

The role of heat flux-temperature covariance in the evolution of weather systems

Marcheggiani and Ambaum

Author response to reviewers

We truly appreciate and are thankful for the effort that has been put by the Reviewers in reviewing our manuscript. Their comments are thoughtful and insightful and in responding to them we believe that the manuscript can benefit substantially. We hope that all their concerns have been duly addressed.

Reviewer's comments are reprinted in a *thinner and italicised style*, our response is typed below it in a thicker and non-italicised style.

Figures from the original manuscript are referred to following the manuscript's order while new figures included in this document are labelled as Figure AR# (Author Response).

Author response to Reviewer #1

We would like to thank Reviewer 1 for the detailed and insightful comments they gave on the manuscript, highlighting some unclear passages in the manuscript and highlighting studies on air–sea interaction which we did not discuss in our initial manuscript. Below we give a point-by-point response to the issues raised by the Reviewer.

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.

We believe there may have been some misunderstanding arising from our description of the mixed time-space anomalies. These are built as the spatial covariance of the departures from a 10-day running mean (which corresponds to a high-pass filter in the frequency domain), thus obtaining an instantaneous description of spatial patterns of synoptic-timescale variability (i.e. 10 days and below), which is what the paper is about. By removing a 10-day running mean, we are filtering out lower-frequency variability, such as seasonal variations, which may otherwise dominate the spatial variance, and which describes different physical processes.

We also emphasise that the high-pass time filtering occurs on the fluxes and the atmospheric temperatures, not on the SSTs. This means that the spatial variability caused by eddies on the gulf stream temperature front interacting with synoptic weather systems are in fact represented in our covariance index, and we show evidence that in the initial stage of development this is the key source of covariance.

We will rephrase the relevant passages in the manuscript to make this all more explicit and clear.

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.

We are very thankful to the reviewer for highlighting the studies by Nakamura & co-authors on the role of the oceanic temperature front in storm track dynamics. In particular, they highlight the importance played by SST fronts in forcing a surface air temperature gradients through differential sensible heating across the SST front. This was shown to be essential for the maintenance of strong near-surface baroclinicity, which anchors the climatological storm track.

Our study does not contradict these results; in fact they are consistent with each other as well as complementary to each other. We find that the spatial variance of the fluxes, indeed

including contributions of the N-S gradient of SSTs over the oceanic front, are associated with instantaneous depletion of baroclinicity. This is consistent with the mechanism discussed in a series of papers by Ambaum & co-workers highlighting the role that eddies play in temporarily depleting the baroclinicity in a predator–prey like relationship; this relationship is really a familiar instance of the nonlinear life-cycle of midlatitude eddies where meridional heat fluxes locally deplete the meridional temperature gradient in the atmosphere; in the older literature this quasi-periodic predator–prey relationship would have been described as an index cycle. However, this does not contradict the fact that high eddy activity on average must be geographically associated with high baroclinicity, which is essentially what the Nakamura papers are about, and of course also classical papers, such as Hoskins & Valdes (1990), and also Ambaum & Novak 2014.

So on a synoptic time-scale baroclinicity is depleted by synoptically induced variance of fluxes, as the many diagnostics in our manuscript show in various ways, but climatologically of course the storm track is anchored geographically by the high temperature gradients in the oceanic front, as highlighted by processes elucidated in the papers referred to by the reviewer, and in further detail in Swanson & Pierrehumbert 1997.

It is clear that this mutually complementary but consistent view on baroclinicity and spatial SST variance is an angle which we did not at all highlight in any detail in the manuscript, and we will discuss this in much greater detail in the revision.

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.

We agree with the reviewer that our index is not formally equivalent to a term in the APE production budget —indeed, we never claimed as much— and we will rewrite the manuscript to make that clearer than we managed to do in the first version. We acknowledge that the hybrid framework we use can lead to confusion and we will rephrase in a clearer way the reasons behind its use in our study.

In our paper we work towards a hybrid understanding of how APE can be affected locally, in particular in response to coupling of the free troposphere with the surface. As the reviewer pointed out, the intensity and sign of surface heat fluxes are typically computed from the energy budget at the surface, hence their covariation with higher layers of the atmosphere is not trivial, and we believe it can have an effect on the evolution of weather systems.

More informally, we examine how synoptic heat fluxes contribute to enhancing or depleting the local synoptic variance in the lower tropospheric temperature field. This local temperature variance is of course part of the global APE integral in the standard Lorenz energy cycle.

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.

We look at the spatial covariance between heat flux and temperature time fluctuations from a centred ten-day average thus we expect part of the total variability over the Gulf Stream extension also to derive from the interaction between the atmosphere and colder waters. In particular, we surmised that the higher level of variability observed over the western end of the North Atlantic could be traced back to the presence of mesoscale oceanic eddies, which would provide for stronger spatial SST contrasts, and hence flux variance. In fact, in the eastern North Atlantic spatial variances and covariance between heat flux and temperature is much weaker (though we did not include this in the manuscript).

Here are some different questions:

- Can you provide some information about the variability of SST (e.g. $\text{std}(\text{SST}')$)?

SST time anomalies, defined as those used in the computation of the spatial covariance, would be much weaker than those for air temperature or heat fluxes as the former vary on much longer time scales. We point out again that we do not analyse the high-pass filtered SSTs, but the high-pass filtered fluxes. These contain the spatial variance introduced by the SST spatial variance, even if that had been chosen fixed in time.

