REPLIES TO REVIEWER 2

We thank the referee for the careful reading of the manuscript and the constructive comments. We have thoroughly considered all remarks and provide detailed responses below. We believe that the suggested revisions have significantly improved the overall quality of the manuscript. The answers to the points raised by the referee are presented in the following sections.

Major comments

Line 138: What is meant by “Amazon sub-region is influenced by the Amazon plume”. Do you mean that there is advection of warm, fresh surface waters into the sub-region? Please be more specific as the influence the authors have in mind ends up being important throughout the text. Without stating this influence up front here, some later discussions are confusing.

We warmly thank the reviewer for this remark. In the revised manuscript, we now clearly state that warm, fresh surface waters are advected into the Amazon sub-region and that this advection is associated with the Amazon freshwater plume. The corresponding modifications can be found between lines 144 and 145 of the revised manuscript.

Lines 203-205: This sentence is unclear. It sounds like you will look into linkages between mesoscale SST anomalies and the Amazon plume, but the Amazon plume is not present in your EURECA box (per line 244). Also, as worded, it sounds like you are saying that the mesoscale SST anomalies lead to the Amazon plume, which does not seem to make sense.

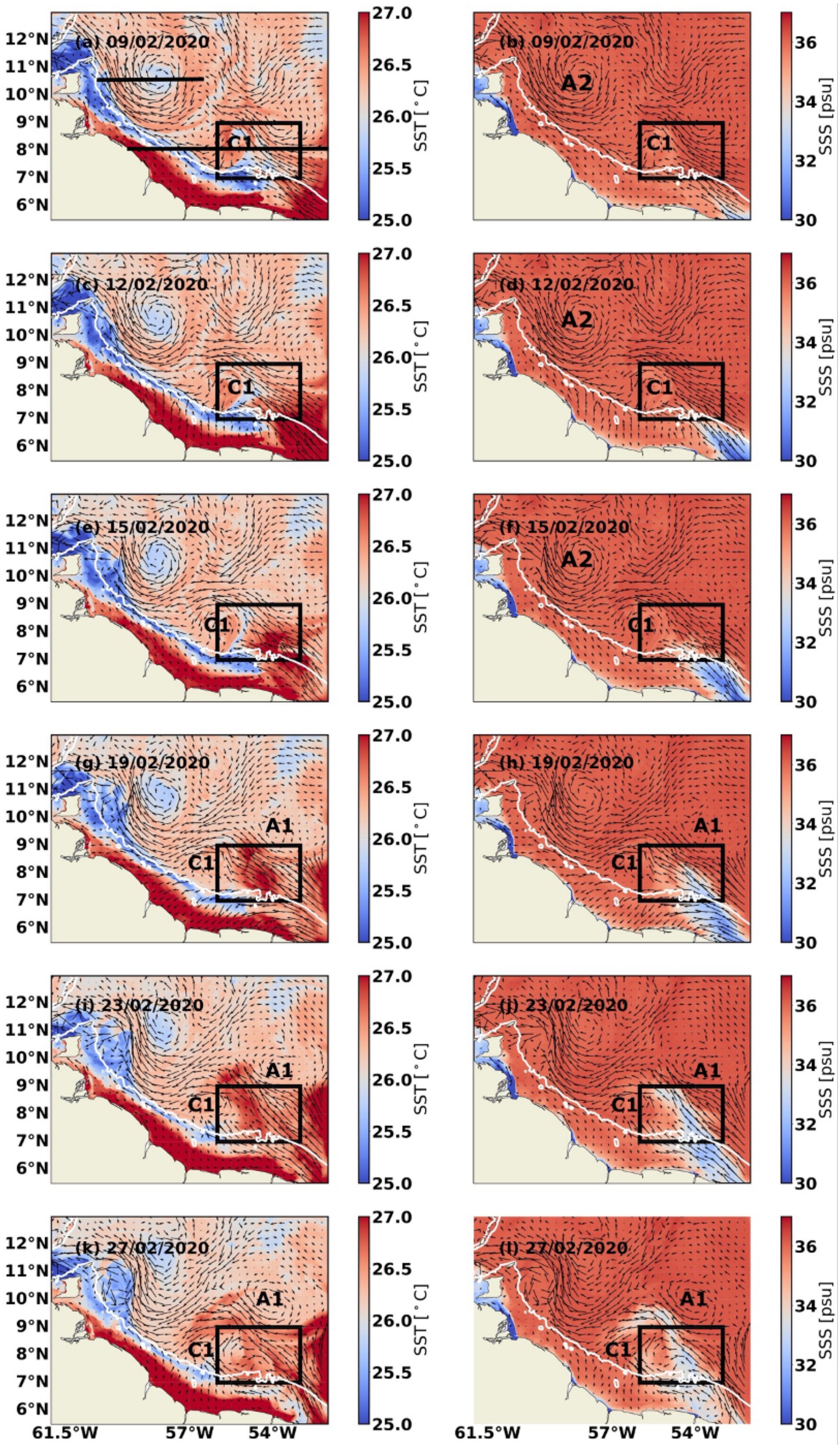
We thank the reviewer for this remark. Negative SSS anomalies associated with the Amazon plume are indeed usually warmer, as Amazon river waters are typically warmer than surrounding ocean waters. The reviewer is correct that this relationship is not clearly visible in the climatology fields shown in Fig. 3. This is due to the high variability of the ocean surface circulation in this region: the warm and fresh Amazon plume interacts with the cyclonic North Brazil Current Ring C1 (see figure below),which advects cooler waters from the cold filament. When averaged over two months, this variability obscures the typical correspondence between low SSS and high SST. The figure below of snapshots of SST (left) and SSS (right) illustrate this variability. To avoid potential misinterpretation, we have removed from the main text all statements inferring a warm–fresh relationship directly from the climatologies. In addition, we have reformulated the oceanic analyses of section 4.4 where this relationship is assessed to further elucidate the linkages between the Amazon freshwater plume and SST mesoscale anomalies.

In the lines the reviewer refers to, our intention was to highlight that the positive SST anomalies associated with the Amazon freshwater plume (which do not appear clearly in the climatology for the reason above) are nonetheless part of the ocean mesoscale: plume waters exhibit a positive mesoscale SST anomaly. This establishes a clear connection between a well-known physical feature (the Amazon plume) and the broader concept of mesoscale SST anomalies, which is central to our analysis. The purpose of computing the mixed-layer heat budget is precisely to diagnose the processes of heat redistribution associated with the Amazon plume that contribute to the maintenance or dissipation of surface mesoscale SST anomalies.

We have also removed the statement in line 244 of the previous version claiming that warm SST anomalies east of the Amazon subdomain are associated with the northward advection of warm Amazon plume waters, as the snapshots show that this interpretation is not supported.

