Answer to reviewers

Color code
Black: reviewer's comment
Blue: our answer to the reviewer's comment

We thank the two reviewers for constructive comments and suggestions, which we have taken into account in the revised version of the manuscript, as summarized below.

Referee #1

The manuscript addresses an interesting topic, estimating global wind work and presents interesting results. I like the idea of separating the time-dependent wind work to the low and high-frequency components. Though it is well written, I recommend a major revision based on my comments below:

- A more than 5 TW global wind work has been reported before as in Yu et al. 2018 (https://doi.org/10.1016/j.ocemod.2018.07.009) and Yu et al. 2019 (https://doi.org/10.1016/j.ocemod.2019.05.003). Though they focused on the wind work over ageostrophic currents (Yu et al., 2018) and used it to explain the global EKE reduction after including ocean surface currents in the wind stress formulation. But these were not cited in this manuscript. And I encourage the authors to do a more thorough search of the topic just in case.

Thank you for your suggestion. We now cite and discuss results from the following papers: Yu et al. 2018 and 2019. In particular, we explicitly mention the 5 TW reported by Yu et al. 2018. After searching papers on this topic, Yu et al.'s papers seem to be the most updated ones.

- I like the idea of low/high frequency wind work components but wondering if the 3.5-month data long enough. Is 3.5 month a good representation of the mean component? Is it long enough for low-frequency (seasonality)? I'd encourage the authors to extend the calculation to the whole 14-month available. This is the main reason why I recommend a major revision.

Thanks very much for this suggestion, which is really timely. Indeed, when we started this study, only 3 months of model output were available. In the present revised manuscript, we have extended our analysis to the last 12 months of the simulation, skipping the first two months because of spin-up issues. Description of the seasonality of the different wind work components is discussed in section 4, and shown on Figures 4, 5,6 and 8,9. Most of the main results are unchanged: when integrated globally the wind work of the different components does not change with seasons. However, as expected, their spatial distribution over the oceans strongly varies with seasons, with an intensification in winter as highlighted in the figures mentioned before.

- Really minor: Line 92, "January 20 using 2012 ocean initial conditions". I think 2012 is a typo otherwise would need to explain why 2012 not 2020.

Concerning this comment, we have added a text to explain what has been done. Specifically (see Lines 106-113 of section 2), we now write, "The COAS simulation was initialized on January 20 using 2012 ocean initial conditions from the forced LLC2160 MITgcm simulation and 2020 atmospheric initial conditions from the Modern-Era Retrospective analysis for Research and Applications, Version 2 (MERRA-2) interpolated to the C1440 GEOS grid. The reason for using 2012 ocean initial conditions is that there was no other spun-up MITgcm simulation of sufficient resolution available at the time we started the coupled simulation. The 2020 atmospheric initial conditions were imposed by the DYnamics of the Atmospheric general circulation Modeled on Non-hydrostatic Domains (Stevens et al., 2019) Phase II protocol. The mismatch in ocean and atmospheric initial condition years is not ideal but given that this is an unconstrained coupled simulation, the simulation year is notional."

Referee #2:

This study extends the work of Rai et al. (2021) to examine the importance of additional scales of variability in the context of wind work. It nicely shows the importance of high frequency variability. However, it seems to leave several aspects of this problem untouched, and these aspects substantially change the numerical results.

The selected drag coefficient is a reasonable choice for neutral boundary-layer stability; however, it is likely that locations with strong currents will also have substantially non-neutral boundary-layers. While I concede that considering stability won't qualitatively change the results, it might change the numbers by 10%, with greater impact on the high frequency numbers. The use of this assumption seems to be needless presuming that the model stress was saved, which seems like a certainty as the stress's curls are shown in the Strobach et al. 2022 paper.

