Response to Reviewer #1

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 you made in this 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.

In this manuscript, the authors aim to test a parameterization of the atmospheric response to so-called current feedback to the atmosphere. Using global coupled model with a spatial resolution of 1/4 and 1/12, they first confirm the existence of linear relationships between surface currents and wind and and surface currents and wind stress. The associated coupling coefficients s w and s st defined in previous studies are then estimated and used 1) to test a parameterization of the wind response to the current feedback in uncoupled ocean model and (2) to validate such a parameterization. This paper addresses relevant scientific questions, an uncoupled oceanic *simulation should incorporate the current feedback to the atmosphere but also the atmospheric response through a parameterization. Although the premise of the study is interesting, there are some strong limitations that should be addressed in order to properly test the parameterization and to give some robustness to the conclusions. In particular the model setup of the uncoupled oceanic simulations should be better designed and the results should be better analyzed (see general comments). The results are therefore not sufficient to support the interpretations and conclusions. The introduction should be improved by better acknowledging what is really new in that paper and what has been done before. The results should also be compared to the existing literature. The overall presentation is well structured and clear and the language is fluent enough.*

General Comments:
1) I'm seriously worried by the model setup that presents many weaknesses. This should be addressed before trying to test and to validate the parameterization as those weaknesses may have a strong influence on the results. There are many difference between the coupled simulations and the uncoupled ocean model, making their comparison impossible.

- We think that the comparison between the coupled and uncoupled simulations based on s_sst is quite promising and we find that in the uncoupled simulations with alpha from the coupled simulations the s_st are quite close to the uncoupled simulations. We agree that differences in the bulk formulations and the forcing introduce differences in the surface stress and therefore s_st, but these seem to be of minor importance, as our results suggest.

a) The atmospheric forcing is drastically different. The CORE forcing has been chosen to force the ocean model. Why don't use the atmospheric forcing from the coupled simulation? the CORE forcing has a very low spatial resolution and a lower temporal frequency (that should be specified). The heat fluxes are certainly very different and the CORE wind would not be comparable to the coupled wind, especially in the nearshore regions. The atmospheric forcing frequency also introduces difference between the coupled run and the uncoupled run. The use of a such a different atmospheric product is problematic because the coupled model is not comparable to the uncoupled model, and the estimated coupling coefficients from the coupled model may not be valid for the uncoupled simulation.

- Output from the coupled simulations that could serve as a forcing is not available within this study and would need a new run of the coupled simulation with high temporal resolution and careful treatment to ensure energy consistency and to ensure that the imprint of ocean currents is eliminated in the atmospheric winds. The CORE forcing serves as a common forcing dataset for the ocean modelling community, therefore we chose to use it for our sensitivity studies. Comparision between the coupled and uncoupled simulations is based on the coupling coefficient s_st and not on EKE/MKE as these are already different due to differences in the forcing.

b) The bulk formulae is also very different, this will also introduce difference between the uncoupled and the coupled simulations (see e.g. Brodeau et al. 2016).

- Thank you for pointing this out. We now refer to the work of Brodeau et al. (2017) and mention the error that may be introduced by the choice of different bulk formulae in the coupled and uncoupled simulations. We take your comment seriously, but think that it would not change the main results of the s_st based comparison.

c) The spatial resolution of the uncoupled domain is too coarse. A 1/4 ocean model is only eddy permitting. A 1/12 ocean model will not surprisingly generate much more EKE than a 1/4 degree model. This is an important issue when addressing the impact of the current feedback on the EKE and testing the proposed parameterization using among other diagnostics the EKE and the MKE. An eddy resolving model is needed to properly assess the EKE dampening by the current feedback and its partial re-energization by the atmospheric response. The mean states of the uncoupled models and of the coupled models are likely very different as the EKE should be as recognized by the authors in the paper. The parameterization should be tested on an eddy resolving model. The ocean uncoupled model should be run with a 1/12 spatial resolution using the atmospheric from a coupled simulation with the very same frequency, bulk formulae, etc .. it should try to mimic the coupled simulation.