The standard deviation in time of the SST is simply not a relevant diagnostic for pointing out a source of spatial variance in the synoptic time scale fluxes. A fixed SST front with no temporal standard deviation would still induce the spatial variance on synoptic time scales of the fluxes, which we diagnose. We will further emphasise this property in the revision to make this point clearer.

- 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.

While it is true that ERA-I at $1.5^\circ \times 1.5^\circ$ resolution does not capture the smaller spatial scales, we found that using ERA-5 leads to slightly larger values for spatial covariance, as these smaller scales add to the variance. However, in the construction of composites, finer spatial details are lost due to the large number of events involved in the averaging process. Therefore, we believe the use of higher-resolution data proves most beneficial when looking at individual case studies.

- 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?

We agree that SLP variance is not the best choice in this context and we modified it accordingly (see Fig. AR1), thanks for pointing this out.

It is interesting to note that the peak flux standard deviation has a small bias towards the southern side of the SST front confirming a previous point by the Reviewer. This pattern is

completely consistent with the mechanism of cold sector being advected in the SE direction over warmer (and spatially variable) SSTs.

- 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).

What is enhanced is the spatial variance of surface heat fluxes which is then followed in time by an increase in temperature spatial variance (we do not imply a causal link, which would act opposite). When the cold front moves across the spatial domain, the temperature spatial variance does not change significantly, while the surface heat fluxes pick up spatial variance when the cold sector moves over warmer SSTs. This is exactly the type of processes that our diagnostics highlight. Note also that this process is consistent with the new Figure 1 (Fig. AR1 in this response).

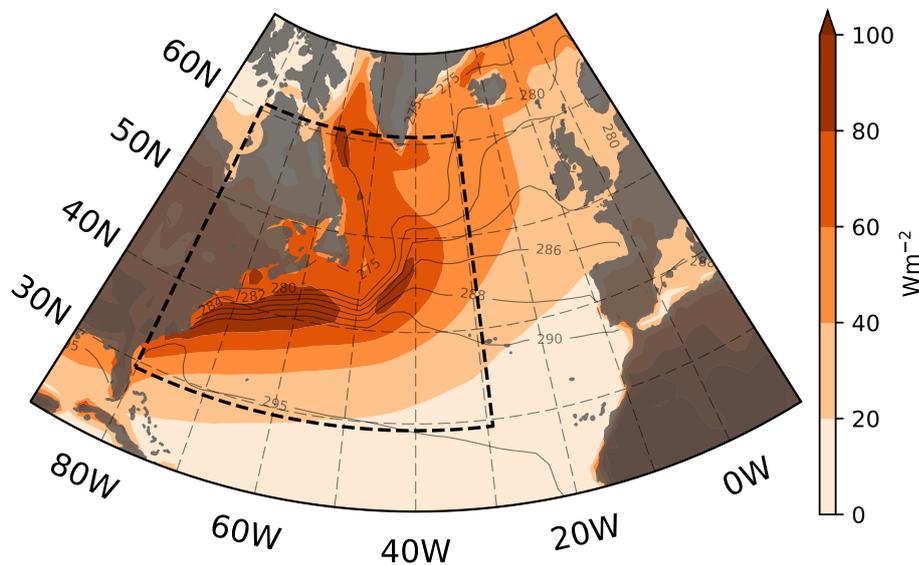


Figure AR1: Update of Figure 1 from manuscript; shading represents temporal standard deviation of F , contours represent SST winter climatology (every 2K from 280K to 290K, every 5K otherwise).

- Can you show a . 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?

The time average of either $[F^*T^*]$ or $[F'T']$, assuming the brackets to be indicating a spatial average operator, would correspond to a negative number rather than a field. The time average of F^*T^* would instead provide a picture of where the spatial covariance of heat flux and temperature is realised within the spatial domain we considered. This is found to peak along the Gulf Stream, where time variances of the fluxes are also larger (see Fig. AR2 - and compare to Fig. AR1).

Thank you for pointing out this useful diagnostic; we will describe this property in the revision.

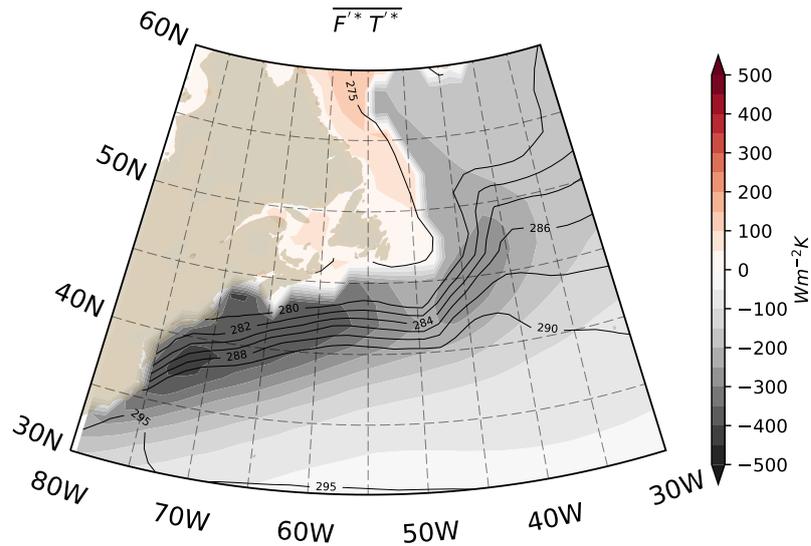


Figure AR2: Wintertime (DJF, 1979-2019) mean (shading) of product between time-space anomalies in flux and temperature over the spatial domain selected for our study. Black contours represent wintertime SST climatology (every 2K from 280K to 290K, every 5K otherwise).

