

## Response to Reviewer #2

We thank the anonymous reviewer for his or her constructive comments and address them here.

Before responding to the individual comments we have to admit, that we might not have been clear enough about the objectives of this study which may have given the reader a misleading impression. The objectives of the study are (now emphasized in the manuscript): on a global scale to (1) identify the imprint, including its spatial distribution, of mesoscale ocean current features on the near-surface winds over the global ocean by inspecting spatially high-pass filtered surface currents and near-surface winds; (2) to investigate its repercussion for the surface momentum flux, i.e., the effect of the current - wind feedback on the current - surface stress relation; (3) to give a preliminary assessment of its potential impacts for the mean and eddy kinetic energies of an eddy-permitting global ocean model, based on a tentative implementation of the diagnosed spatially-variable, monthly-mean distribution of the current-wind coupling in the bulk surface stress formulation.

In order to separate the objectives we modified section 3.2 and now the objectives (1) and (2) deal just with the coupled simulations and therefore this part of the manuscript is not affected by any differences in forcing or bulk formulations between the coupled and uncoupled simulations. For the coupled simulations we show that the re-energization is at work and this re-energization is not found in the uncoupled simulations. Then we are able to show that there is a mismatch in the surface stress coupling coefficients between the coupled and uncoupled simulations. Finally we show that this mismatch is related to the missing re-energization effect in the uncoupled simulations and that the tweak proposed by Renault et al. (2016b) is surprisingly good at reproducing the mechanical coupling seen in the fully coupled models.

Our results give a strong indication that the re-energization of near-surface winds should be implemented in uncoupled ocean models.

This leads to the overall idea that reviewer #1 made in his/her review: the design of a study that extensively tests and validates a parameterization of the re-energization of near-surface winds. Such a study should use a global high resolution coupled atmosphere-ocean model run with relative winds and from this simulation a forcing dataset needs to be build which is a very demanding task on its own. Subsequent uncoupled simulation with the identical ocean model and bulk formulations can then be forced with this particular forcing dataset to ensure that comparable wind power inputs are achieved. Within such a modelling project an extensive analysis of the surface ocean energetics might be possible.

In our case, we made use of an existing coupled simulations that were designed for another study. These studies are computationally very demanding and cost on the order of 100,000 CPU hours per model year (Hewitt, 2016; Small 2014) and to date only a handful of these simulations have been performed. We think that Renault et al. (2016b) and our study serve as good justification for such a project and the related major modelling effort.

Before responding to the individual comments we have to admit, that we might not have been clear enough about the objectives of this study which may have given the reader a misleading impression. The objectives of the study are (now emphasized in the manuscript): on a global scale to (1) identify the imprint, including its spatial distribution, of mesoscale ocean current features on the near-surface winds over the global ocean by inspecting spatially high-pass filtered surface currents and near-surface winds; (2) to investigate its

repercussion for the surface momentum flux, i.e., the effect of the current - wind feedback on the current - surface stress relation; (3) to give a preliminary assessment of its potential impacts for the mean and eddy kinetic energies of an eddy-permitting global ocean model, based on a tentative implementation of the diagnosed spatially-variable, monthly-mean distribution of the current-wind coupling in the bulk surface stress formulation.

In order to separate the objectives we modified section 3.2 and now the objectives (1) and (2) deal just with the coupled simulations and therefore this part of the manuscript is not affected by any differences in forcing or bulk formulations between the coupled and uncoupled simulations. The results of 3.1 and 3.2 show that the re-energization is at work and is missing in uncoupled simulations which leads too strong damping. Afterwards we test the tweak proposed by Renault et al. (2016b) in order to get more realistic damping of surface currents.

Our results give a strong indication that the re-energization of near-surface winds should be implemented in uncoupled ocean models.

This leads to the overall idea that reviewer #1 made in his/her review: the design of a study that extensively tests and validates a parameterization of the re-energization of near-surface winds. As he/she claims such a study should use a global high resolution coupled atmosphere -ocean model run with absolute and relative winds, and the absolute wind simulation should serve as the forcing for an subsequent uncoupled simulation with the identical ocean model and bulk formulations. We also think that the construction of a forcing dataset for global uncoupled from a coupled simulation requires careful examination and might be not as easy as in a regional configuration. However such a project the analysis you suggest would be possible.

In our case, we made use of an existing coupled simulations that were designed for another study. These studies are computationally very demanding and cost on the order of 100,000 CPU hours per model year (Hewitt, 2016; Small 2014) and to date only a handful of these simulations have been performed. We think that Renault et al. (2016b) and our study serve as good justification for such a project and the related major modelling effort.

