Interactive comment on "Impact of the current feedback on kinetic energy over the North-East Atlantic from a coupled ocean/atmospheric boundary layer model" by Théo Brivoal et al.

Many thanks for your interest and your comments on the paper. We respond to your main points:

1 – *"The paper does little to pick apart how the ABL model is producing these effects, whether the SST effects are playing a role, and whether the ABL model is capturing these SST effects».*

In our experimental setup, the tracers (i.e : the potential air temperature and the humidity) are fully nudged towards ERA-Interim by using a relaxation time on the tracers equal to one model time step. This means that with this setup, the tracers cannot react to the ocean surface. As a result:

- The SST / wind or SST / stress coupling coefficients (Chelton et al., 2007; O'Neill et al., 2012) are ~0 when the tracers are fully nudged (Figure R1). This means that the thermal feedbacks between the SST and the atmosphere are negligible in our simulations.
- The ocean surface can only alter ABL1D winds through changes in the surface stress.
- Since the SST / stress coupling coefficients are negligible with this setup, surface stress changes are mostly related to ocean currents.

Moreover, and this is now mentioned in the new version of the paper, the dynamical coupling coefficients ($S\tau$ and Sw) are almost unsensitive to the relaxation imposed to the tracers (when the tracers are not fully nudged towards ERA-Interim, Figure R2). This suggests that the currents mostly interact with the winds by changing the surface stress, and therefore the vertical wind shear.

However, we agree with you that all these points were probably not clear enough in the previous version of the paper. We added more clarifications about this in the new version:

- In sec. 2.1 : "With this setup, ABL1D winds are only impacted by the ocean surface through the surface stress (and not by the turbulent heat fluxes). Therefore, this setup allows us to efficiently isolate the effect of the current feedback from other coupling processes. Note that this is different from Lemarié et al. (2020) where the tracers are modified by the ABL1D."
- In sec 3.2 : «As mentioned in sec 2.1, the tracers in ABL REL or ABL ABS are not impacted by the ocean's surface since they are fully nudged towards ERA-Interim. This means that in ABL REL and ABL ABS, the ocean surface can only have an impact on winds through changes in surface stress. Moreover, SST / wind coupling coefficients (Chelton et al., 2007; O'Neill et al., 2012) are near 0 in all simulations (not shown) and thus indicates that the SST / wind or SST / stress coupling is not present. Therefore, differences between $S\tau$ of ABL_REL and FRC_REL are not attributable to the impact of SST on winds. We also find that $S\tau$ and Sw are almost unsensitive to the relaxation imposed to the tracers (not shown). This suggests that the vertical wind shear adjustment to surface stress is the main driver of CFB_tot since it is the only mechanism that could explain the positive Sw found in ABL REL. »

This also respond to the question: « Is the difference between the two curves entirely due to a damping of the current feedback by boundary layer dynamics, or is the SST effect also playing a role? ». Since

the SST / wind feedbacks is not present in our simulations, the KE differences between ABL_REL and ABL_ABS are only due to the current feedback.
However, differences between FRC_ABS and ABL_ABS are also related to differences in background winds. We also added clarifications on this point in the paper in sec 3.3 : « KE_g in ABL ABS and FRC ABS are in the same order of magnitude (Fig 5.a), differences between the two simulations are only attributable to the changes in background winds between the two simulations as the SST feedback to the atmosphere is not present in both simulations. »

2 – *« What is really needed is to be clear about what is being added and elucidated here, and what is reiteration of established results. There are some useful new results, and a little more work would bring more to light. Some reorganisation and better signposting to the reader of the most significant results would be very helpful, as would some additional diagnostics and discussion. »*

We reorganised and added discussions in the paper to clarify what is new from our study and what was already known.
Major changes :