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.

We replied to this earlier and we agree that it will be a valuable addition to the manuscript to discuss the relation between these arguments and ours. As indicated before, we do not think they are contradictory at all; rather, they are complementary and really speak of different properties of the storm track.

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.

Thank you for pointing this out. That was indeed poorly phrased and will be changed. What we of course meant to say is that local diabatic heating and temperature anomaly fields need to be positively correlated for diabatic heating to maintain a circulation against dissipation.

- 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).

We are aware that the link we made between baroclinicity and available potential energy is informal and that direct expressions for local APE have been devised (Novak and Tailleux, 2017). As pointed out in an earlier response, we will put more effort in rephrasing clearly our intentions and the reasons why we have been using this particular framework.

4) I have some trouble to understand how you relate your 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.

Composites shown on the left in Figure 3 are relative to the peak in the covariance and we did not include lagged composites for the sake of conciseness. Perhaps it is useful to add composites at negative lags to illustrate more clearly how baroclinicity varies, as its depletion at larger FT covariance was indicated by phase tendencies presented in a later section.

In Fig. AR5 in this document it can be seen that the near surface temperature (T2m) at the peak does not particularly appear to coincide with an enhanced N—S temperature gradient. Furthermore, we produced a new Figure 3 (Fig. AR8 in this document) where it can be seen that the overall N—S gradient in T850 does not enhance at the peak of the index although local T850 gradients do appear to be somewhat enhanced, as suggested by the Reviewer. We will discuss this in the revision.

See also our response below.

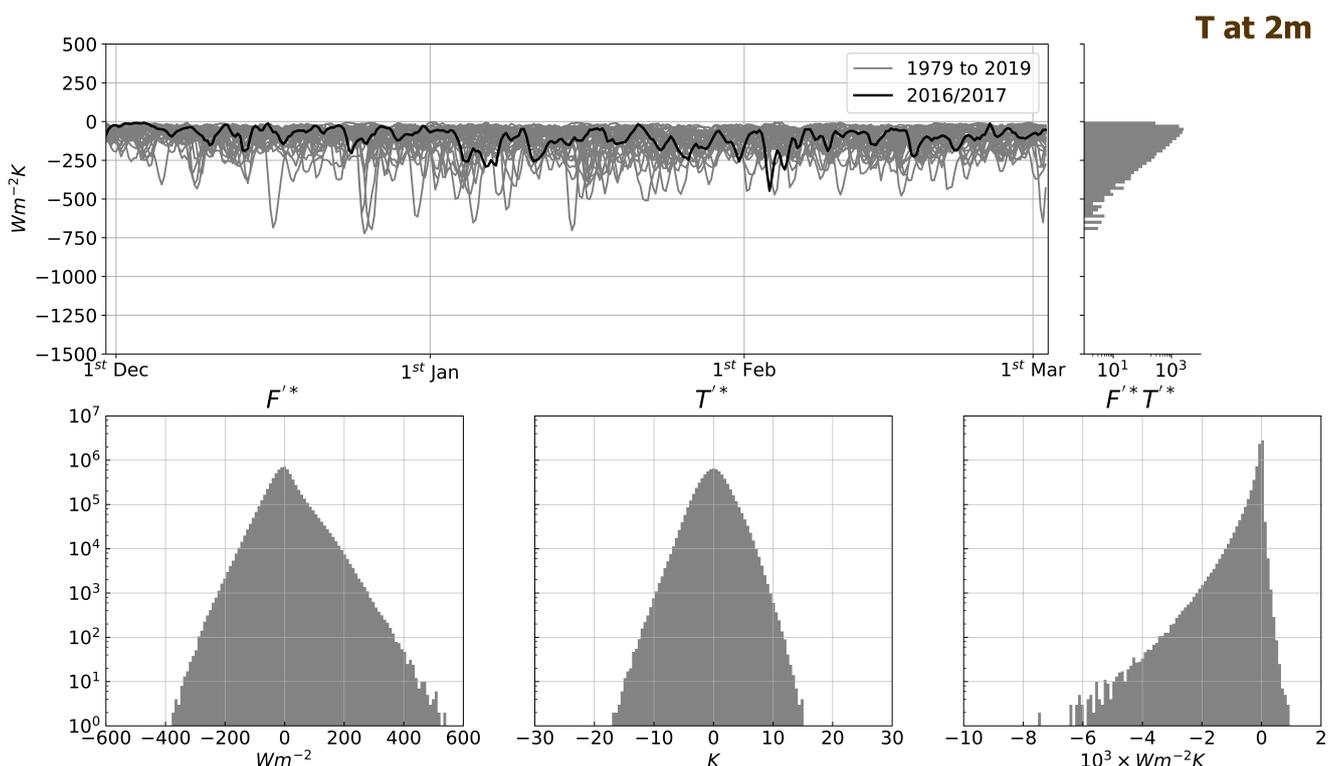


Figure AR3: As in Figure 2 of the manuscript using T2m as T.