- We are aware that the inherent EKE level of a ¼ degree model is much lower than the EKE level of a 1/12 model, therefore we compared the results of the C1/4 (the coupled model with ¼ degree resolution) with the uncoupled models and used the 1/12 uncoupled model solely to show the difference of the coupling coefficients s_w and s_st between the coupled simulations. And these are relatively small.
- The main comparison between the coupled and uncoupled simulations is based on s_st and the agreement is quite promising.
- On the use of the atmospheric forcing from a coupled simulation to force an ocean-only simulation: We agree that this is a good idea and the results of Renault et al. (2016b) are very promising. However, on a global scale and on longer timescales than a few years it might be different, as the absolute wind configuration might produce a different climate than a relative wind configuration (both coupled models). We think this needs to be considered and would need further investigation.

Also, even if the authors refer to another papers, the model setup description should be improved. There is a lack of information on the coupled setup (e.g. number of levels, atmospheric simulations, …)

- The relevant information about the coupled simulations for this study are now updated.

2) I have been surprised the authors do not validate their uncoupled simulations with respect to the coupled simulation using the EKE and the MKE. This is major weakness of the paper. The claim the setup is not the very same is true but it should not be as the setup of the uncoupled model should be comparable to the one of coupled model. For example a larger EKE in the coupled simulation with respect to the uncoupled simulation may lead to a larger s w . It may cause a too large re-energization of the uncoupled simulation. The comparison between s st from the coupled simulations and the uncoupled simulations are not good enough to validate the parameterization. One could imagine the use of a constant s w (say of 0.3) would lead to similar discrepancies. The discrepancies should also be discussed and explained. EKE and MKE should be compared to the coupled simulation, that should be considered as the "true" when validating the parameterization.

- The aim of this study was not to fully validate the tweak (we think parameterization is a little bit overstated) of Renault et al. (2016b), but rather use a set of sensitivity simulations and show that a 'standard' uncoupled simulation (with COREv2 forcing) can get much closer to the coupled simulations with this tweak with respect to the surface stress coupling, which is a measure of the damping of surface velocities at the mesoscale.

- We agree that the logical next step would be to compare EKE and MKE, but as stated above this would be a large-scale project on its own and the production of the forcing data set is certainly more difficult than for a regional configuration.

3) As stated in the introduction, the current feedback to the atmosphere induces a dampening of the EKE. However, as shown by Renault et al. 2016b, this dampening is not only due to a reduction of the transfer of energy between the ocean and the atmosphere but to a negative WW'. It induces a sink of energy from the geostrophic currents to the atmosphere. Such a sink is present everywhere in the world ocean and, in particular, over the WBC such as the GS (e.g. Scott and Xu 2009, Renault et al., 2016c, Xu et al 2016). The current feedback to the atmosphere also induces a reduction (up to 30%) of the mean WW (Scott and Xu 2009), that is even more pronounced when using an active ocean (up to 50%, Renault et al, 2016c). This in turn causes a slow down of the mean circulation as shown by Pacanovski (1987), Luo et al (2005), and Renault at al. (2016c) for the North Atlantic Basin. A parameterization of the atmospheric response in an uncoupled simulation should be able to reproduce the current feedback induced changes of the transfer of energy between the ocean and the atmosphere. Again, the mean EKE and KE resulting from the uncoupled simulations should be validated and compared to the coupled simulations. This point should be introduced and discussed in the paper.

- We agree that in a model with an active ocean the effect on the wind work (WW) is even more pronounced. However as WW=tau * u_ocean, tau itself is already quite a good measure, as tau is effected directly and u_ocean only indirectly. We would like to have investigated the changes in WW itself, but due to the restrictions within this study the wind work was not an output variable of the coupled simulations and it is very important to have them calculated online as the time resolution matters a lot (Zhai et al., 2012).

4) The statistics should be improved. For example, s w , s st estimates and correlations with the surface stability.

- With the estimation of coupling coefficients we follow former studies that have performed these for SST and wind (e.g. Chelton et al., 2004, 2007) and surface currents and wind (Renault et al., 2016b). Additionally we did not apply binning in the scatter plots in order to not average out important information. Error estimation was done extensively for the coupling coefficients using the error of the slope of a linear regression including degree of freedom estimation. Errors for s_w are now included in Fig. 3.

