights, boxplots show all the oxygen saturation data in the DBL and ULW, box: 25 % to 75 % quartile, horizontal line: median, whiskers: largest and smallest value, open circles: outliers (difference to next value > 1.5 times interquartile range). *The solid black lines indicate the start and end points of the bloom phase.*

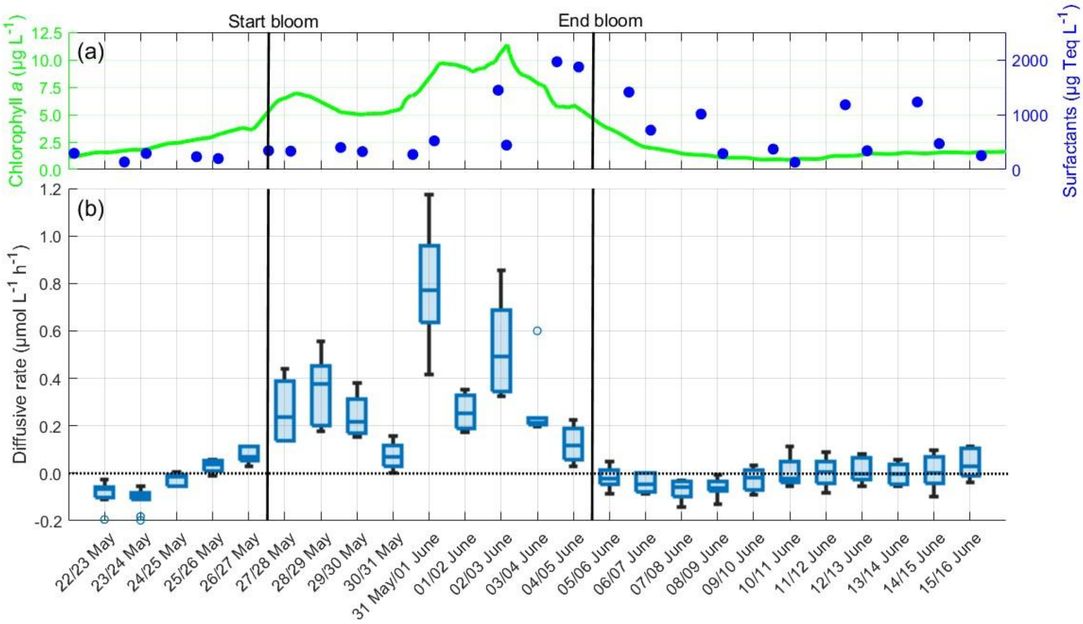


Figure D1 (a): ULW chlorophyll *a* concentration (green) and SML surfactant concentration (blue) between 22 May and 16 June, (b): Oxygen diffusive rate in the SML during the nights between 22 May and 16 June, 4–10 profiles per night, box: 25 % to 75 % quartile, horizontal line: median, whiskers: largest and smallest value, open circles: outliers (difference to next value > 1.5 times interquartile range). *The dotted line indicates the zero level and the solid black lines indicate the start and end points of the bloom phase.*

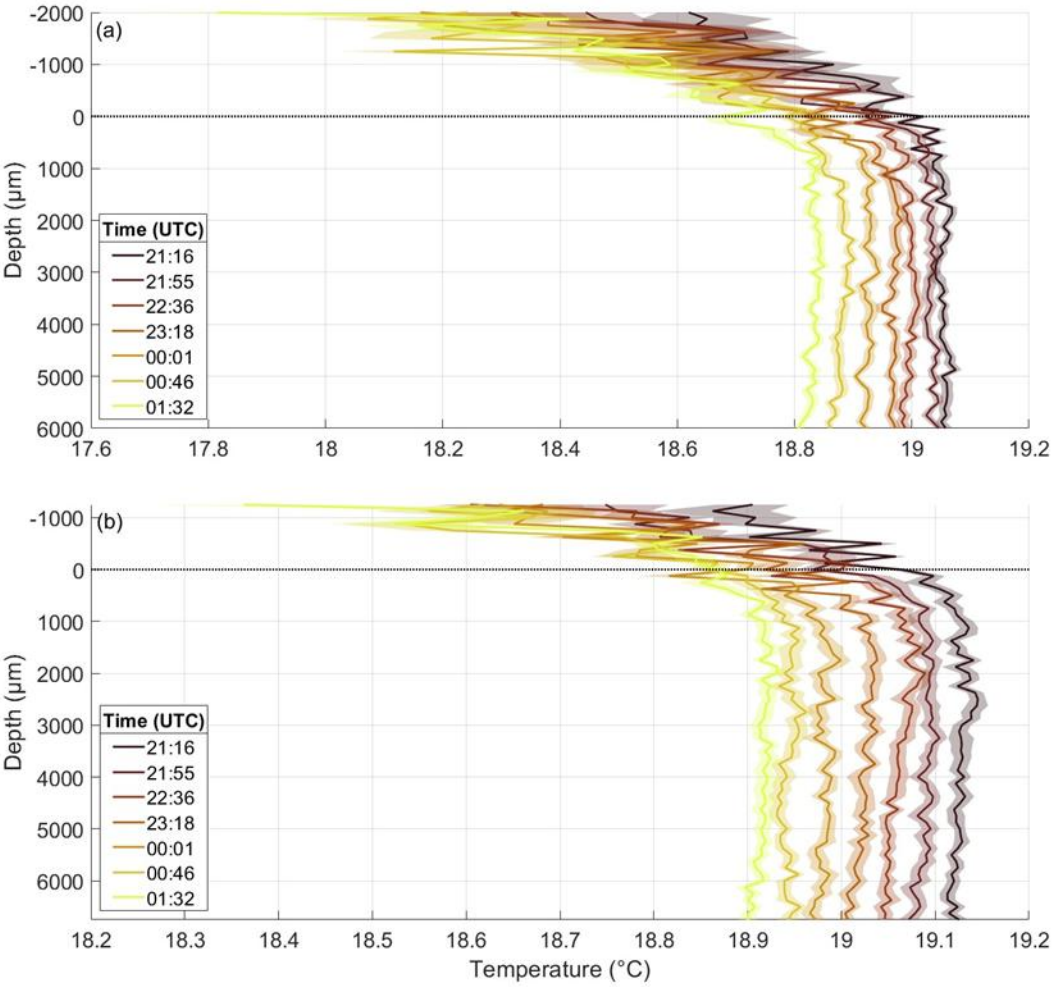


Figure E1: Temperature microprofiles of Sensor 1 *(a) and Sensor 2 (b)* throughout the night of 22 May, in the pre-bloom phase, depth corrected for the proper sensor position. 0 µm indicates the air–water interface *(dotted line)*. Times are the mean times of the profile; one profile takes approximately 40 minutes to complete.