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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 #2

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In this study, the authors examine the co-variability of surface sensible heat flux and 850 hPa temperature, and found that they are generally negatively correlated. They constructed phase diagrams which suggests that most of the strongest interactions occur in the cold sector of the weather systems, with the flux increase leading the 850 temperature increase, and hypothesized that the spatial variability in the SST is important in increasing the spatial variability of the temperature field. I think the results are interesting. Nevertheless there are a number of issues that are not well explained and should be addressed in the revision.

Major comments: 1) The authors define the FT index by the spatial covariance be-

tween time anomalies in air-sea heat flux and 850 hPa temperature (equation 1). Why use both spatial and temporal deviations? Can the authors motivate this a bit better? How might an equation governing the APE, defined as both deviation from spatial and temporal mean, look like? The original definition by Lorenz is for spatial eddies (deviations from zonal mean). Later, Orlanski and Katzfey (1991) derived an alternative form for transient eddies (deviation from time mean). Perhaps the authors should provide reference for defining the APE as deviation from both time and spatial average? Defining the "energy" for a local region by subtracting the mean over that region is not necessarily useful due to the ambiguity of flux and conversion terms as Plumb (1983) pointed out.

References: Orlanski and Katzfey, 1991, JAS 48, 1972 Plumb, 1983, JAS 40, 1669

2) Related to the preceding point, to me, subtracting both the spatial and time mean makes it more difficult to visualize exactly how the passage of a system (e.g. a cold front) over the region would look like. Perhaps the authors should show figures corresponding to a time sequence of both the total fields and the eddy fields to make it easier for readers to understand some of the relationships found in this paper which seem to be a bit counter-intuitive.

3) A surprising result is that spatial variability in F' leads the spatial variability in T'. For weather systems of this time scale, one would expect that it is the atmospheric anomalies that force F', and thus it is, as the authors wrote, "counter-intuitive" (line 211). The authors explained that this "can be explained by the advection of the cold air mass, in the cold sector of a weather system, moving over a more spatially variable SST field such as that of the Gulf Stream extension. SST variability would trigger heat flux spatial variance which would then lead to temperature variance generation". I don't think I can understand this explanation. As the authors point out, F' nearly always damp T', and thus it is difficult to imagine how spatially varying flux, which acts to mostly damp the temperature anomaly, might give rise to increase in the temperature variance. Perhaps the authors could show some sequence of snapshots along the phase space

trajectory to show how this could happen and thus explain this "counter-intuitive" point better?

4) One speculation about F' leading T'. F' reacts to surface temperature anomalies. The surface front leads the 850 hPa front by some time, could this lead to some time lag between F' and T'? Fig. 8b apparently shows upper level temperature anomalies leading lower tropospheric anomalies, but this is for the large scale baroclinic wave in which T anomalies tilt eastward with height (e.g. Holton's text book; Lim and Wallace 1991). The variance increase likely corresponds more to the propagation of the front rather than the large scale temperature anomaly associated with the baroclinic wave? Can the authors show that this is not the case?

Reference: Lim and Wallace 1991, JAS, 48, 1718

5) Lines 251-254: Increasing F' followed by decreasing baroclinicity does not really imply that baroclinicity is depleted by the air-sea exchange. Baroclincity could be depleted by the growth of the baroclinic wave which occurs at the same time as the air-sea exchange is increasing. Causality cannot really be inferred when several things are occurring at about the same time, even with some slight lead-lag relationship.

6) Lines 59-61: There are "local" estimates using reanalysis data. For example, Chang et al (2002) showed that near surface sensible heat flux damps APE. See also Swanson and Pierrehumbert (1997) who also showed that 850 hPa temperature anomalies are strongly damped by surface fluxes over the ocean.

References: Chang, Lee, and Swanson, 2002: J. Climate, 15, 2163 Swanson and Pierrehumbert, 1997: JAS, 54, 1533

7) Lines 270-272: As pointed out above, Chang et al (2002) showed that latent heating (formation of cloud and precipitation in the warm sector) does generate APE, but near surface sensible heating damps APE. They also showed that over the Atlantic, the net effect is damping in winter but there are some regions where there is net generation.

8) In several places, the authors alleged to the importance of oceanic eddies (lines 91, 256, 258, 292). The data used is 1.5 degrees, and even the full resolution of ERA-Interim cannot really resolve oceanic eddies. If oceanic eddies are so important then how could the analysis based on ERA-Interim reveal that?

9) The figures need to be improved. The legends are really small and can't be clearly seen without enlarging the figures by a lot.

Minor comments:

i) Line 156: "lies almost entirely on the negative side of the FT index". I thought the FT index is always negative (line 97)?

ii) Line 237: "A downward propagation of the temperature anomalies" - this is not really "propagation" - related to the eastward tilt of temperature with height in medium scale baroclinic waves discussed above.

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