- 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.

Figure 4 in the manuscript really explains what we mean here: the baroclinicity is efficiently depleted following a peak in FT covariance. This is indicative of the two-way interaction between the two fields. We also see that when the FT covariance is weak, the baroclinicity can build up again. This is consistent with, for example, the set of papers of Nakamura et al., in that locally baroclinicity is produced over these regions, and it is consistent with the view that eddy activity locally (in time and space) depletes the temperature gradient.

The link between baroclinicity and APE has been investigated in previous studies such as Ambaum and Novak (2014). They also suggested the link between baroclinicity and local contributions to the APE in their study of storm tracks dynamics.

We realise that our discussion of these aspects should be clearer, and that we in particular did not include the relation to the climatological link between the SST front and the storm track. In the revised manuscript we will highlight those.

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.

Surface air temperature is directly involved in the computation of surface heat fluxes and this would just emphasise their strong interlink. Temperature at 850hPa, as the reviewer pointed out in a previous comment, is not directly involved in the computation of surface heat fluxes and therefore its covariation with surface heat fluxes is not trivial and entails more information

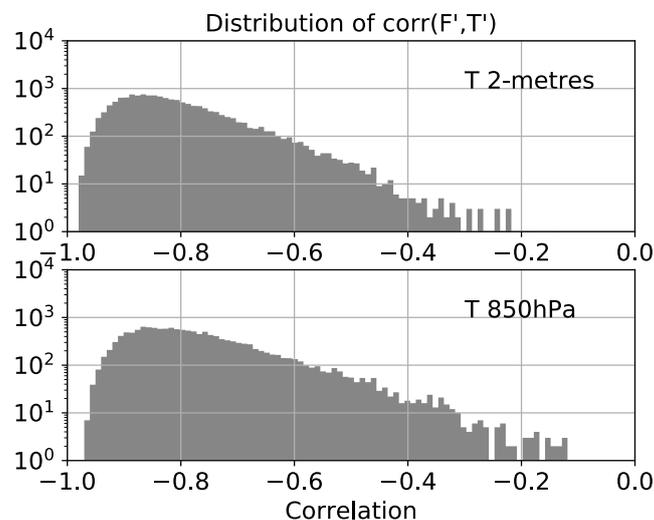


Figure AR4: empirical distributions of spatial correlation between F and temperature at the surface (top) and at the 850hPa level (bottom).

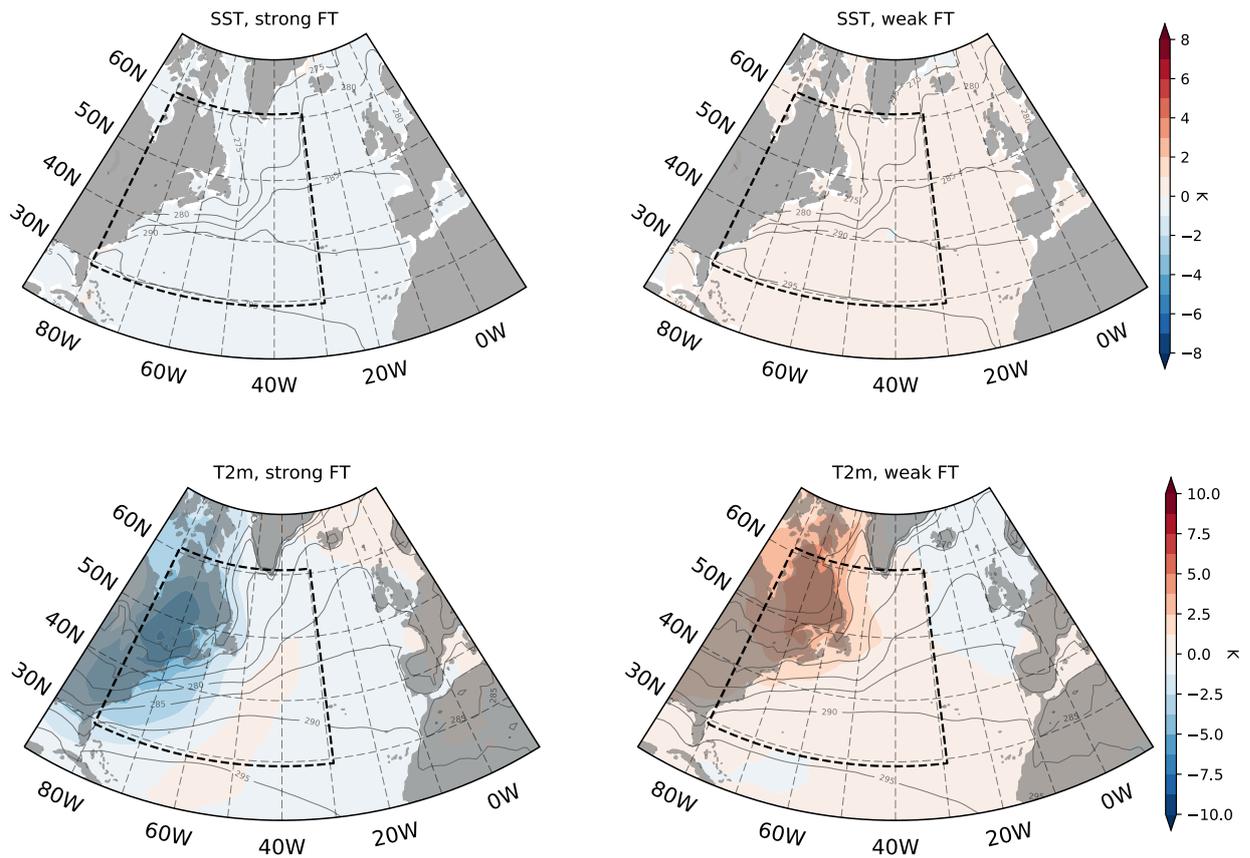


Figure AR5: As in Figure 3 for SST (top) and T2m (bottom) with contours representing the full composite and shading its deviation from climatology.

