CORRESPONDENCE

Reply to "Comments on 'A Combined Derivation of the Integrated and Vertically Resolved, Coupled Wave–Current Equations'"

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ABSTRACT

The comments of Ardhuin et al. concerning the papers by Mellor from 2003 and 2015 are reviewed. It is found that the comments do not impact the validity of these papers.

1. Introduction

The comments of Ardhuin et al. (2017, henceforth ASMA) are appreciated in that my papers are accorded significant scrutiny and even compliments by distinguished ocean researchers. In return, their comments are deserving of this reply.

The subject is primarily Mellor (2003, henceforth M03) and the follow-on paper Mellor (2015, henceforth M15). Both papers share the same fundamental approach as in Longuet-Higgins and Stewart (1962, 1964, henceforth L-HS) and Phillips (1977) but were extended to account for vertical variability. Similarly, whereas L-HS introduced the so-called radiation stress term for the vertically integrated phase-averaged momentum equation, M03 and M15 derived the corresponding vertically dependent radiation stress term.

A more recent paper Mellor (2016, henceforth M16) compares what I will call the vortex force theory with the aforementioned radiation stress theory. The vortex force theory deals only with Eulerian velocities, whereas the radiation stress–dependent variables are the combined Eulerian velocities plus Stokes drift. The paper presents multiple reasons why the vortex theory is incorrect.

2. Error discussion

Many wave circulation theories (e.g., L-HS) are built upon the assumptions that wave steepness $\varepsilon = \varepsilon_1 = ka$ and horizontal gradients such as $\varepsilon_2 = (ka)^{-1}\partial a/\partial x$ are small; here, k is the wavenumber, and a is the wave amplitude. In my writings, I have made a point of the fact that my form of the momentum equation, and I presume others, contain errors of order $(ka)^4$.

ASMA begin with criticism of M03 and M15 and seemingly assert the failure of the linear wave solutions or Airy solutions to approximate wave motion in formulating phase-averaged, coupled wave–current equations. For this discussion, I prefer to write their (4), (5), and (6) for wave pressure and horizontal and vertical velocity as

$$\tilde{p}(x_\alpha, z, t) = kac^2\frac{\cosh k(z+h)}{\cosh kD}\cos\psi, \tag{1}$$

$$\tilde{u}_\alpha(x_\alpha, z, t) = k_\alpha ac\frac{\cosh k(z+h)}{\sinh kD}\cos\psi, \quad \text{and} \tag{2}$$

$$\tilde{w}(x_\alpha, z, t) = kac\frac{\sinh k(z+h)}{\sinh kD}\sin\psi, \tag{3}$$

so that the vertical displacement of water parcels is

$$\tilde{s}(x_\alpha, z, t) = a\frac{\sinh k(z+h)}{\sinh kD}\cos\psi \tag{4}$$

and where $\psi = k_\alpha x_\alpha - \omega t$. The subscript $\alpha = x, y$; the horizontal coordinates are $x_\alpha = (x, y)$, and z is the vertical coordinate; and $k_\alpha = (k_x, k_y)$ is the wavenumber vector, whereas $k = |k_\alpha|$ and ω is frequency. Also, $z = -h$ is bottom depth, $\hat{\eta}$ is the mean surface elevation, and $D \equiv h + \hat{\eta}$. The only difference from that in ASMA is that the above form emphasizes that wave velocities are of order ka [(3) includes a correction to their (5)].

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ASMA point out that the above "are approximations, strictly valid only for a flat bottom, a constant amplitude" flow case. Does that mean that the Airy relations are not useful in formulating phase-averaged equations? No, it means that they will introduce errors of order $(ka)^4$ ($\sim10^{-3}$ or smaller) and of order ε_2 ($\sim10^{-2}$ or smaller) when used by M15, L-HS, and others. Additionally, it was shown by Mellor (2013) that, in applying (1), (2), and (3) to flows where the bottom is not flat, an error of order

$$\left[(ka)^2\left(\frac{k_\alpha}{k}\frac{\partial h}{\partial x_\alpha}\frac{1}{\sinh kD}\right)^2\right]$$

is incurred; generally, the above expression is order $(ka)^4$. In deep water (say, $kD > 3$ so that $\sinh kD \gg 1$), bottom slopes as large as unity could be tolerated. In other words, (1), (2), (3), and (4) are useful if the aforementioned errors are acceptable, as in most oceanographic applications.

Nevertheless, ASMA assert that the use of (1), (2), (3), and (4) are somehow "inconsistent" due to the neglect of wave–current interactions. If this has to do with current vertical shear $\partial\hat{u}_\alpha/\partial z$, it could have been included in the development of M15 as a term $\overline{\tilde{s}\partial\hat{u}_\alpha/\partial z}$ to be added to $\hat{u}_\alpha$ in (21). Completing the operation of the same equation shows that the term is order $[|\hat{u}_\alpha|(ka)^2]$ relative to $\hat{u}_\alpha$ and can be neglected. On the other hand, ASMA invoke a working version of the current only equation—their (11)—which does not include a wave–current term. Is this "consistent"?

One organizational improvement of M15 over M03 is that the derived radiation stress term is obtained [as in ASMA's (8)] before invoking the Airy relations. This is (29) in M15 (after correction for a missing D[1] in M15). At this point, other wave relations might have been invoked to close the formulation, but we do use the Airy relations, with confidence in their correctness in closing the wave radiation term.

In their introduction, ASMA state, "When inferring his (30) from his (28), one can add any term that has a depth-integrated value of zero but can be very large locally." However, my reckoning is that there are no combinations of phase-averaged wave variables that have the dimensions of velocity squared and vertically integrate to zero.

