We have carefully considered review comments and revised the manuscript accordingly. Point-to-point responses to the review comments from the referees are as follow.

Referee comments are in blue colored fonts, and our replies in black.

Response to Referee Comments 1

This manuscript describes the implementation and initial results of simulations using a very high-resolution (1/32°) global ocean model, including waves and tides. This is an exceptional effort and adds to a small handful of similar very high-resolution simulations of the ocean which have been undertaken to date. As such, it is suitable for publication in GMD and deserves to be eventually published.

However, the manuscript is essentially the same as their previous version, gmd-2022-52, which I have reviewed twice and was ultimately rejected. The main problem with their previous version, and the current submission, is their persistent belief that the Bv parameterisation represents mixing by non-breaking waves, which it does not. Although they have removed three of the previous references to Bv as representing mixing by non-breaking waves, there are still several places in which this belief is retained (which I will detail below). Unfortunately, therefore, the paper must be rejected again.

Firstly, though, I include again my response to the authors’ previous comments about Bv: “I thank the authors for their response on the Bv parameterisation. However, no good reason has still been provided to support their assumption that w’ and l’ are in phase for the waves (to leading order). Even though, yes, the fluid motion is not fully irrotational, to first order, for a monochromatic wave train, w’ and l’ are in quadrature, so that <w’l’> = 0. However, in Qiao et al. (2010, Ocean Dynamics 60: 1339-1355), a single monochromatic wave is considered, and the key underlying assumption for Bv is made between equations 34 and 35 that w’ and l’ are in phase, so that <w’l’> is NON-ZERO. There is no justification given for this, either in the paper, or the authors’ response to my original point on this. Regarding the wave tank observations which are purported to support Bv, I would need to look at these very closely as a separate exercise, but would make the observation that mixing effects will result from the sides of the tank which may be difficult to allow for. And their third point that Bv has already been used in a range of leading models and can dramatically improve their mixed layers is irrelevant to the point in question: of course, the addition of a (possibly large) near-surface mixing term will result in the reduction of over-heating in the ocean surface through additional downward mixing.” The overall effect of Bv is to add an arbitrary, unphysical mixing term (which could be quite large) to the upper ocean.

The places which need to be corrected in the present manuscript, concerning Bv, are now:

l. 16. “The non-breaking surface wave-induced mixing (Bv) is proven to still be” should read “A previously described upper ocean mixing scheme (Bv) is proven to still be”

l. 252-270. Why is the discussion of the Stokes shear force introduced here, what impact does it have on anything being discussed? i.e. what does the epsilon parameter compare the Stokes shear force to? Is this intended to justify the inclusion of Bv in the model (ie by saying this will be important when the Stokes shear force is important)? Note that Bv does NOT represent the effect of mixing by non-breaking waves. Therefore, I cannot see the point of this discussion about the Stokes shear force, and the discussion in lines 252-270 should be deleted.

l. 305. “Prior to examining the effects of surface wave-induced mixing in the …” we are NOT examining the effects of surface-wave induced mixing here, only of the Bv parameterisation which does NOT represent breaking by non-breaking waves. This sentence must be changed to “Prior to examining the effects of the Bv mixing scheme in the …”

l. 411. Bv does NOT represent mixing by non-breaking waves, so change “the non-breaking wave induced mixing (Bv)” to “the mixing induced by Bv”

Author response:

After careful study of the comments, we are responding as follows:

First, we would like to thank the reviewer for your overall impression of our manuscript, I quote: “This manuscript describes the implementation and initial results of simulations using a very high-resolution (1/32°) global ocean model, including waves and tides. This is an exceptional effort and adds to a small handful of similar very high-resolution simulations of the ocean which have been undertaken to date. As such, it is suitable for publication in GMD and deserves to be eventually published.”

However, you also had persistent and specific misgivings expressed in multiple comments. Let us summarize the main concerns as follows:

1. Your main concern is our view on the Bv parameterization representing mixing by non-breaking waves, which you think it does not.
2. You also thinks that “the Stokes shear force introduced here, what impact does it have on anything being discussed? …. Note that Bv does NOT represent the effect of mixing by non-breaking waves. Therefore, I cannot see the point of this discussion about the Stokes shear force.”