about synoptic developments, unlike temperature at the surface. The distributions using T at 2-metres are shown in Fig. AR3.

Covariances appear to be weaker when considering T2m which is due to the higher temperature variance at the 850hPa level. Indeed, the distribution for correlation between F and T2m is slightly shifted towards stronger values, while correlation between F and T850hPa features a longer tail towards weak values (see Fig. AR4).

The composites for SSTs (Fig. AR5, top) do not provide further insight into the coupling of the lower troposphere to the ocean surface on synoptic timescales, as these are much shorter than the typical time variability of SSTs.

Composites for T2m are remarkably similar to those for T850 (Fig. AR5, bottom), with slightly weaker anomalies' values (see Fig. AR7) and we will discuss this property in the revision.

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?

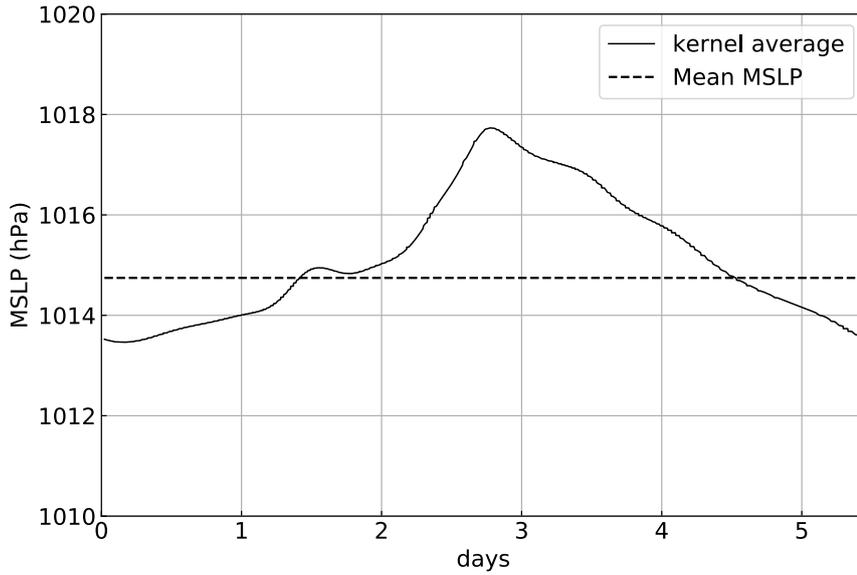


Figure AR6: Time evolution of kernel-averaged MSLP (solid line) and its climatological value (dotted line).

The time evolution of the average MSLP over the domain selected features a peak around day 3 of the path selected in Figure 8a, that is when FT spatial covariance is largest (see Fig. AR6).

The time evolution of the wind direction is shown in Fig. AR7 and is found to be consistent with cold air advection in the first half of the cycle and warm air advection in the second half.

We intend to add a discussion of these results in the revision and thank the Reviewer for suggesting this.

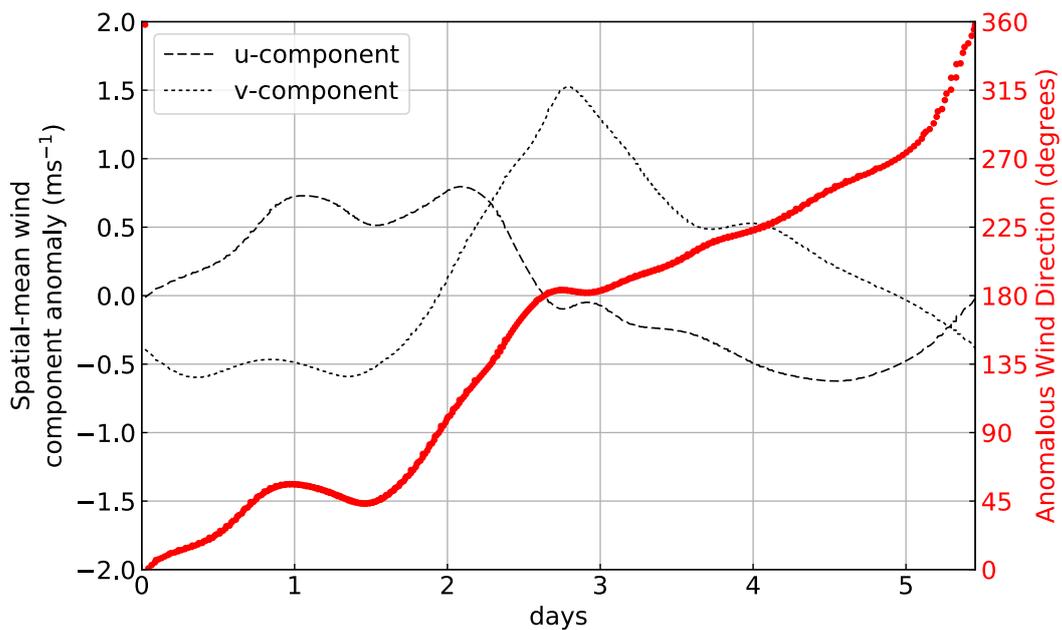


Figure AR7: Time evolution of meridional (dotted), zonal (dashed) anomalous wind components and corresponding anomalous wind direction (red dots).

