Review report

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Title: Analytical approaches for wave energy dissipation induced by wave-generated turbulence and random wave-breaking

Authors: Yongzeng Yang, Fuwei Wang, Meng Sun, Xingjie Jiang, Xunqiang Yin, Yongfang Shi, and Yong Teng

Overall assessment

This study proposes revised parameterizations for wave dissipation from two aspects: wave-generated turbulence and wave-breaking. Through the analysis of mechanical energy equation for wave orbital motion, the authors present a revised formulation of wave attenuation through wave-turbulence interaction, which was empirically fit to observation data previously. As for the wave-breaking induced attenuation, they employ a formulation such that the spectrum adjusts to a prescribed post-breaking spectrum. They also investigate the general behavior of the new formulations through some idealized simulations.

The topic of wave dissipation is certainly important and falls within the scope of Ocean Science. However, the theoretical and experimental development in its current form is not convincing enough. The derivation lacks clear statements of assumptions and several steps in the derivation lack sufficient justification, both mathematically and physically. Moreover, the numerical tests are conducted without observational or analytical benchmarks, leaving their purpose and insights unclear. These issues make it difficult to assess the validity and appropriateness of the proposed formulation, which is central to the manuscript.

The manuscript would benefit from language editing by either a native English speaker or a professional editor.

Major comments

1. Abstract: Given the insufficient theoretical and experimental foundations as pointed out in the comments below, the abstract appears to overstate the fidelity of the findings of the present study. For example:
    - L6: The presented formulation cannot be described as “improved” without rigorous theoretical foundation or adequate comparison against existing models.
    - L8: The comparison with measurements or previous simulation results and associated discussions are too limited and do not adequately “verify” the new formulation.

2. The analytical derivation processes in Section 2 lack clarity in many parts. In particular, the assumptions underlying the transformations are not always stated explicitly, and some of the mathematical steps seem to rely on implicit reasoning.
   - Eq. (1): I have downloaded the cited articles (Yuan et al. (2012) and Yang et al. (2019)), but no such equation as this was provided. I would recommend the authors to outline the derivation process, or at least to clarify the basic assumptions behind. Especially, how are the turbulence, wave orbital motions, and background current are separated, and what kind of assumptions are made about their timescales? How is the turbulent Reynolds stress modeled?
   - Eq. (12): This representation of mixing coefficient $\langle \bar{k}^2/\pi^2\bar{\varepsilon} \rangle_{SM}$ is not supported with the information provided so far. Please justify with a clear statement of underlying assumptions.
   - Eq. (19): Even if Eq. (13) is assumed to be valid, this expression appears to contain an error. In particular, it is unclear how the coefficient $7\sqrt{7}/16$ arises. Please clarify.
   - L348: Why can one transform like $\frac{c_0^2}{2g\mu_0^{1/2}} = \frac{g\mu_2}{2\mu_0^{1/2}\mu_4}$? It should be valid only when waves are monochromatic. It contradicts with later assumed $\rho = 0.4, 0.5, 0.6$ or so.
   - Eq. (26): There is no explanation or justification of this new expression. Please clarify what assumption brings Eq. (21) or (24) to this form. What does the integral term physically represent?
3. The comparison of growth and decay rates in L223-250:
   - To evaluate the decay rate, one needs to assume some value of A, but no explanation is provided. Please clarify.
   - L244-245: Without a relation between u_* and u_{w0}, the comparison between two figures does not make sense. Also, which parameter range do “normal and extreme sea conditions” refer to?
   - Fig 2: The plotting range in y-axis, $0 \leq u_{w0} \leq 8$ m/s, seems to be comparable to, or exceed the phase speed of waves with wavenumbers shown here. Such a situation is unrealistic.
4. The comparison of measured TKE dissipation and wave KE dissipation in L286-310:
   - Through the analysis, do the authors try to argue that the TKE dissipation rate ε_{dis} is locally balanced by the modeled wave-induced production e_{tid}? Such an argument requires quantitative discussion of other terms in the TKE equation because, in such a strongly forced short-fetched wind wave situation, neglected

terms such as the TKE production by breaking waves and wind-driven shear turbulence and the nonlocal transport of TKE through Langmuir turbulence are likely significant.

- Fig 4 seems to contain an error. The slope of the model line should be proportional to $K = 2\pi/\text{wavelength}$, but the slopes of Exp.1 (red) and Exp.3 (blue) are nearly identical. According to Table 1, they should differ by a factor of about 2.

5. Section 3: The purpose and context are unclear.
    - The authors compare MASNUM results using different tuning parameters with the academic case presented in Janssen et al. (1994), but both are numerical model outputs. In this framework, it is not appropriate to refer to this as a "validation" (L416), since no observational or benchmark data are used for comparison, and there is no basis to judge which result is "better".
    - The quantitative behavior of the model can always be adjusted with tuning parameters. Comparing new model results (Experiments 2 and 3) without calibrating the parameters does not demonstrate that one model performs better than another.
6. Section 4: The discussion lacks a clear physical rationale and seems to drift away from the central point.
    - Here the authors argue that the parameter α_{wt} should depend on the orbital-velocity-based gradient Richardson number Ri_g. In Eq. (2), α_{wt} is originally introduced as the ratio of phase-averaged TKE production to that evaluated with phase-averaged eddy viscosity and strain rate. It would be helpful if the authors could clarify how the stratification (i.e., Ri_g) can affect the phase-dependent variation of turbulence quantities and thereby modify α_{wt}.
    - Moreover, the stability criterion based on the gradient Richardson number is only a necessary condition for instability and should not be interpreted as a sufficient condition suggesting that "the perturbation must be amplified" (L504). Accordingly, the evaluations in Table 3 and Figure 10 do not ensure that the areas satisfying the criterion are turbulent. It also remains unclear how this evaluation is relevant to determining α_{wt}.
7. Section 5: Likewise in Abstract, the conclusion seems to overstate the outcome of the present study. For example, in the paragraph starting at L543, the authors suggest that the wave dissipation with breaking only is insufficient based on the experiment made in Section 3. Without calibrating the tuning coefficients, even if one setup performs better than another, it does not necessarily mean the former represents physical processes more realistically. Furthermore, the new formulation was compared against

another simulation (Janssen et al., 1994), which cannot necessarily be considered as reference.

Specific comments and minor issues

8. L4: I do not understand what "high-deterministic model" refers to.
9. There are many undefined mathematical symbols throughout the manuscript: $\sigma_0, K_0, K, k_1, k_2, c_0, T_z$ etc. Since similar symbols are used to represent totally different quantities in this manuscript (e.g., L vs $\bar{L}$), clear definitions are necessary.
10. L157 I suppose k should represent TKE, not k^2.
11. L162 KE and PE seem to have inconsistent dimensions.
12. L195 $\langle AA^* \rangle = \delta(\vec{k} - \vec{k}')E(k_1, k_2)$ is incorrect. Delta function should arise as a result of integration of $\exp i(k - k')x$.
13. Eq. (9) It is unclear why the authors need this transformation (multiplying the numerator and denominator by $\iint_{\vec{k}} \omega^2 K^2 E(k_1, k_2) \mathrm{d}k_1 \mathrm{d}k_2$), as it is not used later.
14. L243 Define τ_w and τ.
15. L243-244: Is there a reasonable explanation for the choice of these parameters τ_w/τ and α_{wt}?
16. L273 "Layer depth x_3 varies corresponding to different A" How and why?
17. L335 ε is used for TKE dissipation in Section 2. I would recommend using another character.
18. L401-408 There is no explanation on the simulated domain (both in spatial and spectral).