3. "We are NOT examining the effects of surface-wave induced mixing here, only of the Bv parameterisation which does NOT represent breaking by non-breaking waves."

We can conclude that the reviewer does not object to the wave induced or enhanced mixing, but you are specifically objecting to the point that "non-breaking wave" could induce or enhance mixing.

It is obvious that ocean mixing involves turbulence. One of the main sources of oceanic turbulence is from breaking waves as reported by the classic studies by Thorpe (2005). However, they both pointed out that the turbulence so generated is confined to the upper most layer of the ocean with the thickness of the order of the wave amplitude. The main contribution of Bv is to propose a mechanism on how the turbulence, generated either by breaking waves or surface drift shear instability, propagates down, in a stably stratified fluid, to a depth that would influence the large-scale general circulation.

Mixing is an energy problem. Therefore, the role of the surface waves, being the most energetic motion on the ocean upper layer, should be seriously considered. This problem has been a hot subject for investigation as reviewed by Qiao et al (2016). This problem was the initiated by Phillips (1961). After many studies, the best effort was the detailed analysis by Teixeira and Belcher (2002, referred as TB thereafter). Phillips first proposed the wave orbital velocity could induce turbulence straining. TB carefully formulate the interaction mechanism using "**a single monochromatic wave**" as a model. The main conclusion of TB can be summarized (with some direct quotations) as follows:

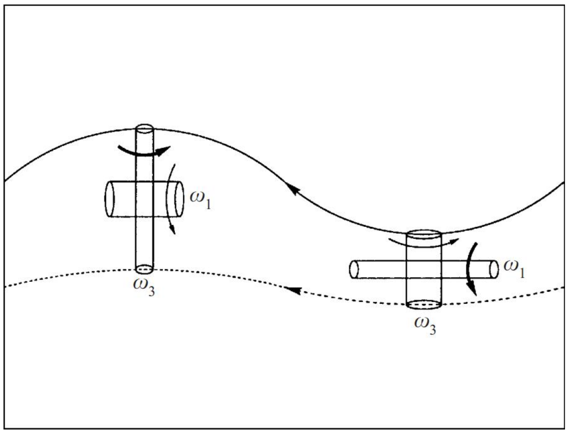
"A rapid-distortion model is developed to investigate the interaction of weak

turbulence with a monochromatic irrotational surface water wave. The model is applicable when the orbital velocity of the wave is larger than the turbulence intensity, and when the slope of the wave is sufficiently high that the straining of the turbulence by the wave dominates over the straining of the turbulence by itself. The turbulence suffers two distortions. Firstly, vorticity in the turbulence is modulated by the wave orbital motions, which leads to the streamwise Reynolds stress attaining maxima at the wave crests and minima at the wave troughs; the Reynolds stress normal to the free surface develops minima at the wave crests and maxima at the troughs (see Figure 6). Secondly, over several wave cycles ***the Stokes drift*** *associated with the wave tilts vertical vorticity into the horizontal direction, subsequently stretching it into elongated streamwise vortices, which come to dominate the flow (see Figure 8). These results are shown to be strikingly different from turbulence distorted by a mean shear flow, when 'streaky structures' of high and low streamwise velocity fluctuations* *develop."* The predicted vorticity distribution has been confirmed by open ocean turbulence measurement by Qiao et al (2016).

"The kinetical energy generation from the turbulence and wave interaction is estimated as

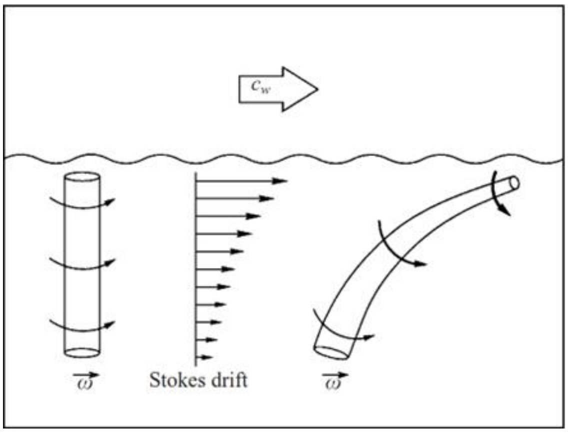
$$\frac{\partial E_K}{\partial t} \approx -2\overline{u_1 u_3} a_w^2 k_w^2 \sigma_w \approx -\overline{u_1 u_3} \frac{\mathrm{d}u_s}{\mathrm{d}x_3}(x_3 = 0). \quad (3.16)$$

