Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-14-RC2, 2018

Ocean Science
Discussions

Interactive comment on "Dynamics of North Balearic Front during an autumn Tramontane and Mistral storm: air–sea coupling processes and stratification budget diagnostic" *by* Léo Seyfried et al.

Anonymous Referee #2

Received and published: 8 June 2018

The paper presents an original study of the North Balearic Front dynamics during a strong wind event, combining observations, high-resolution coupled modelling and stratification budget. The authors give a complete overview of the atmospheric event and of the ocean front evolution. The validation of the coupled simulation is very convincing. The use of a stratification budget seems very promising in particular to better understand the coupled mechanisms involved.

Nevertheless, the fact that the equations are not fully detailed and that the residual term is not described but finally appears as a dominant term, gives the impression that

only a part of this budget is considered.

Consequently, I suggest some major revisions to improve the paper before accepting its publication.

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So, my main comment concerns the stratification budget (section 6) which is very briefly presented:

First, it could be very helpful to give a physical view of EBF. Reading Thomas and Lee (2005), I understood it is a destabilizing flux leading to convection and frontal intensification, but clearly I am not sure about my interpretation. . .

p10, line 10: "the friction induced a wind-driven or Ekman Buoyancy Flux (EBF) given by:"

Where does equation 6 come from? There is no residual term in equation 13 of Thomas and Lee (2005)? What is the value of H considered? Is the equation valid for any H or is there a limit considering the depth of the Ekman layer?

Maybe, one possibility to clarify this section is to give an enlarged description of the reasoning in an annexe. Obviously, the idea is not to reproduce the work from Thomas and Lee (2005) but to try to give the main insights.

Concerning the residual term, it appears later in the text that you can attribute its value to horizontal advection or vertical advection/Ekman pumping. How? If there is a way, you must extract these terms from R and plot their values to complete the stratification budget.

Finally, in section 7, it could be also interesting to discuss the possible limitations due to the hydrostatic assumption or to the convection parametrization in the ocean model.

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Other comments:

p2, lines 2-9: There is a kind of mixing between front/current in the very first sentence of the introduction. In my opinion, it could be helpful to describe the cyclonic circulation with the various branches/currents and then how it constrains/forms the surface density gradient. . . ?

p2, line 21: “maintained strong precipitation offshore and over the southeastern French coasts”

p2, lines 30-32: I am curious to know if there is any action of the perpendicular wind component?

p4, lines 20-21: It appears from fig 4 that the effective resolution of the OSTIA and Copernicus products are much larger than the indicated resolution (6 and 1 km, resp.). This is somehow mentioned p6, line 25 (“The horizontal resolution of the latter...of the model. ”), but could be rapidly indicated in this section 2.2 or commented.

p6, line 34: replace “extremity” by “end”.

p8, lines 4-5 should not be in italic.

p8, line 10: “Modified”: This is the first time this term appears. Could you explain it?

p9, line 16: Remove here the comment about density: “During strong wind event, evaporation dominated and led to an upward (positive) water flux.”

p9, line 19: You may refer here to fig 2 (instead of fig 10a)?

p10: See my main comment + Please, detail g, \rho_0, Qnet and Fw; Some sentences should not be in italic.

p12, line 14: “during IOP16b”

p13, line 26: “the front is less marked” → this is not so clear for me from figs 4 and 7. Are you talking about a smoothing? a reduction of the temperature difference?

p14, line 2: “For example, Thomas et al. (2016) show”

p15: refs Drobinski et al. and Ducrocq et al. → 2014; In addition, I think URLs in this “References” section must refer to the “doi.org” pages.

Figure 5: precise “observation” or “simulation” on each panel if possible. For (c), the caption tells it is the simulation bias but it seems to be more ‘glider minus simulation’. . .

Figure 7c: The red contour is not visible.

Figure 8 is difficult to read. Please consider here to plot separately the density sections for 25 oct., 30 oct. and the differences.

Figure 10: Please, improve the resolution in order to distinguish the arrows. Precise in the caption if the arrows are for the wind or the wind stress + a reference length for vectors must be added.

Figure 14: the plots of “B0+EBF” are not necessary. . .

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