Thank you very much for this comment. We are very sorry for the confusion. We confirm that all our wind work computations have been done using the model-computed wind stresses and ocean currents. In addition, the model-computed wind stress includes both the ocean current feedback as well as the impact of stability of the atmosphere above the ocean. Equations 1 and 2 are just mentioned to motivate the introduction. These comments are now explicitly mentioned in the present revised manuscript, in particular in the abstract (lines 2-3), the introduction (section 1, lines 22-23, 55-56). We have also added a paragraph (Lines 92-105 in section 2) that details how the wind stress is computed in the coupled model.

Studies by Rhys Parfitt and colleagues suggest that the mesoscale atmosphere couples with the ocean on the same spatial scales. Does the ability to represent ocean gradients in the stress calculation (and hence ocean forcing) on spatial scales of roughly twice as fine as the atmospheric gradients cause any problems? This would be a concerning choice of resolutions if the atmospheric boundary-layer responds substantially to the ocean, and hence would oddly impact the feedback on ocean forcing. The authors suggest that this concern might not be a serious problem because the stress in Fig. 1 does not seem to be responding to the currents. This interpretation may be an artifact of the color bar, but in this form the image suggests that the atmosphere is not responding to changes in the current, suggesting that the stress described in this paper is experienced by the ocean but not be the atmosphere, consistent with the seemingly strange use of a calculated neutral stress. Gaube et al. (2015) shows that the impacts of current gradients curl are greater than the impacts of stability, further supporting the concern that winds and currents are not coupled in this study.

We fully agree that Figure 1 is misleading. Indeed, because the impact of ocean currents on surface stress is much smaller than the impact of atmospheric winds, ocean current impact is not easily seen on the upper panels of figure 1. In the present revision, we have included a link to an animation (see line 125 in section 3), where it is easier to see the impact of currents on surface stress (paragraph starting on line 124). In addition, a figure is attached to this document (see figure S1 below) indicating that gradients of wind stress (wind stress curl and divergence) are much impacted by surface currents (in terms of relative vorticity and divergence).

Note that computations of momentum and heat fluxes at the air-sea interface take into account the differences between ocean and atmosphere resolutions. This is done using an exchange grid, created by the intersection of the ocean and atmospheric grids, which ensures complete conservation of momentum, heat, and freshwater flux across the air-sea interface. This is mentioned in the revised manuscript at the end of the 4th paragraph in section 2 (lines 92-105).

