

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?

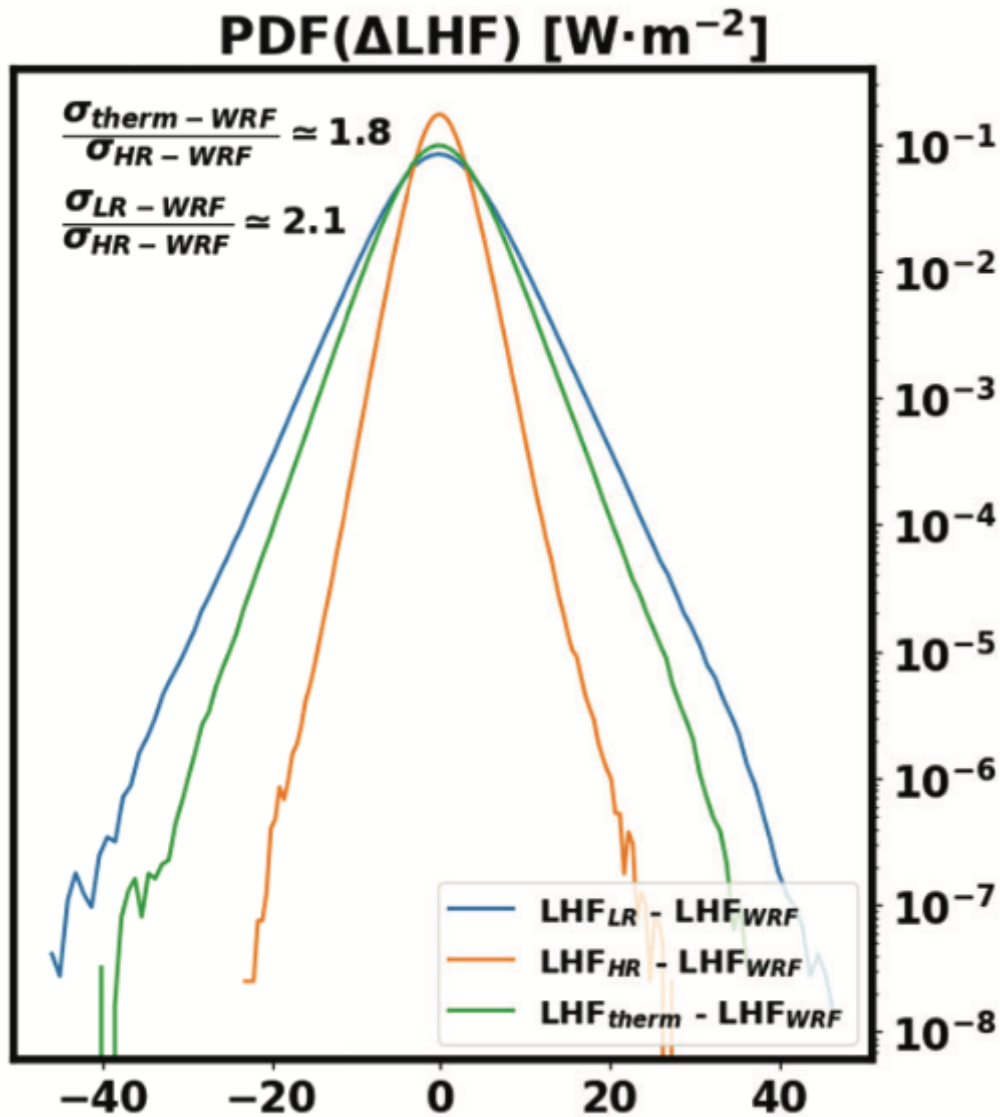
We thank the reviewer for pointing this out. We have not tested explicitly if the downscaling algorithm improves LHF representation in this model. However, we did test it in an ensemble of SST-forced high-resolution WRF simulations in Fernández et al. (2023). In that study we computed three LHF datasets:

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.