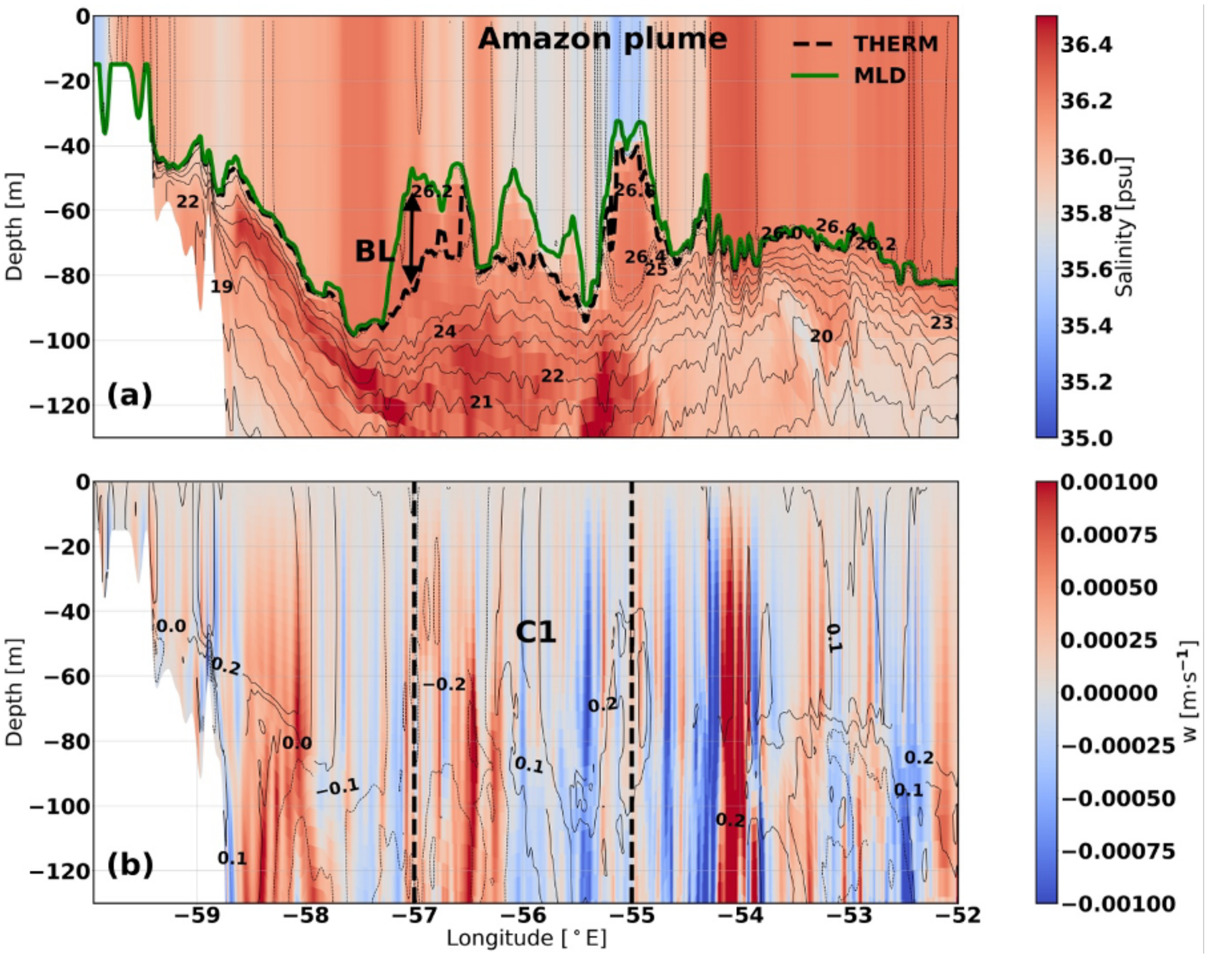
Please, find the rephrasing of lines 203-205 of the old version between lines 214 and 218 of the new one. There, we include as well a definition of the Amazon plume waters based on observational studies in the region (SSS<35psu, Reverdin et al., 2021, https://doi.org/10.1029/2020JC016981)

P.S. We may be misunderstanding part of the reviewer's question, but the Amazon plume (now defined in the main text as waters with less than 35 psu) clearly enters both the EURECA region (see Fig. 2 in the revised manuscript) and the Amazon subregion (see Fig. 3d, which shows the JF 2020 model climatologies).


(a) 09/02/2020
(b) 09/02/2020
(c) 12/02/2020
(d) 12/02/2020
(e) 15/02/2020
(f) 15/02/2020
(g) 19/02/2020
(h) 19/02/2020
(i) 23/02/2020
(j) 23/02/2020
(k) 27/02/2020
(l) 27/02/2020
A1
A2
C1
SST [°C]
SSS [psu]
12°N
11°N
10°N
9°N
8°N
7°N
6°N
61.5°W
57°W
54°W
25.0
25.5
26.0
26.5
27.0
30
32
34
36

Lines 241-242: Can the authors provide more discussion on the cold filament of surface water across your domain? This is a very prominent feature of your experiment region and should be discussed further as background for the remaining analyses.

We thank the reviewer for this comment. This is indeed a very important feature, with notable implications for air-sea interactions, as discussed below. The cold filament is generated by the advection of deeper, colder waters toward the surface, as illustrated in the transect shown below (8ºN, 9th February 2020). We have included two sentences in the main text to clarify this point. These additions appear between lines 251 and 255 of the revised manuscript.



Lines 255-256: This seems to contradict what is said above, as the low salinity patch extends within the Amazon box while the warm SSTs do not, but both are said to be related to the Amazon plume.

The reviewer is correct, and we thank them for this remark. As noted in our responses to earlier comments, the temperature signature of the Amazon plume cannot be reliably inferred from the SST climatology. The mean SST field is highly variable because the warm plume interacts continuously-with the cold filament, and this variability obscures the underlying relationship. When examining instantaneous fields (see Fig. 8 of the revised manuscript), lower SST values are generally associated with higher SSTs; however, this correspondence does not hold uniformly through the plume. We now make this point explicit in the text (see lines 268-269 of the revised manuscript).

We also note that Fig. 2 in the previous version (model climatologies) contained an error: the climatology was computed using December-January-February 2020 rather than January-February 2020 alone-. This has been corrected in the revised Fig. 3 (previously Fig. 2), which now more clearly shows the low-SSS/high SST relationship.

Finally, we have removed the reference to the Amazon plume at this point in the manuscript and now address it explicitly in section 4.4.

Line 372: Again, it is indicated that the Amazon plume is present in the EURECA domain, contrary to what is said on line 244. Discussion around the Amazon plume need clarifying throughout the text, as what remains of the plume in your study area and its impacts on the Amazon box are unclear and inconsistent.

We thank the reviewer for this remark. The Amazon plume is indeed present in the Amazon subregion, as illustrated in Fig. 3d (showing the low SSS patch < 35 psu crossing the Amazon subregion) and Fig.3a (which displays high SSTs over the low -SSS patch, though, as noted in earlier responses, the SST signal is noisier due to interactions with the cold filament located south and west of the Amazon subregion). We hope that the updated Fig. 3 improves the clarity of this discussion. In addition, to avoid misinterpretation, we have removed all references to the Amazon plume from Section 4.1 (including the previous mention line 244) and now restrict the description in that section to the features directly observable in the climatologies.