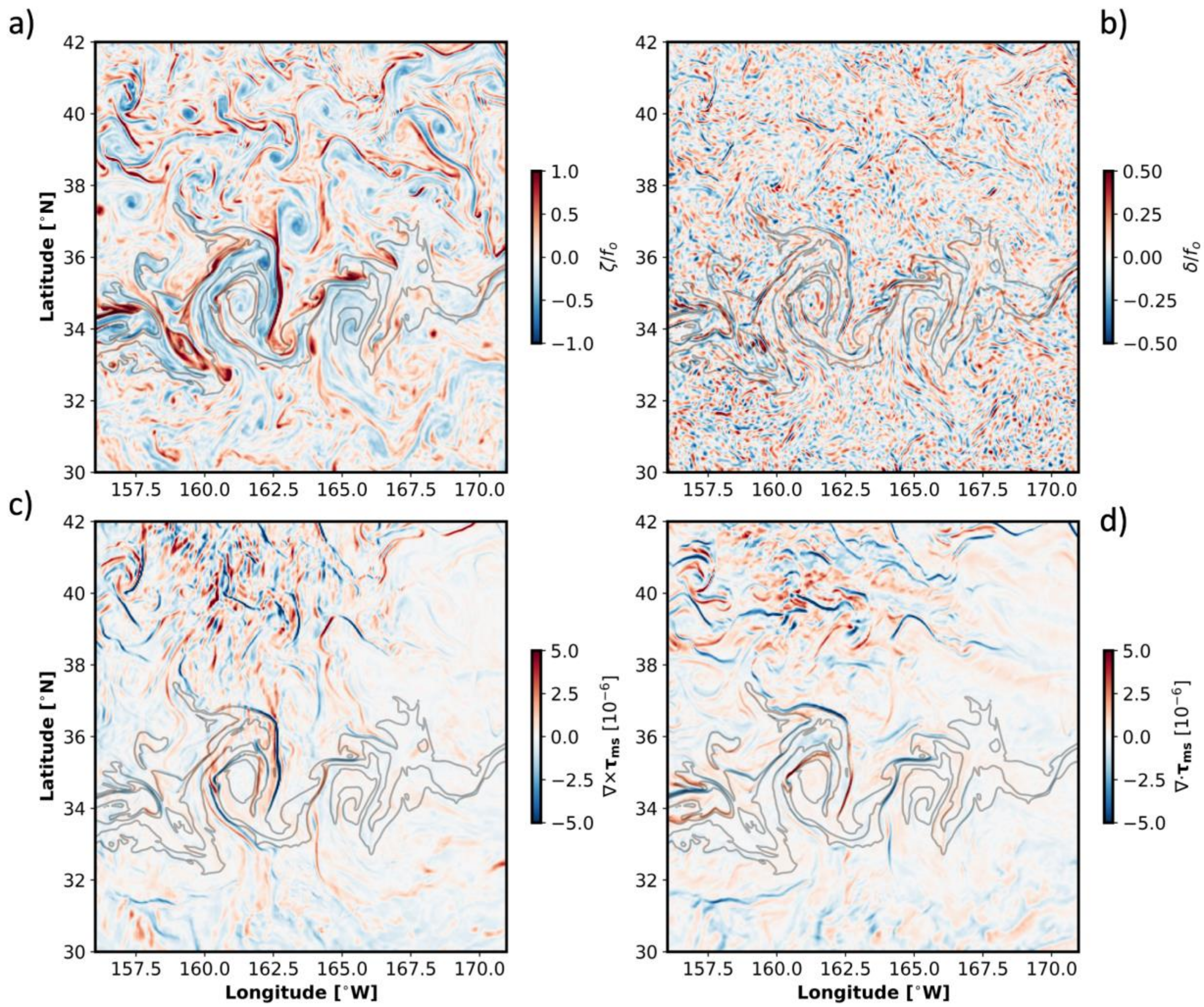

Figure S1. Snapshots of surface ocean relative vorticity (a), surface ocean divergence (b), wind stress curl (c), and wind stress divergence (d) in the Kuroshio Extension region. Gray contours show surface temperature isolines of 18, 19, and 20-deg Celsius.

Neither this paper nor the cited papers explain what information is exchanged between the ocean and atmospheric model, nor is it clear that the stress mentioned in this paper was experienced by the atmosphere, which might explain why the modeled stress values were not used in this study. In other words, it appears the atmospheric response to changes in the ocean is due only to thermodynamic changes in the ocean, as was typical with much of the early work on this subject. This raises the question of the importance of the missing atmospheric response and how those atmospheric changes would impact the ocean forcing. It would be fascinating to see this analysis carried out with a two-way coupled model that includes physics correctly coupling winds to currents, and hence changes to wind work. An evaluation of how atmospheric resolution impacts wind work would also be interesting, but is also clearly too much to ask. While this work demonstrates the importance of a high-resolution ocean on wind work and that it is important to consider surface relative winds in the calculation of wind work, it seems to largely neglect the importance of the atmosphere in the coupled ocean-atmosphere system, for which related changes in wind speed and direction could substantially impact wind work.

