

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

In the first review, I pointed out issues mainly in two aspects: (1) a lack of clarity in the derivation process, and (2) weaknesses in the numerical testing framework, particularly the lack of reliable benchmarks.

Regarding the first aspect, some of the concerns and confusions have been alleviated in the revision. However, other revisions remain rather superficial, and several important concerns regarding the credibility of the derivation largely remain unaddressed. Some of the major comments listed below repeat those from the initial review, as the authors' responses did not fully tackle the core issues.

As for the second aspect, instead of providing additional comparisons with reliable benchmarks, the authors have weakened the tone of their claims on “validation” or “verification.” Since the formulation depends on several empirical parameters that are only loosely constrained, the numerical tests do not meaningfully demonstrate the advantage of the new parameterization or provide physical insights.

Given the current quality of the manuscript, its scientific significance does not meet the standard required for publication in *Ocean Science*.

Major comments

1. L9: “provided and validated tentatively”
The authors slightly modified the wording, but it still seems to overstate the credibility of the results. The presented comparisons with the lake experiment are not robust enough to “validate” the formulation. If the authors intend to emphasize the physical insights gained from the comparison, those points should be stated more clearly.
2. L11 and L572: “Numerical results indicate that the wave energy dissipation ... is inadequate, ... wave-generated turbulence play critical roles of wave energy loss.”
“... that the wave energy loss only induced by wave-breaking appears to be inadequate, ...”
Since the authors attempt to draw physical interpretations without any quantitative comparison against reliable reference data, the simulation results are not sufficiently

- credible to identify the relevant physical processes. Therefore, these statements are not supported by the presented evidence, and I would recommend removing them.
3. Eq.(1) and Appendix A:
The authors added Appendix A, which is very helpful for the readers to grasp the derivation of Eq. (1). Still, some implicit yet important assumptions (which can be only inferred from the derivation) should be made explicit so that the readers can assess the reliability of the formulations:
 - Timescale separation is assumed such that turbulence timescale is much shorter than wave period.
 - Reynolds stress components are represented with $k - \varepsilon$ type eddy viscosity model.
 4. Eq.(12):
In the first review, I have pointed out Eq. (12) is unsupported by the information provided so far. The authors have rephrased some description, but the problem mostly remains as it was. What did Yuan et al. (2013) proposed, how does l_D come in the formulation, and what does the new choice physically mean?
 5. L304-324 and response 2.2.4:
Original comment, designated as “L286-310”, was meant for the experiment series of Wei et al. (2018), described in Table 1, and plotted in Figure 4. They are strongly influenced by wind. Without considering other terms in TKE equation, the comparison between ε_{dis} and e_{tid} does not make much sense.
 6. Eq.(26):
The derivation of Eq. (26) still lacks clear logical steps connecting Eq. (21) or (24) to it. Although the authors have reinforced the description of the underlying physical assumptions, it remains unclear how the formula can be transformed into this form. Therefore, the fundamental questions raised in the previous review remain unaddressed.

Specific comments and minor issues

7. L6 and other parts: “... (turbulence model) with high certainty closure assumptions”
The phrase “high certainty closure assumptions” is unusual and potentially misleading, so I would recommend rephrasing it.
As pointed out in comment 3, the present derivation essentially assumes eddy viscosity formulation to model the Reynolds stress acting on wave orbital motions, which is a nontrivial assumption. In fact, wave-resolving numerical studies show that the

Reynolds stress does not vary in phase with the strain rate. (e.g., Guo & Shen, 2014. *J. Fluid Mech.*, 744, 250-272.)

8. Fig. 1:

How is τ_w related to the estimation of the growth rate? The figures appear to show smaller wave growth rates when the wave-coherent stress is larger, which seems counterintuitive. Please clarify how these quantities are related.

9. Fig. 1 and 2:

While various combinations of friction velocity and orbital velocity are possible, I would recommend the authors to indicate a realistic comparison. For example, a bulk formula for wind stress and the fetch law (or its limiting form for fully developed sea) would provide a typical relationship between γ and γ_{tid} . Alternatively, authors can provide the estimate of γ and γ_{tid} from observational data of wind and waves.

10. L204 and Response 2.2.12:

Indeed, those familiar with Fourier transform theory would understand what is intended, but there is no reason to leave such an evidently incorrect and easily correctable expression unchanged. At least in the copy of the Kinsman's textbook at my hand, his use is correct.

L204 should be $\langle AA^* \exp i[(k_\alpha - k'_\alpha)x_\alpha - (\omega - \omega')t] \rangle = \delta(\vec{k} - \vec{k}')E(k_1, k_2)$, and the first and second line of Eq.(7) should be modified correspondingly.