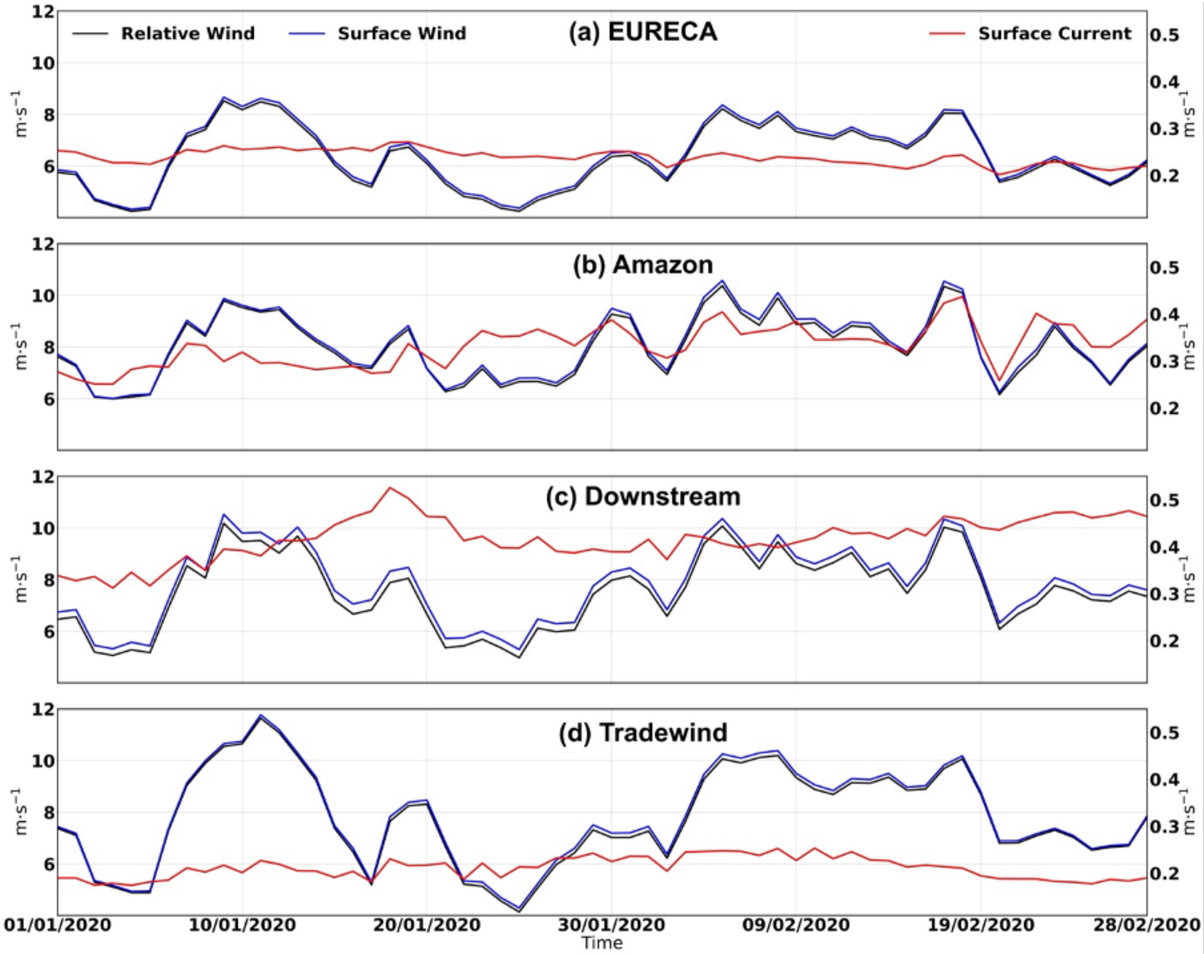
A detailed analysis of the Amazon plume is now provided in Section 4.4. In this section, we apply the definition of plume that we have added the methodology (SSS < 35psu; see line 218 of the revised manuscript).

Finally, we have reformulated the sentence previously located at line 372; the updated version can be found within the paragraph spanning lines 390–396 of the revised manuscript.

Lines 356-357: Wind variations in Fig. 2c are very difficult to see. Are the contributions to the relative wind variability mainly due to the differences in the surface currents in the three regions or is the variability in the winds higher in Amazon and Tradewind boxes? Showing time series of the winds and currents and/or the relative wind for the four regions might be more helpful than comparing their separate time means for the purpose of this discussion.

We warmly thank the reviewer for this remark. The figure below represents the time series of the area mean surface wind speed (blue), relative wind speed (black) and surface current (red). We can see that the variability of the winds is higher in the Tradewind box, which experiences variations between 4 and 12 m/s than in Downstream or Amazon. We can also appreciate that relative winds are closer to surface winds in the Amazon and Tradewind boxes than in the Downstream box. This explains why we observe LHF differences (blue markers) in the positive and negative sides of the x axis in Figs.6b and d of the new version of the paper. However, in the Downstream region surface currents are stronger and generally aligned with surface winds (see Fig. 3e of the new version of the paper). Therefore, relative winds are weaker than surface winds throughout the two months.

We have included this figure in the supplementary as we think there are already enough figures in the main text. However, we cite it in the discussion The corresponding modifications can be found between lines 366 and 383 of the revised version.



Lines 365-368: If CFB always increases surface winds in the direction of the current, then this particular process would result in relative winds that are smaller than the wind alone, correct? So why would this effect ever increase LHF? It seems like this discussion conflates the relative versus full surface wind impacts on LHF with the impacts of CFB alone. Or maybe I am not understanding CFB?

We thank the reviewer for this remark. Please, let us clarify this point with an example and let us refer to Fig. 1b of the new version of the manuscript. In particular, let's focus on the left hand side of the eddy, where surface currents (black arrow) and winds, which include CFB (blue arrow) are aligned.
Taking into account CFB, the relative wind is just the difference between the blue and the black arrows, shown as a green arrow in the schematic. However, within the surface wind's blue arrow, there is the momentum imprinted by surface currents (represented by the orange arrow, this is, the CFB effect). If we want to obtain a surface wind vector without the effect of CFB, we must perform the operation blue arrow minus orange arrow. This is equivalent to removing the CFB-induced wind speed from the total surface winds. Since surface winds and currents are aligned, this operation results in a vector smaller in magnitude than the original surface winds (blue arrow).

If we now compute the relative winds between this surface wind without the CFB effect and surface currents, we obtain a smaller value than if we had computed relative winds with the original surface wind which accounts for CFB. Since LHF is proportional to relative winds, the LHF computed without the effect of CFB will be smaller than the one obtained if CFB is accounted for. Therefore, CFB increases LHF in this case. We hope it is clearer now.

We have included a more detailed explanation of this schematic (Fig. 1b of the new version) between lines 62 and 69 of the new version.

Line 385: Is not that the wind speed increases aloft and decreases at the surface over cold SSTs, but rather that the momentum transfer from aloft to the surface just does not occur. This is the "decoupling" of the surface layer from the free troposphere common in stable boundary layer situations. The wording here is misleading as it implies an opposite momentum transfer to what is happening over warm SSTs.

We thank the reviewer for this remark. We agree that the momentum transfer from aloft to the surface does not occur over cold SSTs. However, the anomaly histogram referenced by the reviewer (Fig. 7b in the revised version) indeed shows a reduction of near-surface winds (negative anomalies) and an enhancement aloft (positive anomalies). This vertical "decoupling" inhibits downward momentum transfer: surface winds weaken, while winds aloft strengthen because their momentum is not extracted. We have now explicitly stated that momentum transfer is suppressed in this context, as pointed out by the reviewer. The corresponding revision appears in lines 409-410 of the revised manuscript.

Line 387: Why show saturation specific humidity rather than potential temperature to illustrate temperature differences?