- Fig 5 and 6 : Total KE is used rather than just geostrophic KE only to be consistent with the rest of the paper.
- We reorganised section 3: 3.3.1 becomes section 3.3 : *Impact on kinetic energy* and section 3.3.2 becomes section 3.4 : *Current feedback impact on kinetic energy budget over the water column*
- Figure 8 is now Figure 7, and now represents only the impact of the current feedback on KE, since the vertical profile of trends did not provide new information. We also added a vertical profile of KE differences between ABL REL and ABL ABS in % of ABL ABS.
- Discussions added in sections:
    - Introduction : clarifications on “why we chose a region of low mesoscale activity”
    - 3.1 : discussion about the differences between ERAi, ASCAT and ABL1D winds and what can cause these differences.
    - 3.2 : discussions about how the ABL1D model is producing the wind response to the surface currents, and about the values of the slope between $S\tau$ and the background winds.
    - 3.3 : discussions added on why the KE response is quasi-homogeneous despite its inhomogeneous mean state and more, and on how the ABL1D is producing the KE partial-reenergisation. We also added discussion about the KE differences between ABL REL and ABL ABS at depth: to our knowledge, this is the first time a study shows that the current feedback can have an impact on the ocean at such depth (1500m). We also added signposting to introduce part 3.4.
    - 3.4 : Clarifications about Ekman pumping mechanism added
    - Conclusion & abstract: we clarified what is new in our study.

- Clarifications about the nudging of the tracers have been added in section 2.1.

*3 – « Renault et al. (2017) derive a theoretical relationship for this coupling coefficient C2 ignoring feedbacks: S_tau = -1.5rho.Cd*Wind, which is -2.2e-3*Wind for their Cd=1.2e3 and rho=1.225. The slope (if not the intercept!) of their relationship is quite close to what they find in observations. The slope here is twice as steep in the matching FRC REL case. Why could that be? What value of Cd is used in these simulations (if Cd depends on winds, then what is the range over these wind speeds) »*

This is an interesting point. The slope we found for ABL_REL (-2.3e-3*Wind) is quite close to the slope they found in observations (-2.5e-3*Wind) and from the slope found in Jullien et al. (2020) from a fully coupled ocean-atmosphere model (-2.3e-3*Wind). However, as you mentioned the analytical relationship derived in Renault et al. (2017) should be representative of the relationship found in FRC_REL since feedbacks are ignored in the analytical formulation. If we compute the slope from their relation from the Cd in FRC_REL and assuming rho=1.225, we found a slope of -2.7^{e}-3*Wind which is still quite far from the slope in FRC_REL. The reasons for such a discrepancy are not clear, this might be related to the filtering technique or to the approximations made in the Renault et al (2017) formulation but should definely be addressed in further studies. Nonetheless, since we use a similar filtering technique as in Renault et al. (2017) or Jullien et al. (2020), the slope found in ABL_REL can be compared to the slope computed from observations in Renault et al. (2017) or coupled models in Jullien et al. (2020).
We added a discussion for this point in the paper in sec 3.2.

4 – Minor technical issues have been adressed in the new version of the paper.

- *« Lines 103-118 - this is rather a confusing description. It seems to say that geostrophic winds are derived from MSLP, which is calculated as a combination of u,v,theta,q and MSLP. The description in Lemarie et al. seems clearer, and doesn't have a geostrophic U in their version of Eq. 1, but R_LS - a geostrophic plus relaxation term. Presumably that is where the other variables come in? And it would be helpful to specify whether this term is independent of height. »*

When the geostrophic winds are used, there is no relaxation done on the dynamics since the model is guided by the large-scale pressure gradients (fk.Ugeo). However, this could be the case when the equatorial region is considered. We added clarifications in the paper.

- *« Line 226 - I couldn't see why the conversion to equivalent neutral winds was done C4 here, why not stick with stresses? »*

ASCAT scatterometers directly measure equivalent neutral winds (ENW), so it was cleaner to directly compare ENW rather than stresses.