This estimate of TKE production has a similar form to the term involving ***the Stokes drift*** *in the TKE equation (5.1) of McWilliams et al. (1997). It is as if there were a Stokes drift 'shear' that generates TKE.*



"Figure 6. Schematic diagram showing the vorticity stretching and compression induced by the orbital motion at the crest and at the trough of a surface wave, in a frame of reference travelling with the wave.

"The distortion of the turbulence by ***the Stokes drift*** *becomes clear after a considerable number of wave cycles.*



"Figure 8. Schematic diagram showing the tilting and stretching of the vertical vorticity carried out by ***the Stokes drift*** *of a surface wave over a number of wave cycles, in a fixed frame of reference.*

"The physical mechanism for the intensification of the streamwise vortices in the present model is the same as mechanism CL2 of Craik & Leibovich (1976) for the generation of Langmuir circulations. It involves the tilting of vertical vorticity by ***the Stokes drift*** *of the wave and its amplification as streamwise vorticity (figure 8). The difference is that the Craik–Leibovich formulation departs from an infinitesimal vertical vorticity perturbation arising from transverse variations of the wind-induced shear current, whereas in the present model, there is initially a finite and isotropic distribution of vorticity, associated with the turbulence. In both cases,* ***the Stokes drift*** *selectively amplifies the vertical vorticity component as streamwise vorticity."*

Clearly, the Stokes drift is the key element for wave-turbulence interactions. In fact, Stokes drift exists in non-breaking, inviscid, irrotational monochromatic wave train. Its existence, however, is in Lagrangian sense, in a particle following frame to reveal mass-transport, first derived by Stokes in 1847, and hence the eponymous drift. From the dynamic point of view, the existence and the influence of Stokes drift is very clear: Even irrotational and non-breaking wave causes energy, ***E***, propagation. In wave motion,

E=Mc,

Where ***E*** is the energy, ***M*** is the momentum and ***c*** the phase velocity. Momentum is mass multiplies by mass-transport velocity. As ***c*** is large, the momentum associated with wave motion is small, of a second order magnitude, but not zero, even the energy is large. In the TB study, the wave is a monochromatic, irrotational, and non-breaking surface water wave. For the key role is **the selective amplification by the Stokes**

drift (a.k.a. Stokes shear) on the long-term straining of the turbulence vertical vorticity to the horizontal direction, thus induce and enhance vertical mixing. There should be no doubt the wave involved is

Non-breaking,
Even monochromatic, and
Stokes drift plays the key role.

Our contribution in the parameterization of Bv is to express its effect in term of wave parameters. In parameterization of Bv, we selected a velocity scale and a length scale. The product of these quantities gives the “Eddy Viscosity” equivalence. In ocean dynamic, many phenomena involved are clearly unknown. Parameterization is the only way to move forward. For example, the eddy viscosity in the general circulation used is just to make the computation stable, with little physical justification. I wish the reviewer would take this point into consideration too.

Next, let us discuss the Bv parameterization scheme, which is also of a major concern of the reviewer. On this point, you stated: “I thank the authors for their response on the Bv parameterisation. However, no good reason has still been provided to support their assumption that w’ and l’ are in phase for the waves (to leading order). Even though, yes, the fluid motion is not fully irrotational, to first order, for a monochromatic wave train, w’ and l’ are in quadrature, so that <w’l’> = 0. However, in Qiao et al. (2010, Ocean Dynamics 60: 1339-1355), a single monochromatic wave is considered, and the key underlying assumption for Bv is made between equations 34 and 35 that w’ and l’ are in phase, so that <w’l’> is NON-ZERO. There is no justification given for this, either in the paper, or the authors’ response to my original point on this. Regarding the wave tank observations which are purported to support Bv, I would need to look at these very closely as a separate exercise, but would make the observation that mixing effects will result from the sides of the tank which may be difficult to allow for. And their third point that Bv has already been used in a range of leading models and can dramatically improve their mixed layers is irrelevant to the point in question: of course, the addition of a (possibly large) near-surface mixing term will result in the reduction of over-heating in the ocean surface through additional downward mixing.” The overall effect of Bv is to add an arbitrary, unphysical mixing term (which could be quite large) to the upper ocean.