We thank the reviewer for this comment. We agree that potential temperature can be useful for assessing vertical stratification. However, we chose not to include it here since we already analyse the distribution of the Brunt-Väisälä frequency in Fig. 7a. To clarify the relationship, we have added the equation for N^2, as suggested by the other reviewer (see line 399 in the revised manuscript). Instead, we present the saturation of specific humidity since it is required to compute the specific humidity deficit, a key factor in the LHF bulk formula. In our analysis, variations in saturation specific humidity dominate over those in the specific humidity (Fig. 7d) in controlling changes in specific humidity deficit (Fig. 7e), thereby modulating the total LHF: higher LHF values occur where the surface saturation specific humidity is larger.

While we acknowledge that within the saturation specific humidity formula there is a dependence on air pressure (which can significantly vary in the first 2000m), the saturation specific humidity is useful when assessing air temperature variations at a given height as a function of the SST mesoscale anomaly. Finally, from a practical point of view, we only need it at the surface to compute LHF, so in theory we could suppress panel 7c. However, for the sake of completeness we would like to keep it.

Lines 407-408: On line 355 you state the opposite, that the LHF variations due to SST mesoscale variations are mainly due to the dynamic contribution. I believe this confusion is due to the many ways these authors use the term "dynamic contribution". On the one hand, it seems to be used to describe part of the overall "thermodynamic contribution" as on line 355, while on the other hand it is also used to describe the relative wind impacts which seem to be what is meant here? I think the terminology needs to be consistent throughout the text given how many effects are being examined. It is difficult as a reader to keep them all straight. And Fig. 1 only shows two of them. Perhaps, the authors can provide a table of effects they are investigating along with a description of what they are and how they are isolated? In any case, for the discussion on lines 407-408 can the authors return to their LHF naming convention and add the appropriate terms in parentheses after the words "thermodynamic contribution" or "SST changes" "variations in specific humidity" and "wind speed" or "dynamic contribution" so we know which term to refer to in Figs. 4 and 5.

We thank the reviewer for pointing out this inconsistency in terminology. As stated in line 335 of the previous version of the manuscript, the dynamic contribution indeed dominates the total LHF variations. In that line, the dynamic contribution was defined as "the LHF variations linked to the SST-induced modification of the near-surface atmospheric variables". For clarity, we now explicitly specify that the thermodynamic contribution corresponds to the component of LHF changes driven solely by SST-induced variations in saturation specific humidity, whereas the dynamic contribution encompasses the LHF changes arising from SST-driven modifications of the near-surface wind speed and specific humidity. We have revised line 335 accordingly and ensured that this terminology is used consistently throughout the manuscript (see line 347-351 in the revised version).

Concerning the discussion in lines 407 and 408, it has been rephrased so that it aligns with the discussion between lines 347-351 of the new version (335 of the old one). Please, find the modifications between lines 442 and 448 of the revised version of the paper.

We would also like to clarify. that Fig. 1 is not intended to represent the "thermodynamic" or "dynamic" contributions. Instead, it illustrates the downward momentum mechanism (Fig. 1a) and the current feedback (Fig. 1b).

Following the reviewer suggestion, we have added Table 2, which summarises the physical processes considered and the LHF differences used to isolate each of them.

Line 425-429 - This entire discussion is difficult to follow. The authors refer to the distribution of the mesoscale SST anomalies in space but we only see a histogram with height in Fig. 6. Also, the Amazon plume is mentioned multiple times despite it not being within the EURECA domain. Please clarify what is meant by the "core of the Amazon plume" since Fig. 1c suggests the plume is mostly outside of this domain.

We thank the reviewer for this remark. The reviewer is right that the distribution of mesoscale SST anomalies in space cannot be extracted from a histogram. Therefore, we have reformulated all the analyses following the atmospheric vertical profile histograms and before the mixed layer heat budget so that the conclusions are more accurate. We now present snapshots of SST and currents (Fig. 8) and clearly mark the boundary of the Amazon plume with the 35 psu isoline as suggested by previous observational research (Reverdin et al., 2021, https://doi.org/10.1029/2020JC016981). According to this definition, the Amazon plume crosses the Amazon sub-region in February 2020.

In addition, we present maps of the mixed layer depth, barrier layer thickness and OSS index integrated down to the mixed layer to discuss the spatial variations we were aiming to point out in the all version.

Please, find the changes in sections 4.4.2 and 4.4.3 of the revised version.

Lines 446-448 - The colormap may be too hard to read for the OSS values, as it seems that they never get higher than 40% (cyan). A value of 50% would be green, and at least in the version of the figure provided in the manuscript there does not seem to be any green color. Also, is it correct to say that salinity is important to the stability when overall the OSS % is well below 50%? According to Line 235, that means salinity is not important to the ocean stratification. Also, the core of the plume appears to be from the surface to about 20m depth, while the peak OSS values appear to be from about 15 to 40 m depth. So the peak OSS seems to occur at the base of the plume, not in the core of the plume.

We thank the reviewer for pointing out this feature. The figure the reviewer is referring to is no longer in the manuscript since a spatial analysis cannot be performed with mesoscale SST anomaly - depth histograms. Instead, we have plotted in Fig. 9d of the new version the averaged OSS index in February 2020. We also have changed the palette and plot limits to highlight the differences.

As the reviewer states, all the OSS values are below 50%, meaning temperature is always the main driver of stratification. There are certain regions (i.e. to the northwest of the plume) where OSS reaches 30%, meaning that the salinity contribution to stratification increases although it is still smaller than the temperature one. All these modifications have been incorporated into the revised manuscript (see lines 510 – 515).

Lines 440-464 - It might help this discussion (as at lines 424 and 426, for example) to provide an additional panel in Fig. 7 showing the SST across the Amazon box only with the SST mesoscale anomalies overlaid as contour lines. This will help to see spatially where the mesoscale SST anomalies are located within the domain. The dT/dz panel could be removed as it confirms no temperature inversions. This could simply be stated in the text without a figure. Also, or alternatively, the Amazon plume waters could be delineated in at least panel 7d.

We thank the reviewer for these two comments. As suggested, we have removed the dT/dz panel from Fig. 9 (in fact we removed the whole figure to substitute it by maps). We acknowledge we checked the absence of temperature inversions in lines 487 and 492 of the revised version.

In addition, we now include several SST and surface current snapshots in Amazon, with the plume delimited with the 35 psu isoline in magenta (Fig. 8) and a February 2020 average of SST (shading), salinity (contours), surface currents (arrows) and Amazon plume boundary (magenta contour, the 35 psu isoline) in Fig. 9a, providing a planar view of all these fields. We hope this improves the clarity of the discussion.

Lines 452-455 - Still confused where the Amazon plume waters are with respect to the mesoscale SST anomalies. If it is represented by the most fresh SSS anomalies (mesoscale SST anomalies of ~-0.02 to 0.2), then the total heat flux over this plume is near zero, not transitioning to negative until mesoscale SST anomalies > 0.2. Heat tendency in the plume is also near zero, with some positive heating at the upper end of the mesoscale SST anomalies within the plume (>0.1). Again, this discussion might be clearer if we had a planar view of the mesoscale SST anomalies within the Amazon sub-region with the Amazon plume clearly delineated on the map either in Fig 6 or Fig. 7.

We thank the reviewer for pointing this out, and we hope that the new analyses and figures in sections 4.4.2 and 4.4.3 of the revised manuscript clarify this issue. Please refer to Fig. 8 for several snapshots of the SST field, the surface currents and the boundary of the plume (35 psu isole) and to Fig. 9 for an analysis of the vertical structure of the mixed layer depth averaged in February 2020, also with the plume boundary overlaid as a magenta contour.

