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Discussions

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.

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The authors have performed a very interesting set of experiments which have the potential to elucidate both the performance of a 1D atmospheric boundary layer model and the mechanisms by which the ocean mesoscale influences atmosphere-ocean momentum fluxes. There are (at least) four different issues involved here: 1) Accounting for ocean currents reduces the energy input to the ocean (many previous studies) 2) Allowing an atmospheric response to the ocean mesoscale reduces this effect (the Renault and Jullien papers cited by the author) 3) Sea surface temperature also influences the wind stresses (e.g. O'Neill et al.). 4) There are two mechanisms for the

SST effect. (Incidentally, one thing I wasn't sure about is whether the ABL model is expected to be able to simulate both SST effects. It would be nice to have a mention of this somewhere.)

The diagnostics presented seem to confirm that effects 1) and 2) occur in this model, as well as showing that the resulting changes in mesoscale energy result from changes in the pressure work in the model. This is quite nice in that it demonstrates that the ABL model can reproduce the mitigating effect 2), seen in more complete atmospheric models. However, 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. That is a pity given the experimental design. In fact the (now published) Lemarie et al. paper describing the 1D model does more along these lines, showing that positive correlations between winds and mesoscale SST arise in the more energetic midlatitude regions.

Overall, I feel there is a lack of focus to the paper. It presents a range of diagnostics, but often doesn't give the context or make clear what is being learnt from them. 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.

Of the latter, I would particularly like to see an expansion of the coupling coefficient data shown in Figure 3, and more discussion of the relationships found. These are summarised as "consistent with the values in the literature", but there is a lot more to be said than that. For example: 1) 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? The latter could be judged by plotting the results for ABL ABS to see whether the SST effect alone makes a difference (and FRC ABS for completeness) 2) Renault et al. (2017) derive a theoretical relationship for this coupling coefficient ignoring feedbacks: S_tau = -1.5rho.Cd*Wind, which is -2.2e-3*Wind for their Cd=1.2e-3 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)? 3) How about coupling constants for SST? O'Neill et al. calculated these from observations, so how does this model compare with those data? You have the data here to calculate these coefficients both with and without the current feedback, to see how these interact.

Similarly, when it comes to Figure 5, there seems to be no discussion of 5a, and all the focus is on the REL-ABS differences. While this is interesting in that it illustrates the mitigation of this KE reduction when the ABL is used, it again doesn't address the SST effect which should be shown by the difference between ABL ABS and FRC ABS. While this is not as consistent as the ABL REL minus FRC REL difference, it is usually the same sign and comparable in size, suggesting that the SST effect is playing a role in the mitigation of energy reduction, it isn't just the ABL response to ocean currents.

One final general point - I felt rather saturated in TLAs (Three Letter Acronyms) as I read the paper. I appreciate that it would get rather long-winded to write everything out in full, but using the full terms sometimes would offer some respite to the reader.

There are a number of more technical minor issues that I would like to see addressed, listed below, but the main thing that would greatly strengthen the paper is a clear dissection what is being learnt about the different effects discussed above, giving a better focus in the purpose of the paper, particularly in the context of what has already been presented by Lemarie et al.

Minor Issues:

1) Line 20 "induces"

2) Line 21-22 "and this" acts? "over the whole water column"

3) Line 42 "sets up" -> is set up

4) Line 44 "consider" -> considering

5) Line 48 (or perhaps earlier) - wind-work here refers to the work on the geostrophic part of the flow. The total work on the ocean is much larger, but much of it is dissipated immediately in the surface mixed layer. Somewhere this distinction should be made clear.

6) Line 55 - eddies -> eddies'

7) Section 2 - begin by explaining that the (now published) Lemarie et al. paper gives the complete description and first validation results for this model.

8) Line 82 - "models" -> model

9) 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.

10) Lines 134-135 "drag coefficient-induced SST changes" - do you mean SST-induced drag coefficient changes? SST doesn't change in response to changes in drag coefficient, but the drag coefficient does change in response to SST.

11) Line 139 - "total kinetic energy" per unit mass

12) Lines 139 to 141 - Cs, Cm and C1 occur in the equations, but only Cm is given a value (4). Is Ce supposed to be Cm? And is C1 something else?

13) Line 210 "were" -> was

14) Line 226 - I couldn't see why the conversion to equivalent neutral winds was done

here, why not stick with stresses?

15) Line 228 "1,22" -> 1.22

16) Line 252 - An assumption is made here...

17) Line 256 - The ability of ABL1D to simulate...

18) Line 263 - low-pass, not high

19) Line 263-265 - This is not clear. Is sigma 1 degree? In latitude and longitude, or great circle? Is sigma the standard deviation of the Gaussian used? What is meant by the 1 in 6sigma+1, 1 degree?

20) Table 2 - give units for S_tau.

21) Section 3.3.2 - There's also the d(KE)/dt term, which is easily calculated as H*[KE(end)-KE(start)]/time where H is layer thickness. This comes out at around 0.04 for the 300 m surface layer, and about 0.2 for the 1700 m deeper layer using values from Fig. 5a (it is unclear what these values are - domain averaged? over all depths?), so there's no need to speculate about longer spinup period at depth, the values can simply be added to the budget if diagnosed over the relevant volumes. There also seem to be sign errors in Eq. 9. The middle terms of the 2nd and 3rd lines, and the final term of the 4th should be the opposite sign.

22) Lines 360-360 - I don't understand what this is showing. If STR is the energy input by wind stress at the surface it should be zero everywhere below the surface. It seems to be balanced by the vertical viscosity term - I think explicit formulae for the terms being shown are needed rather than just these verbal descriptions.

23) 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]).

24) Lines 370-380: This needs to be clarified. Upward Ekman pumping is associated with anticyclones (sea level high, and positive, not negative pressure anomaly). This

represents damping because these baroclinic eddies are a sea level high balanced by a dip in the thermocline. The Ekman pumping damps out this dip in the thermocline. Alternatively, the Ekman flux divergence is a flow down the horizontal pressure gradient, and thus a loss of energy from the eddy.

25) Lines 397-8 - as discussed above, this point needs much more discussion and quantification.

26) Line 404 - "switches" -> acts

27) Line 410 - "and this" occurs "over the whole water column".

28 "Code and data availability" "Go" and "To" -> Gb and Tb.

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