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Geoscientific Model Development Discussions

Interactive comment on "Development of a 2-way coupled ocean-wave model: assessment on a global NEMO(v3.6)-WW3(v6.02) coupled configuration" *by* Xavier Couvelard et al.

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Review from Oyvind Breivik

First, the authors would like to sincerely thank the reviewers for their careful reading of the paper and their valuable comments to the manuscript and helpful suggestions. We further clarified several issues raised during the review process. Please find attached our revised paper and below a summary of how we responded to the comments. Our comments are reported in color in the text below.

General comments

- This paper describes a two-year experiment with a coupled WW3-NEMO setup. The experiment builds on earlier experiments by Breivik et al (2015) and others who investigated the impact of waves on the mixed layer. The paper is well written and clear.

- First, the change from a Dirichlet to a Neumann condition for the TKE flux should be discussed in more detail. It is not clear to me that comparing against an uncoupled run with a Dirichlet conditon is clean. A separate experiment should be run where the uncoupled model ingests a flux in the Neumann form, or alternatively a coupled run where the Dirichlet condition is used to communicate the TKE flux from WW3. That's a good point. From our point of view as soon as a coupling with a wave model is performed the surface boundary condition should systematically be in the Neumann (flux) form because the wave model naturally provides its information through a flux. In the uncoupled case, it is less clear what should be done. In Mellor and Blumberg (2004)[1] the authors consider both a Dirichlet condition (such that $e_{\mathsf{sfc}} = (15.8\alpha_{\mathsf{CB}})^{2/3} u_\star^2$ (their eq. (10))) and an equivalent Neumann condition ($K_e \partial_z e = 2\alpha_{\mathsf{CB}} u_\star^3$ (their eq. (3))) and they mention in their Sec. 7 that *"numerical solutions using Eqs. (1), (2), and "* a Dirichlet condition *"instead of"* a Neumann condition *"reproduced all of the calculations in Figs. 1, 3, and 4"*. Based on their finding, we preferred to focus our efforts in terms of additional simulations toward the clarification of the role of the Axell parameterization on our numerical results. But this would definitely be worth the effort to redo the Mellor & Blumberg experiment with NEMO to check if solutions are indeed insensitive to the nature of the TKE surface boundary condition in uncoupled cases.

[1] Mellor, G. and A. Blumberg, 2004: Wave Breaking and Ocean Surface Layer Thermal Response. J. Phys. Oceanogr., 34, 693–698.

It should be emphasized that a Neumann (flux) boundary condition for TKE has been used earlier in various studies of wave-ocean coupling (e.g. Michaud et al., 2012) and is not something specific to our approach.

- The integration period is rather short. I think the authors should investigate whether there is sufficient convergence after just two years.
  We are not necessarily looking for convergence but we considered it was enough to illustrate the fact that our developments were actually producing the expected results. Integrating longer in time could also lead to drifts in the stratification independently from the wave effects and could thus distort our interpretation. We are lucid about the fact that we can not draw any conclusion on the long term impact of waves effect at global scales, a different experimental setup would be needed to do that.

- The Langmuir experiment is very interesting as it promises a way forward from the ETAU hack. I would like to see a quantification of how much changing from parameterized Stokes drift (1.6% of the wind speed) to a Stokes drift taken from WW3 gives you. I suspect the most important thing you've done is to change the factor from 0.15 to 0.30. Further on the Langmuir experiment, you don't seem to improve the Stokes drift discussion is interesting.
  Besides the calibration of the parameter c_{LC} we have also revised the way the input of Stokes drift contributing to Langmuir turbulence is computed. See in Fig. 1 at the end of this document the annual mean of the surface Stokes drift $\|\mathbf{u}^s(\eta)\|$ vs the surface Stokes drift as parameterized in the uncoupled case (i.e. $0.377\sqrt{\|\tau\|/\rho_0}$). On average those two quantities are significantly different which partially explain the stronger role played by the LC parameterization in coupled simulations vs uncoupled ones.

- I suggest you read the appendix of Li et al (2017) where there is a description of the finite volume form of the profile by Breivik et al (2016).

  Thanks for pointing this out to us. We now make reference to App. A of Li et al (2017) when introducing the finite-volume form of the Stokes drift profile. It seems that Wu et al. (2019) also considered such approach.

- Also, the recent paper by Wu et al (2019) discusses the combined impact (quite small!) of the Coriolis-Stokes force and the Stokes drift on tracer advection.
  We were aware of this paper but we forgot to mention it, it has been added to the revised manuscript. It is indeed well know that you should have both Stokes-Coriolis and the effect of Stokes drift on tracer/continuity equation all together otherwise it does not make any sense. Indeed, because of the geostrophic balance, the Stokes-Coriolis force must be counterbalanced by a pressure gradient which accounts for the Stokes drift. This combined impact seems indeed rather small also in our numerical experiments.