Following the reviewer's advice, we have removed the 2-d ocean histogram (Fig. 7 of the old version) as it does not allow a spatial analysis of the anomalies).

Fig. 8 - Please plot SSS over SST in one of these panels so we can see the salinity signature of the Amazon plume and its corresponding SST signature together. This will make earlier discussions of the plume influences easier to follow. You could also consider such a panel for Fig. 2 for the entire EURECA domain.

We thank the reviewer for this comment. As suggested, we have added the boundary of the Amazon plume (35 psu isoline) in Figs. 8, 9 and 10 of the revised manuscript where we perform the oceanic analysis of Amazon. We have not included it in Fig. 3 of the revised manuscript (model climatology, the old Fig. 2) to reduce the complexity of the figure (which is already charged with shading, arrows and/or contours). However, we have modified it so that it has a discrete palette in the shading fields. This allows to clearly distinguish the SSS isolines in Fig. 3d. We hope these changes facilitate the flow of the discussion.

Fig.8a vs Fig. 2a - It is not clear that the SST contours in Fig. 8a match the filled contours in Fig. 2a. According to Fig. 2a, the warmest SSTs in the Amazon domain are near the 17.2 contour label for specific humidity and towards the northeast, where colors are more yellow. However, there is a clear tongue of warm SST extending from the southwest across to the northeast of this domain (the Amazon plume) in Fig. 8a. Can the authors use a different color bar in Fig 2a to better highlight the SST gradients across the region?

We warmly thank the reviewer for pointing out this inconsistency. The reviewer is correct : the original Fig. 2 (now Fig. 3 of the revised manuscript) was based on the DJF mean rather than the JF mean. And the old Fig. 8 only in February 2020. We have now corrected this and verified that the SST and SSS fields (and the rest of them) shown in Fig. 3 of the revised manuscript are averaged over JF 2020. However, we keep the mixed layer heat budget analysis (now Fig. 10 of the revised manuscript) only for February 2020 since the plume does not arrive to the Amazon sub-region before mid-February 2020.

Line 473 - Do the authors mean temperature advection from the east? The temperature contours appear to be oriented east-west, with temperature increasing to the west. Advection from the south would bring cold water northward I would think, just looking at the SST contours in panels (a) and (b). Or perhaps there are warmer waters below the surface to the south within the ML?

We thank the reviewer for this comment. The reviewer is correct: the advection originates from the east. We show this in Fig. 9a of the revised manuscript where we can find the SST field in shading and surface currents in grey arrows. We comment on this in the main text in line 519 of the revised version.

Lines 474-479 - The temperature tendency within the <35 PSU contour is not just negative, it is positive in the southwestern region of this contour, with heating due mainly to horizontal advection, not atmospheric forcing. The region defined by SST > 26.7 degC and SSS between 35 and 35.4 PSU also seems to not exactly match the very narrow region of positive temperature tendencies. It is not clear where the authors are referring to when they talk about the core of the plume. A panel with SST and SSS together with the plume marked on the figure would facilitate this discussion. Also, this discussion contradicts that on lines 453-454.

We thank the reviewer for this comment. Below we detail the modifications we have implemented to address it.

Given the heterogeneous structure of Fig. 10a (the total temperature tendency map in the revised manuscript, old Fig. 8c), we have removed the dT/dt panel from the histograms (and in fact, the old Fig. 7 with the ocean histogram). The mean values within each bin were strongly influenced by highly variable dT/dt patterns, which limited the interpretability of the panel. As the reviewer notes, the temperature tendency is not uniformly negative within the interior of the plume (whose boundaries are now defined as the 35 psu isoline as shown in all panels of Fig. 10).

We now clarify that the warmest part of the plume exhibits negative total temperature tendencies, primarily driven by the negative atmospheric forcing. However, in its southwestern part, the total temperature tendency is positive due to horizontal advection from the east (Figs. 9a and 10b of the revised version of the manuscript). These clarifications have been incorporated along section 4.4.4 (Mixed Layer Heat Budget) of the revised manuscript. They are summarised between lines 539 and 544 of the new version.

Fig. 8e,f - Panel (e) is not discussed and is an order of magnitude smaller than most of the other terms. Suggest removing this panel. Likewise, although panel (f) is briefly mentioned, this term is also an order of magnitude smaller than the others and could be left out along with discussion on lines 480-482.

We thank the reviewer for this remark. However, we prefer to retain Fig. 8 (now Fig. 10) with all its panels for the sake of completeness. We have added a brief discussion of the panels that were previously not referenced in the text, so that all panels are now explicitly cited. Please find the discussion between 532 and 536 of the new version.

Line 487-488 - If this statement were true, would not the temperature tendencies be zero? They are in fact small compared to the advection, residual and atmospheric forcing terms. Is that what the authors are trying to say, despite the discussion on Lines 474-479 describing the tendencies?

We thank the reviewer for pointing out this inaccuracy. We have removed this sentence since the total temperature tendencies is not zero and it does not add any key information to the conclusions.

title - Suggest a change to the title as it seems to describe only one section of the manuscript. The latter part of the manuscript is spent understanding the ML budget. Maybe “On the Mechanisms Controlling SST and Ocean Mixed Layer Heat Content in the Northwest Tropical Atlantic: A Modeling Approach”.

We thank the reviewer for the suggestion. As a counterproposal, we suggest the following title:

“Mechanisms Driving Mesoscale Latent Heat Flux Variations and Mixed Layer Heat Content Evaluation in the Northwest Tropical Atlantic”

We believe that latent heat flux should appear explicitly in the title, as it constitutes a central component of our study.

MINOR EDITS

Figure 1 - Suggest splitting this figure into two different figures, one with panels (a) and (b) and one with panel (c). The current 3 panel layout is crowded and the text for panel (b) extends into panel (c).

We thank the reviewer for this comment. We have implemented the suggested changes, and the two figures in the introduction of the revised manuscript. As suggested by the other reviewer, we have added the seafloor depth as panel b in Fig. 2.

Line 70-73 - Change “shortens” to “shorten” but also check sentence structure as it does not read well.

Thank you for pointing this out. We have corrected this mis-spelling and the corrected version of the sentence is found in line 73 of the revised manuscript.

Line 125 - Do the authors mean freshwater, heat, and momentum fluxes? Turbulent does not make sense in this context since momentum fluxes are also turbulent fluxes.

We thank the reviewer for this remark. The reviewer is right and the text has been modified as suggested. Please find the corrected version in line 131 of the revised manuscript.

Line 240 & Fig. 2 - Can the authors add Trinidad and Tobago to these panels?

We thank the reviewer for this suggestion. A number 1 has been placed over Trinidad and Tobago and a number 2 over a region close to Barbados in Figs. 2 and 3 of the new version (Fig. 3 of the revised version is the old Fig. 2) so that geographical references are easier to follow in the main text.

Line 249 - Typo, “wuch” should be “such”.

Thank you for pointing out this mis-spelling. Please find the correct spelling in line 262 of the revised manuscript.

Line 263 - Change to “in the following sections.”

Thank you for the comment. We have changed the sentence as suggested. Please find it in line 276 of the new version.

Lines 274-275 - Sentence is not grammatically correct. Please fix.