[1] This should be inserted below $\delta_{\alpha\beta}$ in (29). Also, substitute $\tilde{p} - g\tilde{s}$ for p in the second integral in the equation immediately below section 6b. In the same section, delete the first sentence after (26).

3. A term in addition to the radiation stress term: To be or not to be

In section 2 of ASMA, there is a good deal of discussion concerning $\overline{\tilde{s}_\alpha\tilde{p}}$, a term that appeared in M03 but not in M15. Calculating the term reveals that it is near zero when $kD \geq 3$ and everywhere small relative to terms in (29) of M15 when $3 \geq kD \geq 1$; its vertical integral is zero in all cases. Furthermore, consider

$$\tilde{s}(x_\alpha, z, t) = a\frac{\sinh k(z+h)}{\sinh kD}\cos\psi,$$

so that

$$\begin{aligned}\tilde{s}_\alpha(x_\alpha, z, t) &\equiv \frac{\partial\tilde{s}}{\partial x_\alpha}\\ &= k_\alpha a\frac{\sinh k(z+h)}{\sinh kD}[\sin\psi + (k_\alpha a)^{-1}\partial a/\partial x_\alpha\cos\psi].\end{aligned}$$

It is consistent with the derivations of (1), (2), (3), and (4) that the term that includes $(k_\alpha a)^{-1}\partial a/\partial x_\alpha =$ order ε_2 should be neglected. Thus, noting that $\overline{\tilde{s}_\alpha\tilde{p}}$ contains $\overline{\cos\psi\sin\psi} = 0$, it was considered an agreeable simplification of M15 to eliminate $\overline{\tilde{s}_\alpha\tilde{p}}$. But, in their section 3, ASMA assert that the term must be retained "for consistency" with the stress radiation relations. Apparently, the fact that it is not retained in M15 suggests to them that the Airy relations are somehow inadequate. They cite a need to have some other term to balance the vertical variability of (8). As detailed in Mellor (2013), the required balance is turbulence-supported vertical momentum transfer. This is not surprising; the radiation stress term is the transport of momentum from a wind-driven source, which also requires turbulence-supported vertical momentum transfer.

Among the differences between the vortex force folk and L-HS, M03, and M15 are that the latter assert that the vertical component of Stokes drift is nil and consequently the continuity equation uses velocities, which are a combined Eulerian and Stokes drift. That the Stokes vertical component is nil follows directly from its very definition (Phillips 1977; M16). Conversely, ASMA adopt the horizontal formula from the Stokes drift in deriving the vortex force term but not the vertical component. Thus, they invent a nonzero vertical Stokes component so that Stokes divergence is nil and consequently the Eulerian velocity divergence is also nil, an essential companion to their momentum equation.

The practical application of the vortex force approach is exemplified by their (11), which is a part of the system in Uchiyama et al. (2010) and which includes integral equations for wave energy. They state that the vortex force approach can be derived from the stress radiation approach by reference to Andrews and McIntyre (1978). But I submit that the vortex force equations and the wave energy equation that are actually solved numerically, as in Uchiyama et al. (2010), cannot be derived from the stress radiation equations (M03; M15) and the mean wave energy equation or vice versa, and I challenge ASMA to do so.

ASMA point to the test case shown in their Figs. 1 and 2. This is an inviscid, wave-resolved, nonhydrostatic flow case that, somehow, suggests a need to include a missing term in M15 relative to M03. Of course, a nonhydrostatic solution, particularly in this flow case, does differ from hydrostatic solutions [from, e.g., (11)]. I do not know how they do the diagnostics and cannot fathom the relevance of this portion of their comments. Not included is a comparison of the test case solution with a solution using the vortex force theory.

Three laboratory experiments that compare with calculations and which depend crucially on the radiation stress term (and wherein the vortex force would be nil) and vertical turbulence momentum transfer are to be found in Marsooli et al. (2017). The coupled wave circulation model calculations closely mimic the data.

4. The Eulerian current: Vortex force equations

In the abstract of my recent paper, M16, I stated that "the vortex force approach stems from an interesting mathematical construct, but it does not stand up to physical or mathematical scrutiny." (Unfortunately, the modifier "not" was missing, the victim of a typographical error, but it is here emphatically restored.) In M16, I criticize an early derivation of the vortex force, which I took to be that of Leibovich (1980) and McWilliams and Restrepo (1999). Aside from their mistreatment of the continuity equation, I found that the consequences of their formulations, like boundary conditions, are not physically acceptable. The underlying reason is that, in their derivation, they begin with the curl of the primitive equations of motion, wherein irrotational terms—including the lowest-order terms that yield (1), (2), and (3)—drop out of contention. Upon "uncurling," these terms are not restored. It is these missing terms that contribute to the radiation stress term that after vertical integration were first derived by L-HS and Phillips (1977); these authors, M03, and M15 did not invoke the curl–uncurl process, fortunately.

5. Summary

M15 rederived the vertically resolved wave–current relations and supported the vertically integrated equations of L-HS and Phillips (1977). Although a small error was discovered in both L-HS and M03, the final results in M15 were unchanged. I believe the derivations in M15 are free of error.

In the above, I have tried to understand the comments of ASMA. They apparently reject the idea of the linear wave relations representing waves in phase-averaged, wave–current equations, sometimes invoking the lack of unstated higher-order terms in M03 or M15. I find that the phase-averaged equations are correct to order $(ka)^2$, that is, errors of order $(ka)^4$ are incurred.

It is interesting that criticism is directed at M03 and M15, whereas M16 is simply cited and receives minimal comment. It is in M16 that I lay out multiple arguments and mathematical detail, showing that the continuity and momentum equations that deal with the Eulerian currents and a vortex force are incorrect.

I hope that the comments of ASMA and my response contribute to an understanding of wave–current interaction, a complex subject.

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