The discrepancy on the parameterization scheme should be easily resolved. The key is whether “for a monochromatic wave train, w’ and l’ are in quadrature, so that <w’l’> = 0,” or “w’ and l’ are in phase, so that <w’l’> is NON-ZERO.” In the original Qiao et al (2010) paper, there is no <w’l’> term. Instead, the term should be <w’$_{3w}$l$_{3w}$>. The difference looks subtle, but it is critical. In Qiao et al (2010), it was carefully stated, and we quote that

*"We use an analogy to **the Prandtl mixing length theory to parameterize** the momentum mixing induced by wave motion. ... For ocean surface wave processes, **we assume that the mixing length l_{iw} is proportional to the range of the particle displacement in the i-th direction**. We need to note that the concern of the expression **u'_{iw} here is not the mathematical derivation, but a concept and assumption of equivalent scales**. u'_{iw} should be understood as **the increment of the wave motion velocity at the spatial interval of l_{iw}** in the i-th direction."*

Therefore, u'_{iw} is precisely the Stokes drift velocity, and l_{iw} is the particle excursion range. They are both scales for the wave motion, NOT MATHEMATICAL DERIVATION; obviously, there is no phase relationship at all. "The increment of the wave motion velocity at the spatial interval of l_{iw}" is clearly in the Lagrangian sense, or the Stokes drift. As stated above, Stokes drift exists for non-breaking, irrotational, and even Monochromatic water waves.

NB: Please also note the difference between τ_{wwij} and τ_{wcij}: These represent two types of Reynolds stresses: τ_{wwij} are wave related stresses, the pure wave quantities and could be derived mathematically. For irrotational wave field, their values are zero when the velocities are in quadrature. τ_{wcij} are wave current interactions that include the wave induced Stokes drift and ambient current field. We will concentrate in the wave related Stokes drift here, which exists only in Lagrangian frame. As Qiao et al (2010) was formulated in Eulerian frame, Stokes drift could not be derived and could only be parameterized. However, this much is clearly stated: " u'_{iw} should be understood as the increment of the wave motion velocity at the spatial interval of l_{iw} in the i-th direction;" therefore, the velocity is exactly the Stokes drift.

Now, let us turn to some of the minor points:

1. This is NOT a fully-coupled model as the surface waves are computed offline and do not interact with the ocean circulation fields as they evolve, so change "surface wave-tide-circulation fully coupled model" to "surface wave-tide-circulation coupled model".

We set up a fully coupled model. Due to the limitation of computer resources, we run the wave model offline. Following your review comments, we remove the word "fully".

2. Regarding the wave tank observations which are purported to support Bv, I would need to look at these very closely as a separate exercise, but would make the observation that mixing effects will result from the sides of the tank which may be difficult to allow for.

The effect of side of the tank was a critical issue when that paper was first submitted for publication. The order of magnitude argument could help here. It suffices to say that the molecular viscosity is many orders smaller than the Bv. Additionally, other experiments on the dissipation rate measurement in the open ocean (i.e. Sutherland, 2013) also support the validity of Bv. There are no sides in the ocean, of course.

We thank the reviewer in pointing out many detailed syntax and expression anomalies. We have followed all these suggestions in most places.

Finally, we hope these explanations on the non-breaking, irrotational and even monochromatic wave could contribute to enhancing mixing would resolve the reviewer's concerns.