Thank you for pointing this out. The sentence is corrected and can be found in line 288-289 of the revised manuscript.

Line 328 - Typo, should be “among” or “amongst”.

Thank you for pointing out this mis-spelling. It has been corrected and can be found in line 340 of the revised manuscript.

Sec. 4.4 heading - should be “the Amazon”

The reviewer is correct, thank you for noting this. The title of the section has been modified accordingly (see line 390 of the updated manuscript). In addition, following another reviewer’s suggestion and to improve the flow of the discussion, we have divided subsection 4.4. (Vertical structures) into four subsections addressing the atmosphere, the air-sea interface, the ocean mixed layer structure and the mixed-layer heat budget.

Fig. 6 caption - Please add that the values shown are for the Amazon box only for clarity.

Thanks for your comment. We have added this information to the caption of Fig. 7 of the revised manuscript (Fig. 6 in the old version).

Fig. 7 - The labeling on these panels is overall confusing since the x-axis for all panels is only labeled in panels (g) and (h), but a color bar is shown beneath all the panels. It would be better to include the Mesoscale SST anomaly tick labels and axis label in all panels for readability.

We thank the reviewer for this remark. The figure the reviewer is mentioning in this comment is no longer present in the revised version in the article. However, we have followed this advice to modify Fig. 7 (old Fig. 6) so that x-axis ticklabels appear on every panel.

Fig. 7 caption - Please state what the white arrows represent in panel (c). They are defined on line 446 but should also be defined in the figure caption. Also, what is their magnitude? Also, add that these panels are for the Amazon box only.

We thank the reviewer for this remark. However, after reading the major revisions we believe this figure is no longer pertinent for the manuscript.

Line 472-473 - I think the authors mean to refer to Fig. 8c, the temperature tendency panel, and Fig. 8d, the horizontal temperature advection panel, in this sentence.

We thank the reviewer for this comment. We have checked the figure references and have adapted them to the new numbering of the figures of the paper. Please, find the modifications between lines 518 and 521 of the new version.

REPLIES TO REVIEWER 1

We thank the referee for the careful reading of the manuscript and the constructive comments. We have thoroughly considered all remarks and provide detailed responses below. We believe that the suggested revisions have significantly improved the overall quality of the manuscript. The answers to the points raised by the referee are presented in the following sections.

MAJOR POINTS

Role of humidity. I found myself confused by the role of humidity. The confusion may be due to my ignorance, but I think the paper could try and explain the role of humidity in a clearer fashion. The paper concludes that atmospheric humidity is the main driver of LHF variations, which is supported by Fig. 4 etc.. However some other figures present results which confuse me. Fig. 6d shows extremely weak specific humidity variations near the surface, compared to larger saturation specific humidities (Fig. 6c). I believe the saturation specific humidity anomalies are due to air temperature anomalies strongly controlled by SST. The coupling coefficients also show a weak response of specific humidity to SST (lines 282-292). Despite these weak variations in specific humidity, you seem to show it is enough to be the main cause of LHF variations. I find this part confusing. I would appreciate the role of humidity to be better explained, particularly the connection between Fig. 6 and Fig. 4

We thank the reviewer for this remark. We would like to clarify this point. LHF is directly proportional to the difference between saturation specific humidity (q_sat) and specific humidity (q) (referred to in the main text as the specific humidity deficit). When considering the mesoscale SST-q coupling, we find a weak correlation between these two variables. We argue that the downward momentum flux from the top of the marine atmospheric boundary layer entrains drier air towards the surface (downward momentum mixing, DMM). Therefore, in the presence of DMM the surface specific humidity deficit varies in a manner very close to the saturation specific humidity, following SST mesoscale fluctuations.

However, in the absence of DMM (i.e. large-scale) we expect the near-surface specific humidity to adjust to the SST underneath following the Clausius-Clapeyron equation: the air over sea is saturated or almost saturated at the large scales. This provides a much smaller specific humidity deficit, as q and q_sat are closer to each other. Since the specific humidity deficit is small, LHF decreases as well.

Please, find a more detailed explanation of this point between lines 429 and 435 of the revised version of the manuscript.

Novelty. This is the third paper led by the first author on this topic (the title of the paper is almost identical to that of the earlier JGR paper). I understand that this is OK – people can build careers on the same topic – and that the topic is important. However I would like to see the distinction between this and the other papers made more clear. This new paper uses an ultra-high resolution coupled model, which is great, but why do you expect this to give novel results compared to the other papers? Perhaps in the

introduction you can state any limitations that arose in the previous papers (using reanalysis, satellite, in-situ data) and describe why the ultra-high simulation can fix some of these limitations and provide new results.

We thank the reviewer for this comment. The advantage of using a coupled simulation is that it provides gridded data of the air-sea interface which allow the robust computation of coupling coefficients and LHF statistics, as well as vertical profiles of both the ocean and the atmosphere (information that is too sparse in available in-situ observations and not present in satellites). We have added several sentences in the introduction to clarify this point (see lines 105 and 109 of the revised manuscript).

Minor points

Introduction. Add some motivation on why we are interested in latent heat flux for the atmosphere –possible clouds, convection.

We thank the reviewer for this comment. We have added text providing the motivation for our focus on LHF in the revised manuscript; please see the corresponding explanation between lines 33 and 37 of the new version.

Line 171. At this point, give some background as to why you are using the downscaling method, and why not just use the high-pass spatial field.

We thank the reviewer for this comment. We use this downscaling method since it has been shown to improve LHF representation in the WRF model by a factor of two (see Fernández et al., 2023). This means that if we compare the LHF obtained with the smoothed fields (wind, specific humidity etc, 150km cutoff length) with the model LHF output obtained at a high resolution (1km) and the downscaled LHF with the LHF model output, we are two times closer to the LHF model output. In addition, this downscaling algorithm allows to disentangle the effect on LHF changes from the different controlling variables (i.e. wind speed, specific humidity…) and, in particular, their mesoscale SST-induced changes in an easy fashion. It only uses linear regression. We now state this explicitly in the revised manuscript. The corresponding modifications appear between lines 178 and 179.

Fig. 1b is a bit complicated, especially for the first figure of the paper. I suggest moving the details of the relative wind and CFB to section 3.2 (or create a new section 3.2) and move Fig. 1b to a new Fig. 2

We thank the reviewer for this remark. We agree that Fig. 1 is complex. However, we prefer to retain the schematic of the CFB in the introduction, where the CFB is first introduced. Following another reviewer's suggestion, we have divided the original Fig. 1 in two figures: the first illustrating the physical mechanisms of interest, and the second showing SSS, the main regional features, seafloor depth and the sub-regions of interest. To further facilitate the comprehension, we have added clarifying text in the introduction where the figures are cited. These modifications can be found between lines 62 and 69 of the modified version.

Caption of Fig. 4 and other locations. At a few points in the manuscript, the large number of samples (e.g. 15,000) is mentioned. It is worth noting that for statistical significance testing, one should use the number of independent samples. (For example, many samples may be over the same eddy.) There are ways to take this into account, e.g. using the autocorrelation of the data.