- *Eq. 10 - if zeta is to be accounted for here, it should be inside the curl operator (as, strictly, should f) - the right hand side should be (1/rho)curl(tau/[f+zeta]).*

We followed Gaube et al. 2015, in which Ekman pumping was computed this way. However, it does not change much the results if [f+zeta] are accounted into the curl operator.

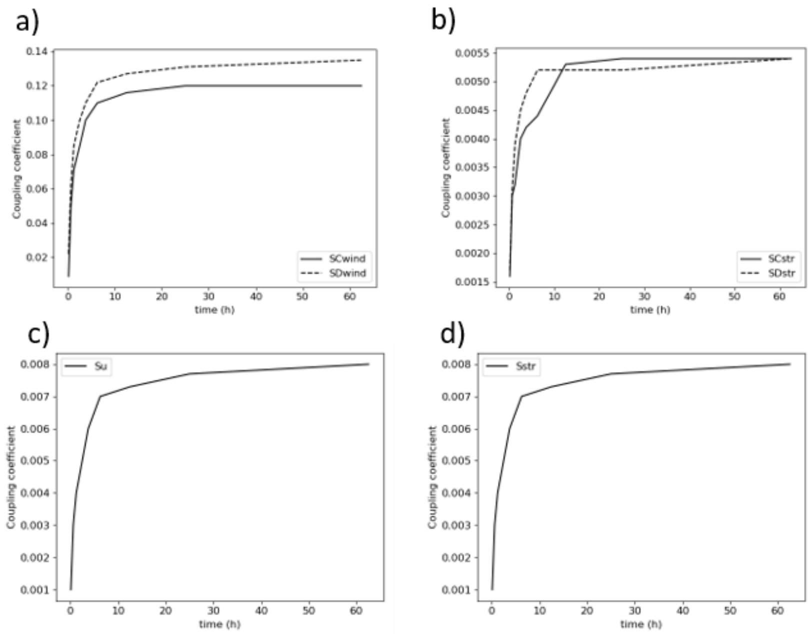

Figure R1 : Mean thermal coupling coefficients (averaged over the IBI area) computed from 1/12° NEMO / ABL1D simulations performed over the year 2017 against relaxation time imposed on the tracers in ABL1D model for a) SCwind = crosswind SST gradient anomaly vs the wind curl anomaly, SDwind = Downwind SST gradient anomaly vs wind speed divergence anomaly, b) SCwind = crosswind SST gradient anomaly vs the stress curl anomaly, SDwind = Downwind SST gradient anomaly vs stress speed divergence anomaly, c) Su = SST anomaly vs wind speed module anomaly and d) Sstr = SST anomaly vs wind stress anomaly.

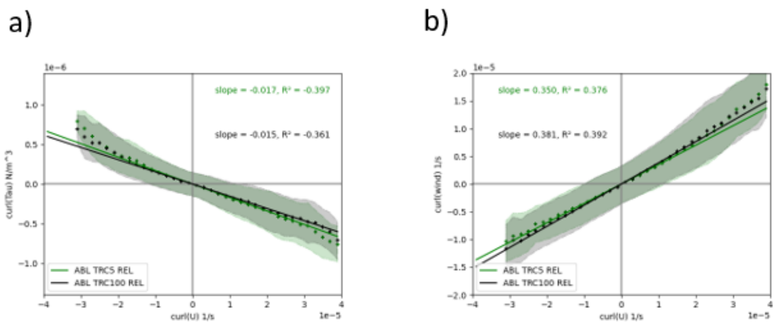

Figure R2 : binned scatterplots between a) wind stress curl anomaly and the current curl anomaly and b) wind curl anomaly and the current curl anomaly for 2 simulations of one year (2017) at 1/12° resolution over the IBI area : ABL TRC5 REL (black) using relative winds and a (weak) relaxation of 5% (2,5h) on the tracers and ABL TRC100 REL (black) using relative winds and a relaxation of 100% (1 time step) on the tracers (as it is in the paper). The slope of a) corresponds to the Sτ coupling coefficient and b) Sw.