*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.*

#### *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  $\alpha$  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?*

- The derived alpha-values from the coupled experiments are freely available as stated in the manuscript. Regarding NEMO modelling efforts we are happy to share our implementation. The surface stress does indeed depend on the choice of the bulk algorithm (Brodeau et al., 2017), and might differ up to 20% between the NCAR algorithm, which we used for the forced simulations and the COARE algorithm that were used in the coupled simulations. This is now mentioned in the manuscript.
- The coupled simulations were run with relative winds.
- The derived alpha-values are from two simulations and could also be used for other ocean-only experiments, probably also with other ocean models. Also when using a forcing derived from a coupled simulation one would use an atmosphere that has seen a different oceanic state than produced by the forced simulation. Therefore re-energization effects are already in the forcing. Such a mismatch could lead to spurious surface stresses.
- For the ocean-only modelling community it is important to rely on a common forcing dataset to be able to do comparison between simulations by different groups (Griffies et al., 2009). Therefore we chose to use the widely used CORE v2 forcing.

*(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  $\alpha$  (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?*

- Our intention is not to fully prove that the forced and the coupled simulations behave similar down to details in the ocean circulation. Therefore we would need the same ocean models (in the forced and coupled case) and an forcing from a coupled simulation with spatially low-pass filtered winds to ensure that the imprint of ocean surface currents is eliminated. Beyond this the construction of such an forcing dataset needs very careful treatment and is a demanding task on its own. This is unfortunately beyond the scope of this study and we therefore chose to do sensitivity simulations and to look directly at the damping process in form of the curl of the surface stress.
- An analytical deviation of the surface stress to see the exact relation between curl( $\tau$ ) and curl( $u$ ) is to our knowledge not possible and the re-energization effect also depend on the atmospheric state. The idea of a tweak as proposed by Renault et al. 2016 is therefore not straight forward. The best measure to see the influence of this tweak is actually the calculation of the coupling coefficient  $s_{st}$ , which is the direct result of changes in the surface stress calculation (as shown in Fig. 6-9).
- The drag coefficient is strongly dependent on the background wind and surface stability conditions which are different in the ACC region in the forced and coupled simulation and are likely to contribute to the differences 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 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.*

- Indeed our findings regarding the dependence of  $s_w$  on the near-surface stability are interesting and need further investigation in more idealized studies.  $s_w$  is basically the coupling between the surface current and the near-surface winds on small scales and displays how strong the surface currents influence the near-surface winds (and not the other way around, see Fig. 1). Therefore the vertical structure in the ocean should not play a role for the coupling coefficient  $s_w$ .
- But we do agree, that if the momentum flux at the surface is distributed over a deeper ocean mixed layer the surface currents are weaker with respect to a shallow oceanic mixed layer.

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  $\alpha$  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.

- EKE levels in the coupled simulations are much larger than in the forced simulations, which is likely due to the finer resolved atmosphere (25km and 200km) and higher coupling frequencies (3 or 1 hourly and 6 hourly). Therefore we still think that a direct comparison is not appropriate.
- From the model setup used here it is hard to tell, if the EKE and MKE is more realistic, but we show what the impact is, if we include a physical process with a tweak into a forced ocean model. For  $\frac{1}{4}$  degree simulations the absolute wind configuration produces higher EKE levels and is therefore closer to EKE from AVISO geostrophic currents. One has to keep in mind that due the only eddy-permitting resolution of  $\frac{1}{4}$  degree generally lower EKE levels are achieved and EKE would not be a good measure with respect to observations. The EKE levels are of course higher when the main damping mechanism is missing in the 'absolute winds' simulation.

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  $\alpha$  instead of just using relative winds ( $\alpha = 1$ ). Thus I would suggest that a comparison of mean state biases for different values of  $\alpha$  is warranted.

- As defined by Renault et al. (2016b) the coupling coefficient  $s_{st}$  is a measure how strong surface currents are damped (more negative  $s_{st}$ , more damping). We find that when using the tweak of Renault et al. (2016b) the damping is smaller with respect to  $\alpha=1$  simulations and therefore close to the damping estimated from coupled simulations. We think that this is already a strong hint that  $\alpha=1$  simulations seem to be missing a process and this is likely the re-energization effect.
- We find that the mean patterns of EKE basically do not change, but the EKE level, when changing values of  $\alpha$  (as also shown in Fig. 10).

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?