We thank the reviewer for this comment. We have computed the autocorrelation length of the fields being binned (i.e. SST mesoscale anomalies, wind-speed mesoscale anomalies, humidity mesoscale anomalies, etc), and in all cases it is of the order of 100 km (consistent with the 50-250 km band-pass Gaussian filter applied). Given the model grid spacing (1 km in the ocean and 2 km in the atmosphere), this would require sub-sampling the model output by selecting only 1 out of every 100 ocean grid points or 1 every 50 atmospheric grid points. While such sub-sampling might be feasible over the whole EURECA domain, it becomes impractical when performing the linear regressions within the Amazon, Downstream and Tradewind subregions, where the number of available samples would be insufficient to construct meaningful binned distributions. For this reason, we chose not to apply autocorrelation-based sub-sampling.

In Fernández et al. (2023) we performed the sub-sampling based on autocorrelation length using satellite and reanalysis data and obtained coupling coefficients consistent with those for the EURECA region. This provides confidence in our approach, even without applying autocorrelation-based sub-sampling in the present analysis.

To avoid potential confusion, we have removed the reference to the explicit number of points per interval from the figure captions.

Finally, the statistical significance in all the regressions present in the paper is based on the bin means and not the individual values so if two adjacent values always present similar values, they are only “counted” once as they are likely to be in the same bin.

Lines 543-545. This needs more explanation. Does adding the downscale method actually enhance representation of air-sea fluxes in the model, or is it just a diagnostic?

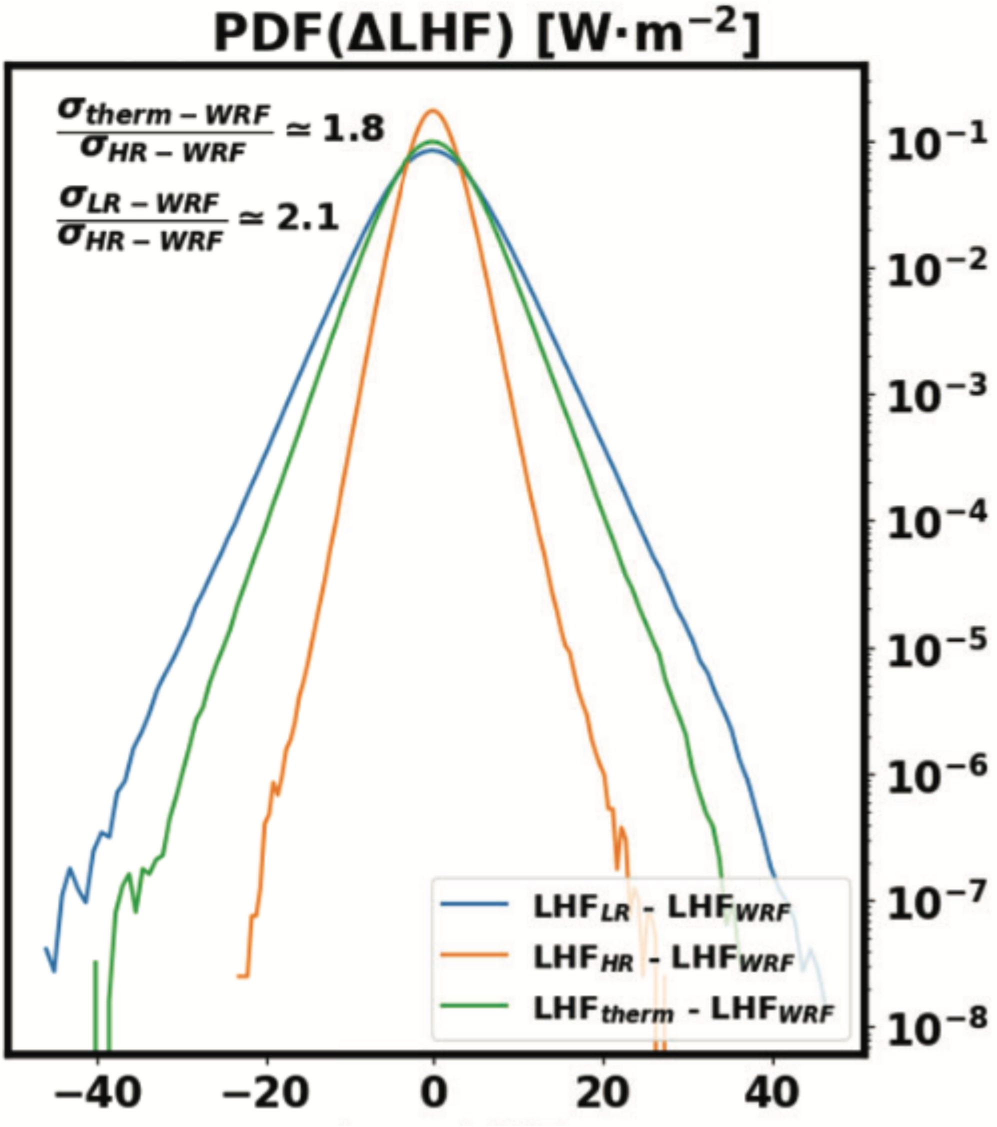
We thank the reviewer for pointing this out. We have not tested explicitly if the downscaling algorithm improves LHF representation in this model. However, we did test it in an ensemble of SST-forced high-resolution WRF simulations in Fernández et al. (2023). In that study we computed three LHF datasets:

1. LHF_HR : LHF computed using the downscaled surface winds, specific humidity and air temperature.
2. LHF_LR: LHF computed using the smoothed (Gaussian filter with a cutoff length of 150km) surface winds, specific humidity and air temperature.
3. LHF_WRF: LHF computed using the model output, which had a high resolution (1km).

LHF_therm represents exactly the same as the LHF_therm from our current manuscript. However, let us focus only on the three LHF datasets above.

In the figure below, the orange curve represents the probability density function (PDF) of the LHF_HR- LHF_WRF. The blue curve represents the PDF of LHF_LR - LHF_WRF. We observe that the orange PDF is approximately 2 times narrower than the blue one (also shown by the ratio in the standard deviations in the top left of the figure).

We have added clarification to this point. Please find the changes between lines 604 and 609 of the new version.



Appendix D. This is interesting but it feels a bit out-of-place. Where is Appendix D referenced in the manuscript? I feel like it could be expanded upon and presented elsewhere, as it is not central to the paper.

We thank the reviewer for pointing this out. As suggested, we have decided to remove Appendix D and replace it with a sentence in the introduction which states that DMM dominates over the EURECA region, a result already established in previous studies based on satellite observations. The corresponding modifications can be found between lines 60 and 61 of the revised manuscript.

Very minor + Grammar Wording

Lines 6-9. Please write without the parentheses for different cases

We thank the reviewer for this remark. The abstract has been rephrased accordingly. The revised wording can be found between lines 6 and 9 of the updated manuscript. Please, note that we have retained the parentheses associated with the coupling involving specific humidity, as this coupling differs from that associated with wind speed.

Line 28. (THFs, comprised of latent and sensible heat fluxes)

We thank the reviewer for this comment. The text has been modified accordingly and the change appears in line 26 of the updated version of the manuscript.

Line 30: which include the latter including

We thank the reviewer for the remark. The text has been modified accordingly, and the revision can be found in line 28 of the revised manuscript.

Line 33. 250km seems to precise about 250km ? But, it probably varies regionally.

The reviewer is correct. We have modified the text and added the “about” so that it is less specific. The corresponding change can be found in line 32 of the updated manuscript.

Line 33. Typo on Gill 1982.

Thank you for this remark. However, the study has only two authors, which is why both surnames appear in the citation. We are therefore unsure what the reviewer considers to be the error.