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

We apologise for this, it seems that the trouble is with Figure 6. The manuscript we submitted was only 8MB but printing it out would still give problems, as we just found out. We will make sure the reviewed manuscript prints out effortlessly.

— *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)*

We will adjust figures according to both reviewers' suggestions.

Author response to Reviewer #2

1) The authors define the FT index by the spatial covariance between 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

We acknowledge that the link we made between the APE budget and flux-temperature covariance is quite informal. Our intention was to capture the local air–sea heat exchange on synoptic time scales and elucidate their role in the evolution of a weather system. The seasonal progression of the meridional gradient would dominate over synoptic spatial variance, hence why we first remove a 10-day rolling average and then calculate the spatial anomalies. To that extent, we were not trying to obtain an expression for one of the various forms of APE.

I think the reviewer will also agree that there is no uniquely optimal choice of mean state or anomaly used to define APE, and no such thing as “the APE” exists. This is another reason why we used a more informal approach to defining a dynamically relevant thermodynamic index. Our index describes whether the air–sea heat fluxes locally act to increase or decrease synoptic variance in the lower-tropospheric temperature.

We are rephrasing the relevant passage as it also elicited questions from Reviewer 1 and it is then clear that our informal link to the APE budget was not made clear enough.

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.

We agree with the reviewer that showing the full fields as well as the mixed time-space anomalies could be helpful in understanding the role of flux-temperature spatial covariance in the evolution of a weather system. In fact, we did inspect these fields in more detail but decided to err on the side of not showing every possible field for the sake of brevity.

Nonetheless, we have decided to replot Figure 3 (see Fig. AR8) in order to include the full fields (contours) beside the anomalies (already shown as colour shading). We will use this figure in a revised manuscript, and include it here for inspection ahead of the revision.

