



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.

Anonymous Referee #2

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The authors ran four numerical experiments on the effect of ocean current feedback; with and without the one-dimensional atmospheric boundary layer model; using atmospheric winds absolute and relative to ocean currents. The OGCM was sub-mesoscale resolving (1/36 deg). In terms of wind speed, the effect of ocean current was small (< 10%), but discernible in coupling coefficients. The resultant coupling coefficients are consistent with previous results. The effect on ocean currents is to reduce the kinetic energy, which was significant. The effect can be seen below 1500 m depth, which is explained by a change in the Ekman pumping.

The aims of the present work was twofold (L.90) – to validate the atmospheric boundary

layer model and to quantify the ocean current feedback effects. I found problems for both.

For validation of the ABL1D model, it was not clear to me what was the true value. The ASCAT product has an error of 1 to 2 m/s relative to buoy wind speed (Fig.11, Bentamy and Fillon, 2012, <https://doi.org/10.1080/01431161.2011.600348>). This is of a similar magnitude to the RMS difference between ASCAT and ABL (L.234, 1.98 m/s) and to that between ASCAT and ERA-interim (L.236, 1.43 m/s). It is probably valid to assume that ASCAT is the true value, but it was not clear to me if the use of the ABL model improved the wind output and/or ocean state estimate. Fig.1b suggests that ABL model did not change the distribution of wind speed much, which is not validation. In Section 3.2, the model was deemed validated based on the qualitative agreement of the present result with past work, some of which are theoretical and simulations. (L.289, L.297). I am not convinced that the ABL model is validated with these attempts.

For the ocean current feedback, I failed to see the significant of the present work, although many experiments were described in detail. I could not point out what was discovered in this work and what was already known. The results are often described without interpretation (e.g. why the KE response is quasi-homogeneous in Fig.6 despite its inhomogeneous mean state?). I also could not find the reason why the authors chose "a region of low mesoscale activity" (L.90) for this particular study.

Based on these problems, I was not convinced that the manuscript merits publication in the present form. A major revision might be able to clarify the novelty of the present work and to place it in the context, but I feel it more appropriate to handle it as a re-submission.

L.41, Small et al.,(2008) not found in the reference list.

L.69, "significative" → significant?

Eq.4, The curly brackets do not make sense. Also I could not find the definition of C1.

L.228, Change comma to period in "1,22"

L.264, What is σ here?

Tables, Captions to tables are usually placed above the table.

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Discussion paper