Review of “On the mechanisms driving latent heat flux variations in the Northwest Tropical Atlantic: a modelling approach.” By Fernandéz, Speich, Conejero, Renault, Desboilles, Pasquero and Lapeyre, submitted to Ocean Science/EGUsphere, 2025.

Summary

The paper aims to investigate the processes causing surface latent heat flux (LHF) variability in the Northwest Tropical Atlantic offshore of Brazil and the Amazon. Selected sub-regions are chosen – one close to the Amazon, one downstream and one in the open ocean “Tradewind”. A very-high resolution (1-2km) coupled regional model is used (as in Conejaro et al.). The role of SST, ocean current, and atmospheric boundary layer in affecting LHF is examined. A “downscaling” approach is used, based on filtering and coupling coefficients. The paper finds an important role for atmospheric humidity variations in causing much of the LHF variability.

Overall, I find the paper interesting and appropriate for the journal, and covers the important topic of drivers of latent heat flux variations: this flux is important to both the atmosphere (buoyancy changes, convection etc.) and the ocean (affecting both temperature and salinity via evaporation, etc.) The choice of model seems good, and the diagnostics are impressive. I believe it is suitable to publication after the major and minor points below are addressed.

Major points

1. 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.

2. 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.

Minor Points

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

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.

Fig. 1b is a bit complicated, especially for the first figure of the paper. I suggest to move 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

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.

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?

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.

Very minor + Grammar/wording

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

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

Line 30 which include-> the latter including

Line 33. 250km seems too precise-> “about 250km”? But, it probably varies regionally.

Line 33. Typo on Gill 1982

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

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, Bshop et al. 2017, Laurindo et al. 2022, JGR). You do not have to reference them here, but they should be of interest. Most are cited in Bishop et al. (2017).

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..)

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.

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

Line 57. “resolution is increased” - > “grid spacing is reduced”

Line 62. “currents and winds are aligned, and surface stress reduced”

Lines 64-65. “currents oppose surface winds, and surface stress is increased”.

Lines 97-98. Reword without parentheses for clarity.

Line 168. Wording is a bit confusing. Suggest “same variables as LHF_u, but smoothed with…”

Line 229. “As in Gevaudan…”

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

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

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

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

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

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.

Line 369 “structure in Amazon region”

Line 379. Give equation for N^2.

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

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).

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

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

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