- We are examining an process (influence of the ocean surface currents on the atmospheric winds) which is mainly dominated by an process in the other direction (winds influencing the surface currents). Therefore we need to get rid of the impact from the atmosphere (compare Fig. A1). Given the time scales inherent by the atmosphere and the ocean one month seemed reasonable and is close to the one month running mean chosen by Renault et al. (2016b). We also would like to examine the influence of shorter timescales, but from the coupled simulations only one monthly data was available and the runs were made before this project came into our minds.

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.*

- We totally agree that the forced simulation are coupled to an ice model, but as the ice covered regions are excluded in this study (due to effects on the surface stress due to ice) and the extra information about the coupling to the ice model might lead to misunderstandings, we prefer to reduce the terminology so that it contains only the most important information.

## *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.*

- We suggest to incorporate the re-energization using the tweak proposed by Renault et al. (2016b), for ocean-only simulations. We made this clearer in the abstract.

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?*

- When using relative winds, i.e. surface currents in the surface stress calculation, the surface stress acts as a damping for surface currents. We found that there is also an anomalously wind excited that acts in the opposing direction of the surface stress and therefore reducing the overall damping effect on the surface currents. More details are now added to the abstract.

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.*

- The 10% increase is with respect to the 'relative wind' formulation, so the formulation where ocean surface currents are considered in the surface stress formulation. Details added to the abstract to clarify.

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.*

- We made now clear that it is the vertical shear between surface currents and surface (10m) winds.

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

- The drag coefficient  $C_D$  depends on the velocity at the surface and the surface stability. The velocity at the surface does depend on the choice of  $\alpha$  and therefore also  $C_D$  does, but the effect is indirect.

*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.*

- In the past the term 'wind stress' was used to characterize  $\tau$ , but this wording might suggest that only the wind determines the stress. Therefore we chose to use 'surface stress' to make clearer that both surface ocean currents and surface winds likewise determine the stress. Throughout the text we use therefore the wording surface stress

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

- We performed sensitivity testes with filters cut-offs ranging from 300km to 75km and while the intensity of the coupling coefficients changed slightly, the overall pattern were insensitive. Commonly the cut-off length has a much stronger impact on the outcome than the type of the filter, i.e. switching between Hanning and boxcar filters.

*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.*

- Earlier studies that looked at the impacts on SST gradients and on the influence of surface currents on wind (Renault et al., 2016b), used the bin averaging and we wanted to acknowledge their work. We chose not to use the bin averaging as it largely reduces the information available in the data and therefore the degrees of freedom. Therefore we would like to keep 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.*

- We are aware that the spinup of the deeper oceans may take a few years or even a hundred years. But regarding surface kinetic energy the models are usually spun up after a few months. In the coupled models the analysis focuses on the ocean surface currents and their effect on the surface stress and near-surface winds. Therefore we do not need the ocean to be totally spun up, but rather need to consider the time it takes to develop ocean eddies and their lifetimes (up to a few hundred days).
- When we compare the EKE in the forced ocean models, a 30 + 5 year spinup was

done. Which should be sufficient to compare the surface EKE.

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).

- The coupled simulations are initialized with '5-year September mean temperature and salinity from the EN3 observational data set' (Hewitt et al., 2016).
- The forced ocean models are initialized with salinity and temperature from World Ocean Atlas 1998 (Antonov et al., 1998; Boyer et al., 1998)
- Again, as we do not compare the ocean states directly the choice of initial conditions should be negligible.

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?

- Yes that is a very good point that needs to be clarified. We use COREv2 interannually varying forcing, but still it has 2 degree spatial and 6 hourly temporal resolution. The idea of this study is, to show that even with standard COREv2 forcing the tweak proposed by Renault et al. (2016b) an uncoupled simulation does produce relative realistic coupling coefficients  $s_{st}$  with respect to the coupled simulations. We would like to have used an atmospheric forcing derived from the coupled simulations, but they are not available and they were run before this project was started. Additionally the coupled atmosphere already has an imprint of the surface currents in the atmospheric winds. To avoid the imprint of the surface currents a careful construction of a forcing dataset is necessary and probably involves new simulations of the coupled simulations. Unfortunately one model year costs on the order of 100,000 CPU hours (Small et al. 2014, Hewitt et al., 2016).

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.

- We only tested for a linear relationship, but we agree that there is quite some spread that seems to be due to the influences of the winds. We now reduced the dot-size in the scatterplot to better show how these are really distributed.

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.

- This is an important point to be made clear. We know that the most of the energy in the ocean is due to winds. But as shown here, there is no relation at the mesoscale in forced (either relative or absolute winds simulations) ocean models between the curl of the surface currents and the curl of the winds (Fig. 1a). But as there is a relation in the coupled models (Fig. 1b,c), it means that it is the surface currents that drive the atmospheric winds. This is actually a crucial point of the whole paper.