- Finally, a quantitative assessment of the relative impact of the various wave-induced processes is needed in order to give the reader an idea of their importance. This applies to the description in Sec 4.2.3 as mentioned below.
  In the revised manuscript an additional numerical experiment has been done to further assess the relative role of the different changes (see Tab. 2). This additional simulation allows to separate the impact of the modifications in the TKE scheme from the impact of the Langmuir cell parameterization. It suggests that for a $1/4^{\circ}$ resolution the additional terms in the wave-averaged primitive equations have very small impact and that most of the improvements we see are related to the change in wind-stress, TKE closure and LC parameterization. We believe that Fig. 10 provides some hints on the relative role of each processes.

- Cost: You have run WW3 on half the resolution of NEMO at 20% added cost. Have you considered the added benefit of running the models on similar resolution? I presume this would cost more than twice the standalone NEMO run, so I sympathize with your decision, though.

Considering a linear scaling going from $1/2°$ to $1/4°$ would increase the cost of the wave model by 8 and thus the added cost would be 160%. Besides the associated cost, our study was motivated by operational purposes in the framework of CMEMS, that's the reason why we had to keep a reasonable added cost for the coupling with the waves.

- All told, I would say that after major revision (rerunning the experiments with Dirichlet or Neumann to make a clean comparison) and assessment of the relative importance of the wave-induced effects, this paper should be acceptable for publication in GMD.
  Our study provides in several ways a good starting to allow a clean separation between various effects. We could imagine refining it by implementing online diagnostics to assess the relative role of the different in the prognostic equation for TKE. This is however beyond the scope of this particular study and we think that a $1/4°$ resolution global oceanic configuration is probably not the adequate simulation to do that.
  Moreover, just like the example you give below for the combined impact of Stokes-Coriolis and the Stokes drift in tracer/mass equations the modifications we make are often not independent from each other and trying to test each modification individually may break some balances. We would have liked to prepare a figure showing the difference between $\Phi_{\mathrm{oc}}/\rho_0$ (the TKE flux from the wave model in the coupled case) vs $2\alpha_{\mathrm{CB}}u_\star^3$ (the TKE flux in the uncoupled case following Craig & Banner) because we did not have enough time to do so because additional experiments would have been needed (Φ_{oc} was not stored in our standard outputs).

Detailed comments

- Fig 2 is a mess. Please explain in detail what is shown in the different panels and refer to those panels in the text. The figure headings are illegible. I am also

surprised by the huge difference in average wave height and would like to see a more in-depth discussion of why this is so.
  Sorry for Fig. 2, the rendering of this figure is perfectly fine on a mac computer but there is something wrong on other operating systems. We have corrected this issue. On this figure we show the seasonal averaged of significant wave height as computed by WW3 on the left panels and the differences between the Charnock parameter computed by WW3 and the standard constant value. Such large deviations of the Charnock parameters from the constant value 0.018 have also been observed for example by Pineau-Guillou et al. (2018) [2].

- 4.2.2 It is interesting that you have rewritten the Dirichlet conditon to a Neumann condition for the TKE flux. However, I think you should also investigate how this affects the results as you compare against an uncoupled run with a Dirichlet condition.
  Please see our comments on this aspect earlier in our reply.

- 4.2.3 The impact on MLD and SST does not separate between Langmuir, TKE flux, and stress. This needs to be done.
  As mentioned above, an additional numerical experiment has been done to separate those 3 effects and results are shown in Fig. 10.

- I was meant to say about the Langmuir mixing that you don't seem to improve the MLD much, but this is part of the general comment I was making that you need to separate the impact of the various processes.
  Based on the new version of Fig. 10, the effect of the parameterized Langmuir mixing is not significantly less than the effect of the revised TKE scheme. The

[2] PineauâĂŘGuillou, L., Ardhuin, F., Bouin, M.-N., Redelsperger, J.-L., Chapron, B., Bidlot, J.âĂŘR. and Quilfen, Y. (2018), Strong winds in a coupled wave–atmosphere model during a North Atlantic storm event: evaluation against observations. Q.J.R. Meteorol. Soc, 144 317-332

figure-1.pdf

Fig. 1. Annual average of surface Stokes drift module $\|\mathbf{u}^s(\eta)\|$ (top), of the portion of the Stokes drift aligned with the wind (middle), and of the surface Stokes drift as parameterized by $0.377\sqrt{\|\tau^{\text{oce}}\|/\rho_0}$ in the uncoupled case (bottom)

Axell parameterization is necessary to make the MLD more consistent with observations.