Reference:

Huang, C. J., Qiao, F., Dai, D., Ma, H., & Guo, J., 2012: Field measurement of upper ocean turbulence dissipation associated with wave-turbulence interaction in the South China Sea, J. Geophys. Res., 117, C00J09, doi:10.1029/2011JC007806

Phillips, O.M., 1961: A note on the turbulence generated by gravity waves. J Geophys Res 66:2889–2893. doi:10.1029/JZ066i009p02889

Qiao F, Yuan Y, Deng J, Dai D, Song Z., 2016 Wave–turbulence interaction-induced vertical mixing and its effects in ocean and climate models. Phil. Trans. R. Soc. A 374:20150201. http://dx.doi.org/10.1098/rsta.2015.0201

Sutherland G., Ward, B. & Christensen, K. H., 2013: Wave-turbulence scaling in the ocean mixed layer. Ocean Sci., 9, 597–608, doi:10.5194/os-9-597-2013

Teixeira, M. A. C. And Belcher, S. E., 2002: On the distortion of turbulence by a progressive surface wave. J. Fluid Mech. 458, 229-267

Thorpe, S. A., 2005: The Turbulent Ocean. Cambridge University Press, New York

Other comments which should be addressed are as follows:

l. 22. Need to explain what are the "unbalanced motions" referred to here? Are they the motions induced by internal tides for instance?

Author response: As introduced by Rocha et al. (2016) and Chereskin et al. (2019), the "unbalanced motions" referred to the geostrophically unbalanced motions, i.e. ageostrophic motions, e.g. internal tides and inertia-gravity waves. This has been added in the revision in line 22.

Reference:

Chereskin, T. K., Rocha, C. B.,Gille, S. T., Menemenlis, D., and Passaro, M.: Characterizing thetransition from balanced to unbalanced motions in the southern California Current, Journal of Geophysical Research: Oceans, 124, 2088–2109. doi:https://doi.org/10.1029/2018JC014583, 2019

Rocha, C. B., Chereskin, T. K., Gille, S. T., and Menemenlis, D.: Mesoscale to submesoscale wavenumber spectra in Drake Passage, J. Phys. Oceanogr., 46(2), 601-620, doi:10.1175/JPO-D-15-0087.1, 2016.

l. 121-125: In the high resolution case, the wave and ocean circulation model are coupled offline, so the wave field cannot interact with the ocean circulation fields as they change (ie because the wave fields are previously saved as fixed data files). Need to explain this fully here.

Author response: The explanation has been added in the revision. As shown in Fig. 16b, the model is surface wave and ocean circulation models fully coupled, and the ocean current can modulate the surface wave height. For computer efficiency, we turn off the coupling pro tempore.

l. 138-139. The Bv field applied to the high-resolution (1/32°) model is calculated from an online-coupled lower resolution model (1/4°). The lower resolution model will have different circulation fields (i.e. the currents will be slower and broader, and probably in different places), so the Bv field applied to the high-resolution model will not be appropriate. What difference will this make to the high-resolution results?

Author response: We totally agree with you that the same resolution of surface wave and ocean circulation models is the best choice. However, in current stage, we have difficulty to run a 1/32° wave model due to insufficient computational resources. Our group tested daily and monthly averaged Bv in a coarse resolution ocean circulation model, the difference is not big (Zhao et al., 2012), and both can much improve the upper ocean simulation. So, we can deduce that the coarse resolution Bv of 1/4° can work for FIO-COM32, although not perfect.

Reference:

Zhao, C., Qiao, F., Xia, C., and Wang, G.: Sensitive study of the long and short surface wave-induced vertical mixing in a wave-circulation coupled model, Acta Oceanologica Sinica, 31(4), 1-10, doi:10.1007/s13131-012-0215-y, 2012.

l. 160 The wave-tide-circulation model is NOT fully coupled since the waves are run offline – change this to “In EXP2, wave-tide-circulation **coupling** is enabled”.

Author response: Done.

p. 8 and fig.s 4 and 5. What longitudes are the ICRE diagnostics defined over (presumably those in the figures)?

Author response: The longitudes of the ICRE diagnostics are defined as those in the Figures 4 and 5, i.e., 115° E-160° E and 90° W-45° W respectively. The explanation is added in the revision.

p. 8 and fig. 5. How is the ICRE defined for the Gulf Stream in the 1/10° model, since the contour used for its definition does not exist eastwards of about 63°W?