19. Why is all your analysis at  $2\sigma \times 2\sigma$ ? This choice was never explained and seems important.

*Further, do you calculate the curl and then regrid to the coarse resolution?*

- In order to perform the linear regression we needed to get a sufficient amount of data. We calculate for 2x2 degree boxes on the 0.25x 0.25 degree grid to have  $8*8*15=960$  data points for every month for the 15 year analysis period. All curls are calculated on a regular 0.25x0.25 degree grid.

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

- This part of the caption was wrong. Removed.

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

- During summer the strongest signals are found (Fig. 3) and therefore we chose June. We tested other months at the Gulf Stream, the ACC and exemplary a few other regions and all show reasonable regression with respect to the scatter plots. Note that this is not the case anymore in non-energetic regions (Appendix B).

*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?*

- As the relation between the curl of the surface currents and the surface winds/tau is linear, the coupling is not necessarily stronger when the surface ocean is more energetic. Additionally we just find that with ORCA12 the coupling coefficients  $s_w$  are slightly larger, which might be related to maybe slightly stronger stability over the GS Extension, but it is not clear.

*23. Figure 2 – a new color scheme with a wider range of color would make interpretation far easier.*

- We increased the resolution of the colorbar to 0.02. We also keep the colormap as cmocean (Thyng et al., 2016).

*24. Figure 2 – are your monthly values based on full simulation length results? Or a subset?*

- All analysis is based on the last 15 years of the simulation. This information is now added to the figure caption.

*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?*

- Indeed the first two model layers are at 20m and 53m. There is a sensitivity to this choice, but it seems to be crucial whether the atmosphere is stable close to the surface. Sometimes the atmosphere is unstable over a deeper layer, sometimes not. However the surface is most important, compare also to Renault et al. (2016b) Fig. 10b.



26. Figure 3 – what are the correlations between? I think it is between  $s_w$  values and stability, but it is not clear.

- Yes, the correlations are between the coupling coefficient  $s_w$  and the vertical temperature gradient. It is now noted in the figure caption.

27. Can error bars be added here? Your one month in Figure 1 shows a relatively small error, but no other months are shown.

- The errors are relatively small. But error bars are now added to Figure 3a. For visibility reasons we decided not to include them to Fig. 3b. But the errors are of equivalent magnitude.

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?

- We computed the standard deviation of the monthly values of  $s_w$ . This thus is a measure how strong  $s_w$  varies between the month. It is intended to give a global view of the variations that were shown for the Gulf Stream and ACC in Fig. 3. Indeed it is not necessarily the seasonal cycle, but a good measure for the temporal variability over a year.

29. Figure 4 – again, I think a wider color scale would be beneficial here. Further your label is not right (specifically  $s_w$ )

- A wider color scale is applied and the right label  $\sigma(s_w)$  is added.

30. Page 7, line 4. How can you draw conclusions on the tropical / subtropical oceans with such large regions that are grayed out.

- Of course our conclusions are for the regions that are not grayed out. We noted that now in the text.

31. Toward the end of the full paragraph on page 8, you assign a fairly large value of  $\alpha$  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.

- The finding, that the ocean surface currents do change the surface winds was only found employing the linear regression of scatterplots. Unfortunately when  $\text{curl}(u)$  values are not large enough, the limits of this methodology seem are reached (Appendix B). This does not mean that the process is not happening there, but it is masked by other processes. Therefore we decided that we want to have this mechanism also working in the forced ocean models, and applied our 'best guess' value (the global and temporal mean of  $s_w$ ) for those regions. As we do not have information about the month to month variability we chose to handle it conservatively and do not allow for temporal variability. Although we did not examine the sensitivity of this choice, it is clear that for larger (smaller) alpha-values

a stronger (weaker) damping is applied to surface currents.

32. *Figure 5 – is the stability computed at the same levels as mentioned in the text?*

- Yes, again the first two model layers. Now noted in the figure caption.

33. *Figure 6 – Why show C1/4 results? Why not the higher resolution results, especially if the mesoscales are better represented?*

- The coupled simulation C1/4 has the same horizontal resolution as the uncoupled experiments. Therefore we used C1/4 to compare it to the uncoupled simulations. The results of C1/12 and F\_alpha=C1/12 are mentioned in the text and show a slightly larger difference with respect to s\_st. Even if the mesoscale feature are better represented in C1/12 we rather chose to make the main comparison between the models that share the same oceanic grid.

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.*

- Yes, that is a good idea. Figures adapted.