5) The study should better acknowledge what has been done in previous studies. e.g., the coupling coefficient s w and s st have already been defined in the litterature. Some of the results shown in that paper are in agreement with previous studies (e.g. s w and s st scatterplot) but this is not discussed or compared. What s w and s st did you find for the US West coast?

- The CCS is better resolved in C1/12 than in C1/4. Renault et al. (2016b) found values of s_st=-1.2 +- 0.35 and s_w=0.23 +- 0.1 for a 5 years of data from their coupled experiments. We find for C1/12 s_st=-1.26 +-0.13 and s_w=0.28+-0.04 also for a 5 year mean with small deviations if another 5 year period is chosen.

- These results are now added to the discussion and compared to Renault et al. (2016b).

Some specific comments:

Line 20: Hugues and Wilson (2008) and Scott and Xu (2009) do not use an active ocean. The reduction of mean WW induces a slow down of the current and then an additional reduction of the mean WW, see e.g. Renault 2016 for the NATL/GS. The reduction of EKE is not only explained by a reduction of the the wind energy transfer but to a negative geostrophic eddy wind work, see studies from Scott and Xu (2009), Renault et al. (2016bc), Xu et al (2016).

- We are aware that Hughes and Wilson (2008) and Scott and Xu (2009) estimate the wind work based on wind stress measurements and measured surface currents. But still they estimate the influence of absolute or relative winds on the WW.
- We agree that if there is an active ocean the change in ocean surface currents due to different surface stresses have an secondary, yet, strong impact on the wind work.

25: This is an interesting statement. Scott and Xu (2009), and Xu et al (2016) show the mean eddy wind work is also largely negative is those regions, which contradicts the Byrne et al (2016) results. It would have been interesting to check the mean eddy wind work in your simulation to address whether there is also a dampening of the EKE in that region or a re-energization as suggested by Byrne et al. (2016). Note Byrne et al. (2016) use only 3 months of simulation, this could explain discrepancies with your own results.

- To clarify the findings of the earlier studies, we rephrased the last two sentences in this paragraph. Seo et al., 2016 found that mesoscale SST anomalies are of minor importance in the surface stress calculation for EKE. Xu et al. (2016) and Byrne et al. (2016) showed that the large scale wind stress curl is of importance for the uneven wind work over cyclonic and anticyclonic eddies. Byrne et al., 2016 aslo found out that mesoscale SST anomalies change the atmospheric boundary layer above which leads to differences in the wind and therefore differences in the wind work. These effects cannot be investigated in the coupled simulation we have at hand, as most of the output is only available at monthly time resolution.

page 2 L30 5: The dampening of the EKE is explained by a negative geostrophic eddy wind work that allows a sink of energy from the geostrophic currents to the atmosphere (See Renault et al., 2016b). Such a sink of energy is overestimated when using an uncoupled ocean model. The current feedback induces large scale sink of energy from geostrophic currents to the atmosphere (Renault et al. 2016c Xu et al 2016) that explains the EKE dampening. This should be checked in the coupled and uncoupled simulations.

- In our view, a more straightforward description of the mechanism basically focuses on the surface stress: Ocean surface currents induce a stress that is directed in the opposite direction of the surface currents and effectively damp the surface currents. Additionally as found by Renault et al. (2016) the stress exited by the surface currents also excite an anomalous wind that is directed in the same direction as the ocean currents and therefore reduces the relative velocity between ocean and atmosphere and subsequently reduces the surface stress and less damping is apparent. Of course this mechanism is not working in uncoupled simulations, but can be tweaked with the modification of the surface stress calculation as proposed by Renault et al. (2016). This change in damping of surface currents is discussed in more detail in section 3.2.

page 8 L10: what about just extrapolating the values ?

- As discussed in the Appendix B in less energetic ocean regions the methodology seems to produce unreasonably large values of s_w. It is fair to assume that the re-energization is still at work in more quiet ocean regions, but harder to detect given the noise due to the winds. Even if the s_w patterns found in some regions might suggest that extrapolation might be feasible, there is no indication that verifies this assumption. Therefore we chose to use the mean values from reagions were a s_w estimation was possible.

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