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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 George Nurser

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.

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.
  Thanks for this encouraging comment, the code is indeed publicly available and the developments are now in the process of being incorporated in the official NEMO release within the H2020 IMMERSE project.

- 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.
  From your remark, it seems that our description of how the surface wind-stress is computed was not clear enough. The computation of the wind-stress in the wave model and in the oceanic model are both function of the Charnock parameter computed by the wave model. As mentionned in Sec. 3.2, in the wave model the general formula used is $\tau_{\mathrm{ww3}} = \rho_a C_{\mathrm{DN}} \|\mathbf{u}_{10}^{\mathrm{atm}}\| \mathbf{u}_{10}^{\mathrm{atm}}$ where $C_{\mathrm{DN}} = \left(\frac{\kappa}{\ln\left(\frac{z}{z_0}\right)} \right)^2$ where z_0 depends on the Charnock parameter. The only difference between τ_{ww3} and the stress computed in the oceanic model is that the latter accounts for atmospheric stability. Note that what you call a "shortcut" is what is actually done in all coupled ocean-wave models as none of them guarantees energetic consistency (this point is often swept under the carpet in publications). Let us mention that :

  - Wave models in "forced mode" do not have any information on atmospheric temperature/humidity or SST which explains why they neglect atmospheric stability in the wind-stress computation.

  - The solution of wave models is very sensitive to wind-stress and our wave configuration has been designed and validated in forced mode with neutral drag coefficient. We tried to run WW3 with the same bulk formulation as NEMO but the quality of the wave solution was drastically deteriorated doing so.

We tried to further clarified those aspects in the revised manuscript.

- 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.
Following your suggestion we have added a new case TKE_CPL in Tab. 2 which includes all ingredients but the Langmuir Cells parameterization. The results thus obtained are showed and discussed in Fig. 10 and Sec. 4.2.3.

- 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.
Sorry for that, we tried to correct as much as possible these issues.

Detailed Comments

- p2,l39 Should refer to Lu et al. (2019).
We believe that instead of Lu et al. (2019) it should be Wu et al. (2019) ? A reference to Wu et al. (2019) has been added, in particular they also compute the Stokes drift as a "layer-averaged Stokes drift profile" based on the Breivik et al. (2016) profile. It is really not clear from their paper but it seems that they

introduced in NEMO a Neumann (flux) boundary condition for the TKE equation. Thanks for pointing out this reference to us.

- p3,Eqs. (1)–(4) Various w should be ω.
It has been corrected in subgrid scale terms

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

- 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.
Indeed you are right, it has been changed.

- p3,l78 Please explain "the dynamic boundary condition imposing the continuity of pressure at the air-sea interface"
We do not necessarily see what should be explained here. There must be no pressure jump at the air-sea interface, continuity of pressure translates into $p = p_{\mathrm{atm}}$ at the interface with p_{atm} the atmospheric pressure at sea surface.

- p3,l80 $\omega(z = -H) = 0$. This is only true for terrain following coordinates, not for a generalized coordinate.
Since ω is the dia-surface velocity component, at the lower boundary the no-normal flow boundary condition should read $\omega_{\mathrm{bot}} = 0$ which is equivalent to $\mathbf{u} \cdot \mathbf{n} = 0$. We do not understand the issue here, the continuity equation equation integrates starting from $\omega = 0$ even with geopotential 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 $\frac{1}{2}\left[(\mathbf{u}+\mathbf{u}^s)^2 - \mathbf{u}^2\right]$ 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?

Thanks for raising this issue. First we tried to clarify the notations in the paper and the way the additional wave related terms are introduced. In Suzuki and Fox-Kemper (2016) (SFK16) the Craik-Leibovich (CL) equations are used while our implementation relies on the more general wave-averaged primitive equations. In the CL equations the Bernoulli head term (let us note it $\mathcal{K}$) is defined as the kinetic energy increase due to the waves, i.e. $\mathcal{K} = \frac{\|\mathbf{u}^s + \mathbf{u}\|^2}{2} - \frac{\|\mathbf{u}\|^2}{2}$. In the wave-averaged equations, the form of the Bernoulli head is much more complicated (see eq (9.20) in McWilliams et al. 2004 or the S^{Shear} term in Eq. (40) in Ardhuin et al. 2008) and it does not appear explicitly in our implementation because of the general weak vertical shears in the wave-mixed layer. The effect of that term was also found to be much weaker than S^J in shallow coastal environments, except in the surf zone. It is also mentioned in SFK16 (their Eq (14)) that the contribution of the Stokes shear force should be retained but since this term results from the combination of the vertical component of the vortex force with the Bernoulli head, it does not appear explicitly in our derivation. Finally, compared to the previous version of the manuscript, additional terms related to the slope of the vertical coordinate have been added in Eqs. (7) and (8). Those terms are pieces of the vortex force which need to be taken into account with a generalized vertical coordinate. They seldom appear in the literature because most people present the wave-averaged equations in geopotential coordinate.