35. *I think it would be cleaner to see your large section of data on line 10-13 of page 12 in a table.*

- Indeed, with the table it is much clearer now.

36. *Throughout most figures, verify "Grey areas are as in Fig. 2"*

- Done.

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.*

- We agree, the extra information due to the Kuroshio current is marginal and removed it therefore.

38. *in your WCRP comment – are they suggesting the use of absolute winds for ocean/ice only?*

- The meeting by the CLIVAR Ocean Model Development Panel (OMDP) was about forced ocean and sea-ice models. They indeed suggested for these kind of models to use absolute winds. I think that the main motivation was that with absolute winds the EKE levels of eddy permitting models are closer to 'reality' (AVISO).

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.*

- As we were able to show that the coupling parameter s\_w is related to the near surface stability of the atmosphere, there could be hope that with data that is available at the ocean surface some sort of parameterization might be possible, but as you say it might be probably a demanding task to work on such a parameterization.

The re-energization of near-surface winds needs certainly be studied in more dedicated studies employing high resolution boundary layer models and idealized (uncoupled) ocean surface forcing for these boundary layer models. I think with the climatologies of  $s_w$  we provide along with this paper we made a good step in the right direction.

40. *Figure A1 – Does your conclusion change if C1/12 data is used?*

- Even for C1/12 the spectra look surprisingly similar even with the better resolved mesoscale in C1/12. But due to technical limitations we also used ocean surface velocities that were interpolated on the atmospheric (25km resolved) grid.

### 3 Technical Comments

1. *Line 2 of abstract – "model with an eddy ocean"*

- Corrected.

2. *Line 2 of introduction – some references seem useful at the end of this sentence.*

- The first sentence is an introduction to the paragraph. Right below we give more details and references.

3. *Line 5 of intro – again references would be useful here*

- Xie (2004) and Chelton et al. (2004) actually investigated the thermal effect driven by strong SST gradients on winds.

4. *Line 6 of Intro – same, references*

- Added.

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"*

- The whole sentence is rephrased now.

6. *Page 2, 3 lines above the line 5 marker should be "damping of EKE"*

- Now changed to: damping on surface currents.

7. *Page 2, 1 line above the line 5 marker, extra space between 20 and %*

- Removed the extra space.

8. *Line 4 page 3, need comma after stress*

- Added a comma.

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..."*

- Reads better, thanks. Changed.

10. *second to last line of page 3 – however, bin averaging was not found...*

- Changed.

11. *Line 19 page 4 – why have italics for "25 km"?*

- Typo. Changed.

12. Line 21 page 4 – suggest removing parenthesis around for C1/4 and C1/12

- Done.

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.

- The pioneering work of Small et al. (2014) is indeed very nice, but here we wanted to state, that the model setup we use, was already shown to have impacts on the oceanic mesoscale and was used to study the impact of SST – surface stress relationship.

14. Line 12 Page 5, strike 'thus'

- Done.

15. Line 19 page 5 – I would suggest "The largest mean values of  $s_w$  (up to 0.5) are found..."

- Changed.

16. Line 24, page 5 – "wind depends on the atmospheric boundary layer state."

- Changed.

17. Line 2 page 6, after region a comma is needed

- Added.

18. Line 16 page 7 – "This is emphasized in Fig. 5, which shows the correlation...."

- Changed.

19. Line 11 Page 9 – I would consider putting the coupling coefficients in parenthesis.

- Done.

20. Line 14 Page 9 – explain "top drag" and clarify, is it mesoscale ocean features?

- The surface stress due to surface currents is actually the friction or drag these currents feel and finally damps them. In the case of the high pass filtered curls of the ocean currents and curl of the surface stress it is actually the mesoscale ocean features. Clarified, that we are talking about ocean features and not atmospheric features.

21. Figure 6 – Why use color here? it isn't discussed and doesn't add to the discussion.

- In order to emphasize the difference between the simulations and the strong similarity between  $F_{\alpha=C1/4}$  and C1/4 we used the colors. The color coding is now also depicted in the figure caption.

22. Line 6 of page 11, "simulated surface ocean"

- Changed.

23. Figure 9 – (to exclude sea-ice regions). Grey areas in Fig. 2 are excluded.

- Changed.

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..."*

- Changed.

25. *Appendix A – I would change "show this exemplary" perhaps and is shown for three locations...*

- Changed to exemplary. But if you mean that exemplary and three locations is a little bit contradictory: We mean that we just show three regions and not more areas of the world ocean.

26. *Appendix B – curl( $u$ ) needs consistent notation.*

- Changed.

27. *Figure A2 – change  $std$  to  $\sigma$*

- Done.

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