Author response: The ICRE defined for the Gulf Stream in the 1/10° model is identical to that of 1/32° model, hence the total area misfit eastwards of about 63°W of the 1/10° model is quite large due to that the 1/10° model is not able to reproduce the deep penetration of the Gulf Stream into the Atlantic ocean. As a result, in Fig. 5 the ICRE of the 1/10° model is much larger than that of 1/32° model.

l. 293-294: how do you justify the claim that "the global tide accuracy is sufficient to support" …. " the investigation of tide-circulation coupled processes" given that the errors in fig 8g are in excess of 25cm over large regions of the ocean?

Author response: As shown in Fig. 8f the overall pattern of the global tide of M_2 agrees well with the TPXO9. This paper focus on the effect of the global tide-circulation coupling not the accuracy of the global tide systems. We refined the sentence accordingly.

Fig. 12 (d) shows the L_{SML} not the MLD as specified in the caption.

Author response: Thanks for pointing out this, the caption is corrected in the revision.

Fig.s 13 and 14 and discussion of the inclusion of internal tides. This was nice to see and the most useful part of the paper. The inclusion of the internal tides appears to add SSH variability between 70-250 km and increase the amount of energy in the spectra (fig. 14) in the more quiescent tropical regions, so that the spectral slope is reduced and more in agreement with the observations. However, fig. 13 clearly shows that the internal tidal field at the surface is too strong, probably because of the lack of bottom dissipation. Can the authors comment on how to reconcile these two aspects, ie if the internal tidal field was realistic, what would the effect on the spectral slopes be (e.g. in fig. 14(e)).

Author response: Although the slopes of EXP2 (Fig. 14c) are more in agreement with the observations than that of EXP1 (Fig. 14b), there is still some discrepancies, especially in the low latitude regions of Atlantic. In this region, the slope of EXP2 is apparently more flat than the observations, which may indicate the internal tide here is too strong. More realistic internal tidal field may further improve the agreement of the model and observations. This also remind us that a proper dissipation scheme of the internal tide and its adaption with the traditional viscosity schemes for OGCM need to be developed in the future.

l. 390. Replace "it displaces by clear discrete beams" with "these are shown by clear discrete beams"

Author response: Thanks, it is replaced in the revision.

l.395: what are the unbalanced motions – presumably the internal tides, IGWs etc?

Author response: The "unbalanced motions" referred to the ageostrophic motions, here mainly internal tides and inertia-gravity waves. The explanation has been added in the revision.

Fig. 15. Please say which solid black line is the tenth normal mode, and which is the first?

Author response: The caption has been modified. "Solid black curves denote the dispersion relations for inertia-gravity waves of the first (upper) and tenth (lower) vertical modes. "

Fig. 15 caption is wrong: e.g the box centred at 138°E, 26°N is shown in panels (a), (b) and (c) and not in panels (a) and (b) as in the caption, with similar comments for the other rows of panels.

Author response: Thanks for the comments, the caption has been corrected in the revision.

l. 402. This is NOT a fully-coupled model as the surface waves are computed offline and do not interact with the ocean circulation fields as they evolve, so change "surface wave-tide-circulation **fully** coupled model" to "**surface wave-tide-circulation coupled model**".

Author response: This has been changed in the revision.

l. 438-439: "we **clearly** show surface wave-tide-circulation coupling can dramatically improve our simulations" is not true since the surface waves are not fully coupled. So delete the word "clearly".

Author response: Done.

Minor corrections to the English (up to line 147) are as follows (there are many more such corrections which could be made, so a thorough read-through by a native English speaker would be of benefit here):

l. 33 Further improved resolution has a significant **impact**

l. 48 The most **uncertain** term

l. 51 proposed **an** upper ocean mixing scheme of Bv

l. 61 in many **coarse** resolution

l. 64 **coarse** and high resolution

l. 97 configurations and **design**

l. 147 baroclinic experiments **so** that

Author response: Thanks for these corrections, they have been corrected in the revision. We have turned to a native English speaker to polish the draft.