Thanks for this comment and sorry for not having detailed this important information, either in the present paper or in the previous ones. The revised manuscript now includes the methods used for the coupling between the atmosphere and the ocean and, in particular, the importance of the atmospheric response to ocean currents and how these atmospheric changes impact the ocean forcing. This is explicitly detailed in the 4th paragraph of section 2 (lines 92-105), as:

"The formalism of the coupling between the atmosphere and the ocean is classical and can be explained as follows. The Monin-95 Obukhov similarity theory-based parameterization of surface layer turbulence used to compute air/sea fluxes of heat, moisture and momentum is described in Helfand and Schubert (1995), and includes the effects of a viscous sublayer over oceans based on Yaglom and Kader (1974) that describes a resistance to enthalpy transfer that increases with surface roughness. The stability functions for unstable surface layers are the KEYPS equation of Panofsky et al. (1977) for momentum and its generalization for scalar quantities. For stable surface layers the stability functions are those of Clarke (1970) for momentum and heat. The100 ocean roughness is determined by a polynomial which is a blend of the algorithms of Large and Pond (1981) and Kondo (1975) for low wind speeds, modified in the mid-range wind regime based on recent observations in the Southern Ocean according to Garfinkel et al. (2011) and in the high wind regime according to Molod et al. (2013). Note that the ocean and atmosphere exchange momentum, heat, and fresh water through a "skin layer" interface which includes a parameterization of the diurnal cycle (Price et al., 1978). For the high-resolution simulation discussed here, the inertia of the skin layer is small. At last, Computations of momentum and heat fluxes at the air-sea interface take into account the differences between ocean and atmosphere resolutions. This is done using an exchange grid, created by the intersection of the ocean and atmospheric grids, which ensures complete conservation of momentum, heat, and freshwater flux across the air-sea interface."

Technical corrections:

1) Better explain how the models are coupled by detailing the variables that are exchanged.

2) Be clear about why a neutral drag coefficient is used when model stress is available.

In summary,

1) A new paragraph (4th paragraph) has been added in section 2 to explain how the models are coupled and in particular how the variables are exchanged.

2) We now specify in different parts of the manuscript that we do use wind stress and ocean current outputs from the coupled simulation. We also detail how the wind stress is calculated in our model (see again the 4th paragraph of section 2).

References:

Helfand, H. M., & Schubert, S. D. (1995). Climatology of the Simulated Great Plains Low-Level Jet and Its Contribution to the Continental Moisture Budget of the United States, *J. of Climate*, *8*(4), 784-806.

Yaglom, A., & Kader, B. (1974). Heat and mass transfer between a rough wall and turbulent fluid flow at high Reynolds and Péclet numbers. *Journal of Fluid Mechanics, 62*(3), 601-623. doi:10.1017/S0022112074000838

Large, W. G., and Pond, S. (1981). Open ocean momentum flux measurements in moderate to strong winds. *J. Phys. Oceanogr.*, 11, 324-336.

Kondo, J. (1975). Air-sea bulk transfer coefficients in diabatic conditions. *Bound.-Lay. Meteorol.* 9, 91-112.

Garfinkel, C. I., Molod, A., Oman, L. D., and Song, I.-S. (2011). Improvement of the GEOS-5 AGCM upon updating the air-sea roughness parameterization. *Geophys. Res. Lett.*, 38, L18702, doi:10.1029/2011GL048802.

Molod, A., Suarez, M. J., and Partyka, G. (2013). The impact of limiting ocean roughness on GEO-5 AGCM cyclone forecasts. Geophys. Res. Lett., 40, 411-415, doi: 10.1029/2012GL053979.

Price, J. F., Weller, R. A., and Pinkel, R. (1986). Diurnal cycling: Observations and models of the upper ocean response to diurnal heating, cooling, and wind mixing, *J. Geophys. Res.*, 91(C7), 8411– 8427, doi:10.1029/JC091iC07p08411.

Strobach, E., Klein, P., Molod, A., Fahad, A. A., Trayanov, A., Menemenlis, D., and Torres, H.: Local Air-Sea Interactions at Ocean Mesoscale and Submesoscale in a Western Boundary Current, Geophysical Research Letters, 49, 1–10, https://doi.org/10.1029/2021GL097003, 2022.