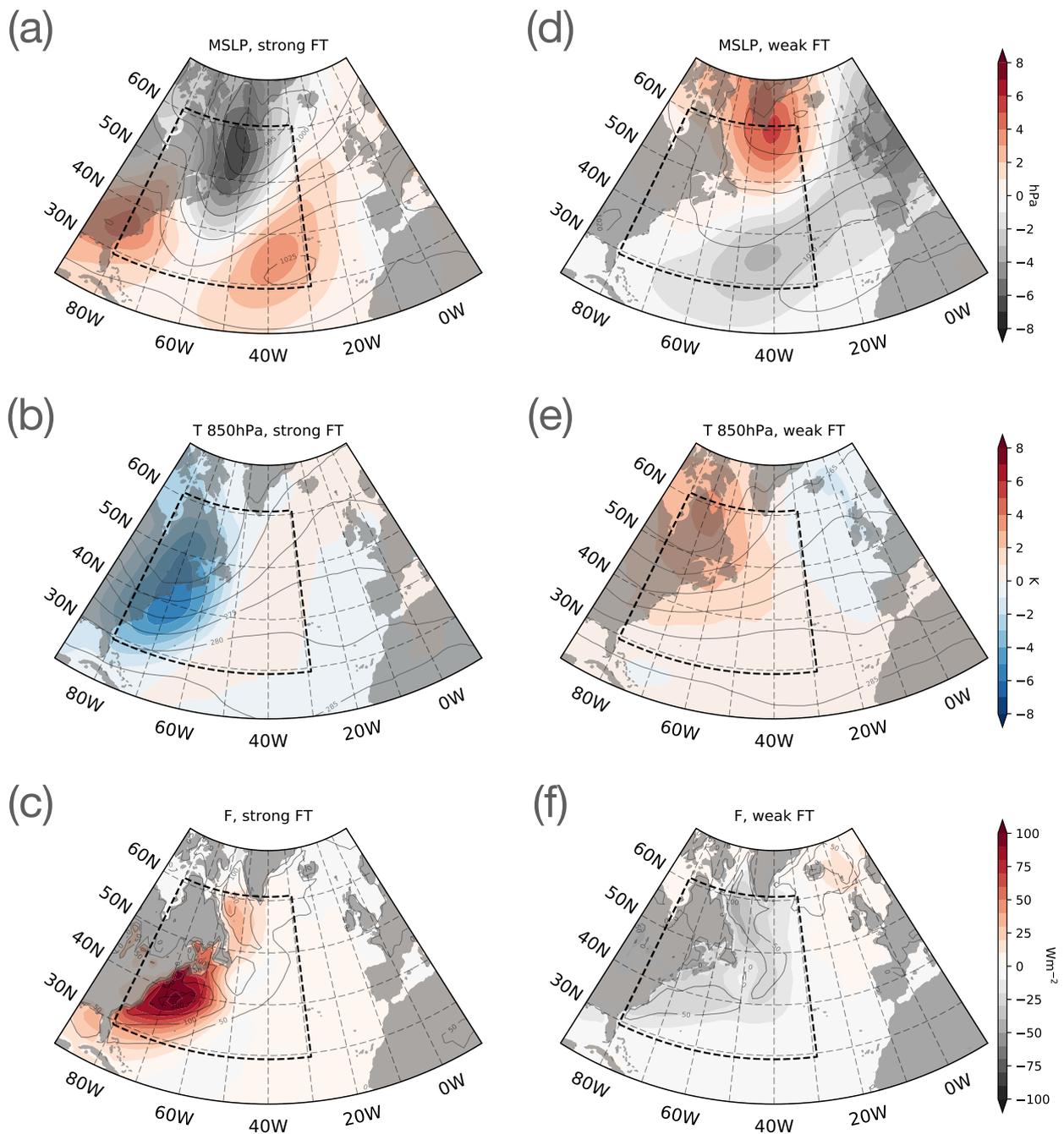


Figure AR8: contours indicate actual composite fields every 5 hPa, 5 K and 50Wm⁻², respectively for strong (a-c) and weak (d-f) FT index values.

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?

Perhaps the term ‘lead’ in our description of the FT index dynamics can be misinterpreted. We literally meant “leading in time”, but did not want to imply any causal link. What we observed is that the increase in T’ spatial variance was followed by that in F’, which is the opposite of what would be expected if you interpreted F’ as a source of T’. But our work actually shows that the correlation is negative, indicating that T’ typically determines F’ (cold T’ leads to a positive F’ and v.v.) —with that causal link we might expect the T’ variance to lead the F’ variance but we actually find the opposite to be true, hence “counter-intuitive”.

We then surmised that this effect could be caused by the advection of cold air with a more spatially uniform temperature pattern over the Gulf Stream extension region, which features a much more variable temperature spatial field. The effect of surface heat fluxes would be that of eroding the spatial temperature variance in a weather system by damping the cold sector temperature anomaly, while the warm sector is less affected by this coupling with the surface.

Again, in the interest of compactness we decided to not show every possible diagnostic. We did select two relevant diagnostics in the paper which, as suggested by the reviewer, shows certain properties following the phase-space trajectory, namely the depth of the boundary layer (Figure 8) and the cold-sector area fraction (Figure 9). Figure 3 does not really show trajectories for synoptic fields but related composites, so they indicate the synoptic evolution. Nonetheless, kernel averages for strong and weak spatial standard deviations are able to reproduce the same spatial structures that are found by compositing on extreme values, as represented in Fig. AR9.

We are grateful for the reviewer for highlighting this aspect of our work, and in the revised version will include and reemphasise the above summary of the situation.

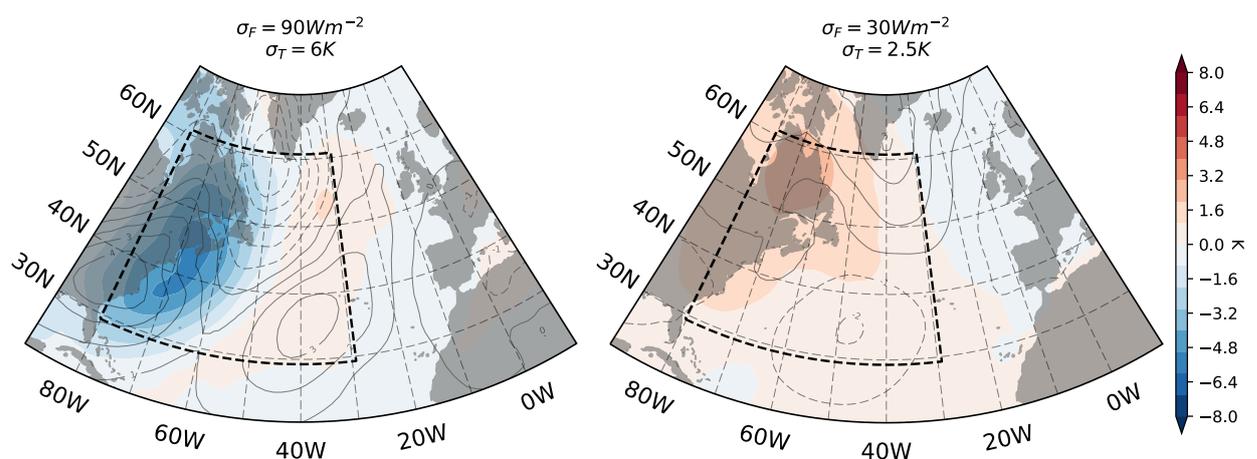


Figure AR9: kernel average of MSLP and T for strong (left) and weak (right) values of the spatial standard deviations in F’ and T’ (indicated at the top). Contours (every 1hPa, negative values dashed) and shading represent difference between kernel averages of MSLP and T and their winter climatologies, respectively.

Note also that in response to another Reviewer's query, we produced diagnostics of the wind direction and pressure following the phase space trajectory, analogous to Figure 8b in the manuscript (see Fig. AR7 in this document). As can be seen, the move towards the index peak (around "day 3") corresponds to a shift of the prevailing wind anomaly from initially N, N-W to W and then S at the peak, indicating an initial inflow from the cold sector into the domain going along with an enhancement of the system and the frontal winds.

