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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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A review of GMD-20180189: Development of a 2-way coupled ocean-wave model: assessment on a global NEMO(v3.6)-WW3(v6.02) coupled configuration

by Xavier Couvelard, Florian Lemarié, Guillaume Samson, Jean-Luc Redelsperger, Fabrice Ardhuin, Rachid Benshila, and Gurvan Madec

Firstly, I need to apologize for the very late review.

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Discussion paper

Summary

This MS describes the implementation of a 2-way coupled wave–ocean model, involving v3.6 NEMO and Wavewatch 3 v6.03. This model aims to include most of the wave impacts on the ocean. It includes

1. 2-way coupling of the ocean and wave models using the OASIS3-MCT coupler. Surface ocean velocities are passed to the wave model, and various wave properties, including surface and depth-integrated Stokes drift, energy dissipation rate, wave-ocean stress and Charnock parameter are passed to the ocean model.

2. Calculation of the wind-stress exerted by the atmosphere (τ^{atm}) using a value of the Charnock parameter output from the wave model, which is itself forced by a stress $\tau^{\mathrm{atm}}_{\mathrm{WW3}}$ calculated from the 10-m winds using an approximate, constant (independent of atmospheric stability or Charnock number) drag coefficient. The surface-stress that drives the ocean is then $\tau^{\mathrm{atm}} - \tau^{\mathrm{atm}}_{\mathrm{WW3}} + \tau^{\mathrm{oce}}_{\mathrm{WW3}}$, where $\tau^{\mathrm{oce}}_{\mathrm{WW3}}$ is the rate of momentum transfer from the waves to the ocean by breaking waves.

3. Wave-influence terms added to the momentum and tracer advection equations that are linked to the Stokes drift. The vortex-force term is included in the momentum equation, as well as the Stokes-Coriolis force included in previous implementations.

4. Use of the wave energy dissipation rate Φ_{oc} as a flux (Neumann) surface boundary condition for the TKE that is prognosed by the TKE model that is used here to specify vertical eddy diffusivities and viscosities.

5. A revised version of Axell's (2002) parameterization of Langmuir turbulence for the TKE model, with a doubling of the TKE source associated with vertical Langmuir turbulence imposed within an upper Langmuir-influenced part of the surface boundary layer.

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The effects of including these wave impacts on the ocean are briefly examined through four 2-year simulations that introduce subsets of the above enhancements (1)–(5), involving v3.6 NEMO run at 1/4° resolution (ORCA025) coupled with WW3 run at 1/2° resolution. With the full model with all 5 additions, mixed-layer depths are found to be improved in the Southern Ocean, particularly in southern summer, when the standard model gives summer mixed-layers that are too shallow.

Major Comments

The authors have done a lot of work here in producing a coupled version of NEMO with WW3. The explanation of the extra terms added to NEMO is full, very much in the spirit of a GMD contribution, and it is good to see that the code is indeed publicly available. However, shortcuts have been taken e.g. the use of a neutral drag coefficient independent of Charnock number to estimate the atmospheric stress transferred into the waves, while the total atmospheric stress is separately calculated and depends on Charnock number and atmospheric stability.

The testing of the modifications with 2-year runs is rather cursory, but I guess that the intention of that short testing period is more to check that the code is basically OK rather than to optimize the parameterizations. However, there really should be a test run that includes the changes to the TKE model coming from the flux condition using wave-dissipation energy [(4) above] but not including the Langmuir cell parameterization.

The paper generally seems a bit rushed, and the English while being perfectly readable, is not great; there are many extra s's where there should be none, etc.

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Recommendation

This MS should be accepted, subject to minor revisions.

Detailed Comments

p2, l39 Should refer to Lu et al. (2019).

p3, Eqs. (1)–(4) Various w should be ω.

p3, Eqs.(1)–(4) Please define p_h and p_s.

p3, l78 τ^{oce} is not strictly the wind stress; it is that part of the stress that drives the ocean rather than developing the wave field.

p3, l78 Please explain “the dynamic boundary condition imposing the continuity of pressure at the air-sea interface”

p3, l80 $\omega(z = -H) = 0$. This is only true for terrain following coordinates, not for a generalized coordinate.

