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Interactive comment on "Feedback of mesoscale ocean currents on atmospheric winds in high-resolution coupled models and implications for the forcing of ocean-only models" *by* Rafael Abel et al.

Anonymous Referee #2

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article [utf8]inputenc Abel et al. seek to extend the work of Renault et al. (2016) to the global domain. They follow the suggestion of Renault to allow forced ocean only surface momentum fluxes to be modified by the degree to which the ocean current is included in the parameterization. In so doing they demonstrate, quite cleanly, that EKE is increased by a modest amount in certain energetic regions of the ocean in ocean only simulations when the surface current is not allowed to modify the surface momentum flux. I offer some broad questions regarding the method and wider applicability followed by a number of more minor comments for the authors to consider.

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1 General Comments

1. My biggest issue with this work was that I came away from the manuscript not fully convinced as to the importance and/or broader applicability of this work. For example,

   (a) It seems that to get the coupling coefficients (s_w) for the ocean only run, you must run the coupled model first. If this is correct, I have a number of issues, (1) doesn't the degree of imprinting of the surface currents on the atmosphere depend critically upon the chosen surface stress parameterization in the model coupler? Relatedly, inclusion (or discussion) of the momentum flux parameterization in the coupled configuration would be helpful for this manuscript. In particular, is the full effect of the surface current included in the stress parameterization? (2) If you would have to run the coupled model to find the α and s_w, s_{st} values this leads me to question how useful this potential parameterization might be. If I have to run the coupled model, why not just run and analyze the coupled model? Further, if you had to run the coupled model, why not use forcing from the fully coupled run to drive the ocean/ice run?

   (b) I do not feel that you proved that your ocean-ice case behaved similarly to the fully coupled run. For example, if you are using α (and corresponding s_w) values derived from a fully coupled run, wouldn't you expect the coupling (linear relationship) between the stress and the surface current to be similar when using a forced ocean/ice only simulation except where subsurface ocean dynamics are of leading importance? Could this perhaps explain the discrepancy near the ACC in Fig. 7-8?

2. Despite the two concerns above, I found the connection of surface current imprinting to near surface atmospheric stratification to be an important and interesting result. Could there be regions where a similar argument could be made for the

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ocean? For example, where ocean boundary layers are deep the momentum of a surface current is spread over a deeper depth, which may reduce the degree of coupling/imprinting.

3. In the paper, you state that a direct comparison of EKE is not appropriate. While I agree with the reasoning here, I feel a comparison of mean state biases between the two ocean/ice runs (imprinting vs. no imprinting) and between the ocean/ice and fully coupled runs is warranted. You convincingly show that EKE and MKE is changed in the ocean/ice run with the modified α values, but I did not see any convincing evidence that these changes are for the better. For example, does the change in ACC EKE drive the ocean/ice run to a more realistic state? This seems important.

   I do not mean to suggest that ocean currents should not be included in the surface stress calculation, but I don't feel you have adequately proven that the current should be scaled by some value α instead of just using relative winds ($\alpha = 1$). Thus I would suggest that a comparison of mean state biases for different values of α is warranted.

4. For your analysis you are using monthly-mean output to diagnose the impact of mesoscale variability. It seems that this will reduce oceanic variability and will only accurately capture fairly stationary mesoscale variability. Have you examined the influence of this choice?

5. Minor, but general point. Your terminology throughout of forced ocean/ice versus coupled is imprecise. The ocean/ice simulations are still coupled. I would suggest fully coupled vs. ocean/ice only.

2 Specific Comments

6. Your abstract suggests (especially near line 10) that your results imply the need for inclusion of this modified parameterization in fully coupled simulations. I think you intend to imply this for ocean/ice only. Please clarify.

7. The sentence beginning "The positive feedback of mesoscale currents..." is not clear when just reading the abstract, and only became clear after reading the paper. Could more detail on the physical mechanism of the feedback be added here?

8. At the end of the abstract you mention a 10% increase in kinetic energy, but most of the time you discuss reductions for this inclusion. Can you clarify the point here? I think you mean compared to assuming no influence of ocean surface currents in the surface stress parameterization.

9. Line 16 of introduction: doesn't the influence of vertical shear suggest an influence for ocean mixing parameterizations? This was not mentioned in the paper.

10. First sentence following equation 1. You state that C_D depends on the choice of α, this is not at all apparent here. Please explain further.

11. I don't feel the explicit definition of surface stress instead of wind stress is important. You only consider the influence of wind in this work. I would use one or the other and not bother with the definition.

12. In appendix A you discuss sensitivity to filter cut-off, but what about to the filter type itself?

13. in Section 2.1 you discuss bin averaging for a few sentences and state it was needed before, but not in your work. Can you explain why? I think another alternative is to simply remove that discussion.

14. Your discussion of the coupled model simulations (Section 2.2 end of first paragraph) suggest that you only allowed 4 years for spin up (19 years total run, 15 years for the analysis). If this is true, that length of spin up for a coupled model seems tenuous, even for a focus on the surface ocean.

15. Relatedly what is the initial condition for the ocean? In many models, certain initial conditions can lead to a need for a very long spin up (More than 100 years).

16. In your ocean/ice only simulations are you using COREv1, no interannual variability? If so, is there a reason for this choice? COREv2 allows for interannual variability and higher spatial and temporal resolution of the forcing. Or as mentioned previously, why not simply use atmospheric forcing from the fully coupled simulation?

