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Interactive comment on “The role of heat flux-temperature covariance in the evolution of weather systems” *by* Andrea Marcheggiani and Maarten H. P. Ambaum

Anonymous Referee #1

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Review of "The role of heat flux-temperature covariance in the evolution of weather systems" by Marcheggiani and Ambaum

The authors discuss the covariation between air-sea heat fluxes and tropospheric temperature in the North Atlantic. At synoptic timescales, they find a negative covariance between these fields. They propose that air-sea fluxes on the cold sector of atmospheric storms are enhanced where the spatial variability of the SST is the strongest leading to a subsequent variability in the atmospheric temperature.

Although the paper presents original results about air-sea interactions, I am a bit skeptical in their significance. Also, I am concerned by different issues that would need to

be addressed before publication.

Recommendation: Major revision (or perhaps even reject in present form)

— One of the drawbacks of the paper concerns the physical meaning of the anomalies. The spatial variability of the sea surface temperature (SST) in the region near the Gulf Stream (GS) is generally due to the GS SST front. Here the spatial variability of the fluxes and temperature anomalies for timescales inferior to 10 days are not even presented. Is it related to the GS front? Centered over the front, or on its warm side? Is it related to oceanic eddies (as claimed near the end of the paper)? Understanding what are the characteristics of this variability is essential to the interpretation of the main results.

Another drawback is that a major process of air-sea interactions is completely overlooked: the so-called "oceanic baroclinic adjustment", as introduced by Nakamura et al. Their mechanism relies on the feedback of atmospheric temperature on air-sea fluxes. It seems to me that the results of the present manuscript are in disagreement with their findings. This issue should be tackled.

A last drawback relies in the motivation of the paper, i.e. the study of the generation/depletion of available potential energy (APE) by air-sea fluxes. Surface fluxes are involved in the budget of temperature inside the boundary layer, not in the 850hPa temperature budget. Hence the product air-sea heat flux times 850hPa temperature (above the MABL) cannot be interpreted as a term related to APE production. It is more simply related to the relation of air-sea fluxes with the free troposphere.

Detailed comments are given below.

— Major points:

1) I would have guessed that the variability of the air-sea fluxes lies above the warm side of the GS front, so that the spatial average used in (1) only captures this mode of variability. Indeed this seems to be the case when looking at Fig.3. But, this would

contradict line 257 where you say that spatial variability (at synoptic timescales) is due to mesoscale oceanic eddies.

Here are some different questions:

- Can you provide some information about the variability of SST (e.g. std(SST'))?

- Given the dataset you use (ERA-I at 1.5deg of resolution), you are unable to represent the small spatial scales present in the fields you examine. I would like that you redo Figure 3 with a higher resolution dataset (e.g. ERA-5 at 0.25deg) to see more clearly whether the SST front is important or not.

- I do not see the point to show the SLP variance in Figure 1. Instead, could you present the std of F' and T' as well as the SST contours?

- Lines 212-213, You present a scenario where a cold front moves above a spatially varying SST, which would trigger spatially varying heat fluxes and then spatially varying T850. But, in my opinion, the cold front is already associated with a strong T850 anomaly. Please explain why and how this anomaly will be enhanced (in particular at what spatial scales).

- Can you show a figure of the time averages of [F'* T'*] and [F' T'] to contrast in which spatial region the synoptic eddies give a different response to the total eddy field?

2) You do not discuss at all of the mechanism proposed by H. Nakamura (Nakamura et al 2008 in GRL , Sampe et al. 2010 in J. Clim., Hotta and Nakamura 2011 in J. Clim), called the oceanic baroclinic adjustment (see Fig.12 in S10 or Fig. 20 in H&N11). This mechanism is related to a feedback between air-sea fluxes and surface temperature. Hotta and Nakamura relate the cold air advection of synoptic eddies to the interaction between air-sea temperature difference and air-sea fluxes. They stress the importance of SST gradient and surface baroclinicity. How does this relate to your Figure 4 and the co-evolution of T' and F'? More generally, please discuss their mechanism in comparison to yours.

3) I don't understand why you motivate your study by saying that FT is related to potential energy generation :

- Diabatic heating does not produce work, contrary to what is stated in line 2.

- Surface fluxes are only involved in the budget of temperature inside the boundary layer (see for instance Small et al. 2013 in Clim. Dyn.). Hence the product air-sea heat flux times 850hPa temperature has no physical meaning, per se, and cannot be related to APE production, contrary to what is stated in lines 70-71.

- I don't understand why flux-temperature covariance affects baroclinicity (line 160), or APE generation (which is quite different from the former).

4) I have some trouble to understand how you relate you covariance index to baroclinicity.

- Lines 158-159, you state that "baroclinicity was found to be depleted during extreme FT". However, from Fig.3b, it seems to me that baroclinicity is enhanced. Please explain.

- Also, you seem to relate mean baroclinicity (related to temperature gradients) and available potential energy (related to temperature anomalies), line 160. Please explain.

- Lines 252-253, you state that "air-sea exchanges drives the depletion of the baroclinicity over the domain". You seem to conclude this statement from the FT index life cycle which is not related to the baroclinicity.

5) You seem to think that surface air temperature would not react as 850hPa temperature when computing covariances. Could you compute pdfs like Fig.2 using surface temperature (either 2m or 10m) instead of 850hPa temperature? From that point of view, I would also like that you add the 2m temperature and the SST in Figure 3.

5) The argument about the triggering for heat flux variability (line 265) would need more firm bases. Could you complement Figure 8b with time evolutions of sea level pressure

and surface wind direction?

6) The pdf file is really too big (40mb). It made my printer crashed. I urge you to produce a much smaller size pdf.

— Minor points:

a) Figure 1 is too small when printed. Also, I don't understand why you chose to plot the SLP standard deviation. It would have been more logical to plot the T850 and the air sea-fluxes standard deviations (in blue and red) as well as the SST, since it is the subject of this paper.

b) Baroclinicity (line 154) should be defined.

c) Can you keep the spatial projections the same between figures: by choosing either the Conus representation (Fig.3) or the cylindrical one (Fig.1)

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