*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*

We certainly considered this interpretation of the tilt but initially thought that the tilt appeared insufficient to be consistent with such a baroclinic life cycle picture. We have now reconsidered some existing diagnostics, particularly from Lim & Wallace (1991), where a weak forward tilt of temperature is also diagnosed at lower levels, as it must be for growing waves (Hoskins & Heckley, 1981), but where it is also substantially less than the westward tilt of geopotential. Of course, the magnitude of the tilt is hard to compare to our results as the x-axis in our Figure 8 maps onto time in a non-trivial way.

The stronger tilt/lag of temperature at upper levels that we find is *not* consistent with observations or expected from theory of idealised life cycles, where in the lower stratosphere at least, the tilt/lag is expected to reverse, as suggested in the quoted studies.

Of course, as is the case with such diagnostic studies, it is not always obvious we are looking at growing, mature or decaying systems, even though we would expect dominance by the growing systems in the chosen geographical area.

So the question on whether the tilt is dynamically important remains open, and we are in fact currently working on a related problem. Our preliminary analysis shows that there may be contributions of non-normal growth as well as normal growth present in such statistics; of course the tilt of non-normal growth is not fixed and may potentially explain why the observed tilt in Figure 8 is so weak. This is however speculation at this point, and a topic in our current research.

Having reconsidered this issue following the Reviewer's suggestion, we conclude that this interpretation and discussion of the observed tilt is broadly consistent with our view of the synoptic development driving this index evolution, and we will want to discuss the above in the revision.

In particular, the temperature variance could well have been dominated by the cross-frontal temperature contrast being advected into the analysis area, while the flux variance is dominated by the cold-sector following the front. But if that were all there was to it, we would

probably expect temperature variance to lead the flux variance, while we find the opposite to be true.

So it appears that the cold sector induced variance of fluxes shows up before the frontal temperature anomalies reach their maximum amplitude.

It is clear from the Reviewer's comment on this that this would be a valuable addition to the discussion and in the revised manuscript we will want to discuss this interpretation.

5) Lines 251-254: Increasing F' followed by decreasing baroclinicity does not really imply that baroclinicity is depleted by the air-sea exchange. Baroclinicity 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.

The reviewer is of course right to stress the care that should be taken when inferring causality and we will emphasise that more in a revised manuscript.

Nonetheless, we believe from our analysis that there is strong evidence that the air—sea fluxes locally in time and space damp the synoptic temperature variance. From that point of view the interpretation that the FT index is a measure of both the eddy amplitude and how the air—sea fluxes might erode the eddy growth rate (baroclinicity) remains circumstantial but strong evidence.

We will caveat our discussion around Figure 4 to stress these important issues of interpretation.

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

We are very thankful to the reviewer for highlighting the important links with these references. To our defence, we can only say that our route into this work came from a different direction—which we agree is a poor excuse! It is clear we will want to discuss the relevant links in a revised manuscript.

In particular, the importance of lower tropospheric thermal adjustment on short timescales to the underlying sea surface is something our work supports, and also supports the work that Reviewer 1 pointed out about the strong anchoring of the climatological storm-track to the ocean temperature front. In fact the inability to erode lower tropospheric temperature variance by synoptic eddies in the presence of strong SST variance is exactly consistent with our view that spatial variance in the SST is the cause of high flux variance which locally adjusts the lower tropospheric temperature by dragging it towards the SST. This happens at synoptic timescales.

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.*

As indicated in our above answer, we are grateful to the Reviewer for highlighting this important link. We will revise that passage in order to take into account the study by Chang et al (2002) where they presented the contribution of the different components of diabatic heating to eddy APE. Their results are based on a dataset consisting of Januarys from 1980 to 1993 while we use the whole winter seasons from 1979 to 2019, though perhaps there are more recent studies with larger datasets that we are not aware of.

We did do many of our analyses using sensible heat fluxes as well as latent heat fluxes and total heat flux, and although the diagnostic results obviously differed in detail, and in magnitude, the structure of our main diagnostic results was quite insensitive to such choices. We did not endeavour and tease out the differences there were, as that would really constitute a different study. We decided to concentrate on sensible heat fluxes precisely because of the type of results presented in Chang et al., or Swanson & Pierrehumbert, as well as Hotta & Nakamura (2010), namely that sensible heat flux has a strong local effect of relaxing the lower troposphere towards the underlying sea surface.

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?*

Although the spatial resolution we chose or even the finest available in ERA-Interim would not allow for oceanic eddies to be fully resolved, their effect on surface heat flux at the resolved scales would be captured by the reanalysis system which means there would be still some residual variance that can be revealed by our analysis, although obviously not all of it.

We are grateful for the Reviewer to point this out, as did Reviewer 1. We need to more acknowledge the potential role the overall N-S SST gradient could play in forcing variance in the fluxes; this contribution is obviously less sensitive the the underlying data resolution. Fig. AR1 in this document indicates how most of the flux variance (at least in time) is realised of the warmer part of the gulf stream front. This figure will replace the corresponding figure in the manuscript and we will duly discuss its properties and consequences.

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.*

We apologise to the Reviewer and agree to remake the plots to improve readability.

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)?*

That was badly phrased - apologies. Due to the kernel smoothing that was applied, in the phase plot it may seem that the index changes sign at times. However, the FT index is never positive, as stated earlier and also shown in Figure 2. We will make it clearer in the revised manuscript.

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.

Indeed. See also our response above.