Line 39: Add “and coupled models (Small et al. 2019).

We thank the reviewer for this remark. The document has been modified accordingly, and the reference has been added. Please see line 41 of the revised manuscript.

On this theme, there is a body of literature on stochastic coupled and ocean-only models which are relevant here (Frankignoul and Hasselman1977, Barsugli and Battisti 1998, Frankignoul et al 1998, Wu et al. 2006, Bishop et al. 2017, Laurindo et al2022, JGR). You do not have to reference them here, but they should be of interest. Most are cited in Bishop et al. (2017)

We really thank the reviewer for these references. As the reviewer suggested, we will not include them in this paper but will certainly consider them in future work. Thank you very much.

Line 50 "from hours to weeks to long-term climatologies (e.g. Chelton et al., Minobe et al. 2008). (I am personally interested in how the processes change between hours and days and months – I may be giving my name away by referencing here Small et al. 2023, J. Clim..)

We thank the reviewer for making this point. The text has been modified accordingly, and Small et al. (2023) has been cited. Please, find the changes in lines 51 of the revised manuscript.

Lines 280-281 and 314-318 (and to a lesser extent 416-422). I think that comparison with work done by co-authors using the same simulation may be considered just a "sanity-check" or consistency-check. It is an important testing procedure but probably should not be cited. It would be better to compare with an independent study if available. However it is fine to compare your model analysis with your work on in-situ and reanalysis data (which you do later) as these are independent datasets.

We thank the reviewer for these remarks. References to the work by co-authors that used the same simulation for comparison with our results has been removed and the references to studies based on independent data have been retained.

Line 52: "mesoscale eddies" -> "and fronts"? Some of the referenced papers discuss time-averaged fronts (e.g. Minobe et al. 2008).

We thank the reviewer for this clarification. "and fronts" has been added to the sentence, which now appears in line 54 of the revised version.

Line 57: "resolution is increased" "grid spacing is reduced"

Thank you very much for this comment. The proposed wording has been adopted and is now included in line 59 of the revised version of the manuscript.

Line 62: "Currents and winds are aligned, and surface stress is reduced"

Thanks for this information. It has been added to the text. The corresponding modifications appear in lines 64 and 65 of the revised manuscript.

Lines 97-98: Reword without parentheses for clarity

Thank you for this suggestion. The sentence has been rephrased without the parentheses for improved clarity, as recommended by the reviewer. The corresponding changes can be found between lines 98 and 100 of the revised manuscript.

Line 168. Wording is a bit confusing. I suggest "same variables as LHF_u, but smoothed with…"

We agree with the reviewer that the original wording was somewhat confusing. We have rephrased the sentence as suggested, and we hope it is now clearer. The revised text appears in lines 176 and 177 of the new version.

Line 229: As in Gevaudan

Thank you for the remark. We have modified the text accordingly and the change can be found in line 240 of the revised manuscript.

Fig. 2. Can you add bathymetry onto one of these panels?

We thank the reviewer for this comment. As suggested, we have added the model bathymetry to Fig. 2b of the new version of the manuscript. We decided not to add it with the model climatologies since the panels are already very charged with shading, contours and/or arrows..

Lines 249-250. Some mis-spelling and grammar issues

We thank the reviewer for this remark. The misspelling and grammatical errors have been corrected. The revised sentence now appears between lines 262 and 263 of the updated manuscript.

Line 258. Replace with "lacks strong temperature and salinity signatures at the surface."

Thank you very much for this remark. We have incorporated the modification into the revised manuscript. It appears in lines 271 and 272 of the new version. To be more precise, we removed the mention to temperature.

Sentence beginning line 272 could be moved down to the discussion of s_u (line 276).

We thank the reviewer for this remark. However, we are unsure we fully understand the modifications being requested. As we interpret it, line 272 of the previous version concludes the discussion of the effect of surface currents on wind speed. Therefore, it should follow the analysis of s_w. In contrast, lines 276 and onward address s_u, which quantifies a different physical process from s_w and thus should remain distinct from the s_w discussion. Please, let us know if we are misunderstanding the reviewer's intent.

Lines 304-305. I think smoothed variables are not obtained by subtracting low-pass values from the original data.

We thank the reviewer for this remark; the reviewer is correct. The smoothed values are obtained directly by applying the low-pass filter. We have modified the text accordingly; the changes appear in line 321 of the revised manuscript.

Fig. 5 labelling is hard to follow. Suggest to use words on the horizontal axis labels, e.g. "Relative minus absolute wind" (blue) and "Relative wind (no CFB) minus Relative wind (CFB)" (orange) and use the corresponding equations in the text.

We thank the reviewer for this remark. The labeling of Figure 6 (Figure 5 in the old version) has been updated accordingly.

Line 369: “structure of the Amazon region”

We thank the reviewer for this remark. Taking into account this suggestion and those from the other reviewer, we have revised the subsection title as follows:

The Amazon Sub-region Vertical Atmosphere and Ocean Structures and Mixed Layer Heat Budget

Please find the change in line 390 of the new version of the manuscript.

Line 379: Give equation for N^2

We thank the reviewer for this remark. The Brunt–Väisälä equation has been added to the manuscript. The corresponding modification can be found in line 401 of the updated version.

Line 383. The pressure adjustment can also give dipoles due to a secondary circulation - Small et al. (2003 (Tropical Instability Waves)).

We thank the reviewer for the remark. However, in Small et al. (2003) it is reported that although the wind speed increases are in phase with the SST maximum, the extrema in moisture and air temperature form downwind of the SST maximum, with pressure adjustment (PA) associated with advection by the mean flow. This is not the case in our study: the largest saturation specific humidity anomalies occur directly over the strongest SST anomalies, and specific humidity shows little variation across the range of SST anomalies. In addition, previous studies from satellite-based studies (Fernández et al. 2023) that rule out a significant role of PA in the EURECA region. This leads us to interpret the dipoles in the wind-speed histogram as arising from downward momentum mixing (DMM). We have added two sentences in the main text to clarify this point. Please see lines 411-416 of the revised manuscript.

Section 4.4 could be separated into a sub-section on atmosphere and one on the ocean (with the air-sea interface discussed between the two perhaps)

We warmly thank the reviewer for this suggestion. To improve the flow of the discussion, also taking into account the comments from the other reviewer, we have divided Section 4 in four separated subsections :

1. Atmosphere Vertical Structure
2. Ocean Surface Features
3. Vertical Ocean Structure
4. Mixed-Layer Heat Budget

Fig. 8. Black contours in panels c-h are salinity?

Yes, they are, and this information had not been indicated in the figure caption. We have now added this information to the captions of all the figures where such contours are present in the revised version (Figs. 9 and 10). Thank you for this comment.

Line 492-493. “fully resolving ocean mesoscale” is a motivation that could be put in the Introduction – see major comment on Novelty.

We thank the reviewer for the suggestion. We have added the term “fully resolving ocean mesoscale” in the introduction. The corresponding modification appears in line 106 of the revised manuscript.

Line 508. Wording issue , should be “negative in Amazon and downstream”.

We thank the reviewer for pointing this out. However, we have modified the phrasing of that sentence in order to highlight that these coefficients are different from the estimates of the Clausius Clapeyron equation. After discussion with other co-authors, we think this message is more relevant than the specific sign of the coefficients. Please, find the new sentence in lines 564 and 565 of the revised manuscript.