17. Your core relationships (equations 2 and 3) assume a linear relationship. Is this the only assumption tested? For example, looking at Figure 1 (c in particular), you could perhaps see a quadratic fit as more representative of the data.

18. Line 8 page 4, isn't the lack of imprint in a ocean/ice only simulation by definition? I assume absolute winds are used.

19. Why is all your analysis at 2^o x 2^o? This choice was never explained and seems important. Further, do you calculate the curl and then regrid to the coarse resolution?

20. Figure 1 – How does this figure show a dependence on stability?

21. Figure 1 – why choose june? Is there a way to show the linear assumption is reasonable for all months?

22. page 5, line 22. Why do you expect the coupling to be weaker for higher resolution? If this resolution better resolves the mesoscales (especially over the ACC) wouldn't we expect this to be closer to truth values?

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23. Figure 2 – a new color scheme with a wider range of color would make interpretation far easier.
24. Figure 2 – are your monthly values based on full simulation length results? Or a subset?
25. when you compute atmospheric stability, why choose 20m and 53m, seems random. My guess is these are the first two model layers? If so, perhaps just say that? Is there sensitivity to this choice?
26. Figure 3 – what are the correlations between? I think it is between s_w values and stability, but it is not clear.
27. Can error bars be added here? Your one month in Figure 1 shows a relatively small error, but no other months are shown.
28. Figure 4 – This figure is quite confusing to me. It is not clear how this shows annual variability. If I read it correctly, it shows how much variance there is about a monthly mean, which isn't necessarily a seasonal cycle. Would a power spectral density plot in given regions be more clear?
29. Figure 4 – again, I think a wider color scale would be beneficial here. Further your label is not right (specifically s_w)
30. Page 7, line 4. How can you draw conclusions on the tropical / subtropical oceans with such large regions that are grayed out.
31. Toward the end of the full paragraph on page 8, you assign a fairly large value of α to the gray regions, this is presented without justification. Throughout the paper you show an importance to the parameter, so did you examine the influence of this fill value? There is a ton of gray in the figures, so this choice seems important.
32. Figure 5 – is the stability computed at the same levels as mentioned in the text?

33. Figure 6 – Why show C1/4 results? Why not the higher resolution results, especially if the mesoscales are better represented?
34. Figure 7 and 8, I think it would be easier to interpret the plots if you could have the fully coupled (or ocean/ice) result and then a difference plot between the two tests.
35. I think it would be cleaner to see your large section of data on line 10-13 of page 12 in a table.
36. Throughout most figures, verify "Grey areas are as in Fig. 2"
37. Figure 10 – the embedded panel in (b) is confusing. I would suggest separating to panel c or omitting as it shows a similar behavior to the pacific section near the equator.
38. in your WCRP comment – are they suggesting the use of absolute winds for ocean/ice only?
39. Last line of section 4. By state parameters, would you get these from reanalysis, CORE forcing, other? A general enough parameterization seems incredibly difficult.
40. Figure A1 – Does your conclusion change if C1/12 data is used?

3 Technical Comments

1. Line 2 of abstract – "model with an eddying ocean"
2. Line 2 of introduction – some references seem useful at the end of this sentence.

3. Line 5 of intro – again references would be useful here
4. Line 6 of Intro – same, references
5. Line 27 of intro (page 2) – "the EKE of vortices and their lifetime" is hard to read. Perhaps "with respect to vortex lifetime and EKE"
6. Page 2, 3 lines above the line 5 marker should be "damping of EKE"
7. Page 2, 1 line above the line 5 marker, extra space between 20 and %
8. Line 4 page 3, need comma after stress
9. Line 4 page 3, Starting with Assuming linear relationship doesn't read well. I think perhaps "Here we assume a linear relationship..., where the coupling coefficients..."
10. second to last line of page 3 – however, bin averaging was not found...
11. Line 19 page 4 – why have italics for "25 km"?
12. Line 21 page 4 – suggest removing parenthesis around for C1/4 and C1/12
13. Near discussion page 4, It may be useful to cite the work of Small et al. (2014, JAMES) who studied the influence of resolving the mesoscales in CESM.
14. Line 12 Page 5, strike 'thus'
15. Line 19 page 5 – I would suggest "The largest mean values of s_w (up to 0.5) are found..."
16. Line 24, page 5 – "wind depends on the atmospheric boundary layer state."
17. Line 2 page 6, after region a comma is needed

18. Line 16 page 7 – "The is emphasized in Fig. 5, which shows the correlation....
19. Line 11 Page 9 – I would consider putting the coupling coefficients in parenthesis.
20. Line 14 Page 9 – explain "top drag" and clarify, is it mesoscale ocean features?
21. Figure 6 – Why use color here? it isn't discussed and doesn't add to the discussion.
22. Line 6 of page 11, "simulated surface ocean"
23. Figure 9 – (to exclude sea-ice regions). Grey areas in Fig. 2 are excluded.
24. last line of page 14 - 15 " our results suggest using a revision of relative winds in the form..., where a variable coefficient s_w is used in conjunction with..."
25. Appendix A – I would change "show this exemplary" perhaps and is shown for three locations...
26. Appendix B – curl(u) needs consistent notation.
27. Figure A2 – change std to σ

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