- p4,Eq.(12) The "wave pressure vector" seems a little odd. It would be more natural to make $\mathcal{W}_{\text{prs}}$ 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 ... $\mathcal{W}_{\text{prs}}$"
  This has been reformulated by including the gradients in the $\mathcal{W}_{\text{prs}}$ term and removing the reference to the vertical integral of $\mathcal{W}_{\text{prs}}$ since after our simplifications this becomes a 2D horizontal field. The notations have been adapted and are now more consistent with Bennis et al. (2011) and Michaud et al. (2012) except

that the S^J and S^{Shear} terms are expressed directly in terms of pressure terms $\widetilde{p}^J$ and $\widetilde{p}^{\text{Shear}}$.

- 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) ?
  The non-linear bottom drag in the baroclinic mode is computed in an implicit way as $(C_D\|\mathbf{u}_h\|)^{n-1}\,\mathbf{u}_h^{n+1}$. Because of the linearization this term is analogous to a linear bottom drag and thus easy to separate into a barotropic and a baroclinic contributions. See Sec. 10.4 in the version 4.0 of the NEMO documentation for more details.

- p6,Figure1 You seem to have evaluated the primitive $\mathcal{I}_{\text{B14}}$,as you plot it out here. Why is it not written out explicitly like $\mathcal{I}_{\text{B16}}$, which is set out at the bottom of p5?
  The primitive of $\mathcal{S}_{\text{B14}}$ requires the evaluation of the exponential integral function $\text{Ei}(z)$. This special function is not available in the fortran standard while the erfc function in the primitive of $\mathcal{S}_{\text{B16}}$ has an intrinsic procedure to compute it. This is the reason why we say that "The $\mathcal{S}_{\text{B16}}$ is more adapted" for a Finite-Volume interpretation of the Stokes drift velocity.

- 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 e3 than a diagnostic equation for $\omega + \omega^s$.
  We changed the wording. For your second remark, the overwhelming majority of NEMO simulations are done with a quasi-Eulerian vertical coordinate (either $z^\star$ or σ or a mixture of both) meaning that $\partial_t \text{e}_3$ is given by the free-surface evolution (i.e. the coordinate system breathes with the free-surface) and in this context Eq. (10) is used to diagnose $\omega + \omega^s$. With a $\widetilde{z}$-coordinate $\partial_t \text{e}_3$ is also prescribed by the evolution of the free-surface but also by the time-evolution of the coordinate

surfaces but the rationale is the same: Eq. (10) is used to diagnose $\omega + \omega^s$. A sentence has been added to clarify this point.

- p9,l211 Should be $\|\mathbf{u}^s_{\mathrm{LC}}\| \propto \sqrt{\|\tau\|}$
Yes, it is indeed the case, we agree it should be $\sqrt{\|\tau\|}$
- 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 $\tau^{\mathrm{oce}}_{\mathrm{WW3}}$?
At this point, the notation τ^{oce} refers to the momentum flux used as a boundary condition for the oceanic model independently from the way it is computed. In practice it is indeed in the wave model that τ^{oce} is explicitly computed while in the oceanic model it is only diagnosed via equation (22). We hope this aspect is more clear in the revised manuscript.
- 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?
This issue has been tackled earlier. The WW3 model is forced by a neutral drag coefficient which depends on the Charnock parameter. Historically, the stress computation in WW3 does not depend on atmospheric stability because only winds were provided to the wave model. As mentioned earlier, we tried to include the NEMO bulk formulation in WW3 but the wave solution thus obtained was extremely different from the original solution in the neutral case. Changing the bulk formulation requires a complete re-calibration of the wave model parameters. Again, to our knowledge our practice is customary to all coupled ocean-wave models.
- 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.

Indeed momentum is not conserved because we compute twice the atmospheric flux with two different bulk formulations. As mentioned above, to our knowledge no coupled atmosphere-wave-ocean coupled model guarantees the momentum consistency (on top of the fact that most coupled models use a non-conservative grid-to-grid remapping of the wind-stress). This issue was already explicitly mentioned in the paper in Sec. 3.2: "*This strategy is not fully satisfactory since it breaks the momentum conservation*".