p4, Eqs.(7)–(8) Please define p^J and p^{FV}. I assume p^J is the J that is only significant in shallow water, defined in eq. (20) of Bennis et al., (2011). If so, then presumably p^{FV} represents the term $\frac{1}{2}[(\mathbf{u} + \mathbf{u}^s)^2 - \mathbf{u}^2]$ found e.g. on the RHS of Eq. (2) of Suzuki and Fox-Kemper (2016). This term would seem to scale with the vortex force term. Can the authors justify its neglect?

p4, Eq.(12) The “wave pressure vector” seems a little odd. It would be more natural to make $\mathcal{W}_{\mathsf{Prs}}$ the column vector of x- y- and vertical gradients, especially given that in p5, l118 you refer to “the additional wave- induced barotropic forcing terms corresponding to the vertical integral of the ... $\mathcal{W}_{\mathsf{Prs}}$”

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p5, Eq.(13) How do you decompose baroclinic and barotropic contributions to bottom drag when using non-linear bottom drag, as you do in these experiments (p13, l342)?

p6, Figure 1 You seem to have evaluated the primitive $\mathcal{I}_{\mathrm{B14}}$, as you plot it out here. Why is it not written out explicitly like $\mathcal{I}_{\mathrm{B16}}$, which is set out at the bottom of p5?

p6, l140–141 This is a nice point. But note on l141 that "summed to ω" should summed with ω. More importantly, please briefly explain how $\omega + \omega^s$ is set; Eq. (10) looks more like a prognostic equation for e_3 than a diagnostic equation for $\omega + \omega^s$.

p9, l211 Should be $|\mathbf{u}^s_{\mathrm{LC}}| \propto \sqrt(|\tau|)$

p11, l272–73 "most of the momentum flux going into the waves is quickly transferred to the water column through wave breaking (we call this fraction τ^{oce}". Do you mean $\tau^{\mathrm{oce}}_{\mathrm{WW3}}$?

p11, l276 The Charnock number is used to give the surface roughness that presumably in fact allows wind to drive waves. Why is the WW3 model then not forced by a drag coefficient that includes this effect? Also, why is the stress that drives the WW3 waves not stability-dependent?

p11, Eq. (21) This equation does seem to ensure that momentum is conserved, although I guess $\tau^{\mathrm{atm}} - \tau^{\mathrm{atm}}_{\mathrm{WW3}}$ may be much bigger than it should be.

p11, l289–291 I understood that the situation is not that clear, especially for eddy-resolving models, and that some consideration *does* need to be paid to the ocean current when calculating wind stress. E.g. the last sentence of Renault et al. (2018) states: " A simulation without current feedback—by overestimating the eddy amplitude, lifetime, and spatial range.."

p13 Much of the first para seems to describe the wave model rather than its specific setup, so might fit in better into section 3.1.

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p13, , l337–8 “The numerical options are the one commonly chosen by the Drakkar group”. This is a bit confusing; please indicate which of the options described here are the Drakkar options, and whether there are other option choices described in the Drakkar website that are not described here.

p13, , l342 How does the lateral diffusivity vary away from the equator?

p14, l348–351 More specific details and/or references are required here. Is it only solar forcing that is given a diurnal cycle? A reference is required for the data correction to ensure consistency.

p14, section 4.1.3 There should be a test experiment with ST_CPL + changes to the TKE scheme but no Langmuir parametarization, to see whether the new TKE boundary condition makes any difference.

p14, section 4.1.3 Please specify the initial conditions. Is it a spun up run of some standard NEMO setup? If so, give details.

p15, Figure 2 Various random missing letters on panel titles.

p16–p17, section 4.2.2 Given the amount of space devoted to the extra TKE injection (& 2 figures!), it really does seem strange that no run with ST_CPL + changes to the TKE scheme but no Langmuir parameterization has been presented.

p17–p19, section 4.2.3 Give reference for ARGO MLD climatology, and specify the MLD criteria used in model and climatology.

p17–p19, section 4.2.3 Maps of discrepancies of MLD from ARGO, and zonal-average MLDs would be more convincing than the MLD pdfs.

p20, l444–445 “an increased heat content during winter leading to higher SST during summer.” Is this the wrong way round?

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p29, appendix B It is not easy to see which of these solutions is best. On p9, l25–16, you write " Based on single-column experiments detailed in App. B, we find that parameter values in the range 0.15 – 0.3 provide satisfactory results compared to LES simulations" Where are these LES simulation results?

—George Nurser

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