- 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 feedbackâĂŤby overestimating the eddy amplitude, lifetime, and spatial range."
We have to make a distinction depending on the type of coupling with the atmosphere. In a fully coupled mode, oceanic currents have to be taken into account because the corresponding loss of kinetic energy by the ocean is partially compensated by a re-energization of the ocean by the atmospheric PBL. In a forced mode, this re-energization is absent because atmospheric PBL processes are not accounted for and the loss of kinetic energy is thus largely overestimated. Since it is clear that in a forced mode we don't represent the key feedback loops to properly represent the coupling between oceanic currents and the atmosphere we decided not to include this effect. But it should be clear that it is not a limitation of our implementation because it would be straightforward in coupled ocean-wave simulation to include the ocean current when computing the wind-stress (the namelist parameter rn_vfac just needs to be set to 1 instead of 0). The objective of our simulations is not to improve our physical understanding of ocean-wave processes but to check the robustness of our implementation.
- 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.

The aspects of the wave model discussed in Sec. 4.1.1 are specific to our particular global configuration and other options are available in WW3. That's the reason why we structured it that way. From our point of view Sec. 3.1 should introduce things that are common to any WW3 simulation and necessary to understand where the coupling operates.

- 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.
  In the manuscript we provide most of the information about the options used for the NEMO runs. For more details on those options, the namelist we used for the simulations are available under zenodo. In particular, see
  https://zenodo.org/record/3331463/files/namelist_cfg?download=1 and
  https://zenodo.org/record/3331463/files/namelist_ref?download=1
  Furthermore as the reference to the Drakkar group was indeed confusing since Drakkar refers to NEMO global modelling community and not to specific numerical scheme, this sentence has been removed from the new manuscript

- p13,,l342 How does the lateral diffusivity vary away from the equator?
  We have clarified it in the manuscript. The values of lateral (hyper)-viscosity and diffusivity we give in the paper are the values at the equator. Away from the equator those values vary proportionally to Δx for the diffusivity and Δx^3 for the hyper-viscosity.

- 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.
  This remark was indeed confusing and was simply wrong. The only correction we make to the forcing fields is to guarantee that their annual mean matches the

annual mean obtained from satellites.

- p14,section4.1.3 There should be a test experiment with ST_CPL + changes to the TKE scheme but no Langmuir parameterization, to see whether the new TKE boundary condition makes any difference.
  This is a good point, we have done this additional experiment (referred to as TKE_CPL) and results are shown in Fig. 10. Note that besides the new boundary condition for TKE other changes have been done also to the boundary condition to diagnose the mixing length and an extra forcing term related to the Stokes drift shear has been added in the TKE equation.

- p14,section4.1.3 Please specify the initial conditions. Is it a spun up run of some standard NEMO setup? If so, give details.
  All ORCA025 experiments have been initialised from reanalysis GLORYS2V4 delivered by MERCATOR-OCEAN-INTERNATIONAL

- p15,Figure2 Various random missing letters on panel titles.
  Yes, the rendering of this figure was fine with Mac but is bad on other operating system. The problem has been solved.

- p16–p17,section4.2.2 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.
  A new simulation TKE_CPL including all terms of the wave coupling except Langmuir parameterization has been performed and results where added in Figure 10, description on table 2 and discussion added in the text

- p17–p19,section4.2.3 Give reference for ARGO MLD climatology, and specify the MLD criteria used in model and climatology.
  ARGO data are issued from an updated version of de Boyer Montégut et al.

(2004) where the criterion used is Rho_10m-Rho_10m*0.03 . This has been added in the new manuscript

- p17–p19,section4.2.3 Maps of discrepancies of MLD from ARGO, and zonal-average MLDs would be more convincing than the MLD pdfs.
At first, we looked at maps of discrepancies, but due to the scarcity of the measurements the relevance of such comparison seems meaningless. From our point of view, the best way to compare is to co-localize the model results with the data. Eddies and fronts in the numerical simulations are not at the same place such that it makes more sense to look at PDFs rather than point-by-point differences. That is the reason why it has been chosen to use MLD pdfs. Although far from being an ideal diagnostic it at least shows a reliable statistical improvement of the MLDs when wave coupling is activated.

- p20,445 “an increased heat content during winter leading to higher SST during summer.” Is this the wrong way round?
Indeed this the wrong way round, winter and summer have inverted in the new manuscript

- p29, appendixB 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?
The LES results are the one presented in Noh et al. (2016) and our Figure B.1 should be compared to their Fig. 3. We modified the text to clarify this.