Reply

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1. Introduction

The comments by Bennis and Ardhuin (2011, hereinafter BA11) are partially correct but, for the most part, are incorrect.

2. Discussion

The paper by Mellor (2008) contained some worthy aspects: in particular, the expression for the depth-dependent radiation stress,

$$S_{\alpha\beta} = E\left\{\frac{k_\alpha k_\beta}{k}F_{\rm CS}F_{\rm CC} - \delta_{\alpha\beta}\left[kF_{\rm SS}F_{\rm SC} - \frac{\delta(z - \hat{\eta})}{2}\right]\right\}, \tag{1}$$

where the F functions are defined in Mellor (2003, 2008) and BA11. Equation (1) includes a Kronecker delta coefficient $\delta_{\alpha\beta}$ and a Dirac delta function $\delta(z-\hat{\eta})$, where $\hat{\eta}$ is the mean elevation ($D \equiv \hat{\eta} + h$, where h is the water depth). The problem with Mellor (2008) is that the radiation stress appears as $\partial S_{\alpha\beta}/\partial x_\beta$ in the Cartesian form of the phase-averaged momentum equation; additional terms were missing (see appendix A of Mellor 2005) that would cancel the Leibnitz terms in BA11's Eq. (17). Thus, a vertical integration of the momentum equation did not equate to the corresponding equations in Phillips (1977) and Smith (2006). BA11 are correct about this issue. A recent corrigendum corrects the Mellor (2003) paper so that Eq. (1) does appear as $\partial(DS_{\alpha\beta})/\partial x_\beta$ but in the sigma form of the phase-averaged momentum equation. As seen below, it can be vertically integrated so that agreement with Longuet-Higgins and Stewart (1964), Phillips (1977), and Smith (2006) is obtained.

The delta function $\delta(z-\hat{\eta})$ in Eq. (1) is defined in Mellor (2008) and in BA11's Eq. (8) such that $\delta(z-\hat{\eta}) = 0$ if $z \neq \hat{\eta}$ but

$$\int_{-h}^{\hat{\eta}^+} \delta(z - \hat{\eta})\, dz = \int_{-\varepsilon}^{\varepsilon} \delta(z')\, dz' = 1$$

for small ε and $z' = z - \hat{\eta}$. The "approximated" form in Eq. (14) of BA11 is incorrect, however. A proper form might be

$$\delta(z') = \lim_{a\to 0}\frac{1}{a\sqrt{\pi}}\exp(-z'^2/a^2), \tag{2a}$$

where the right side is a "generalized function" (Lighthill 1958; Greenberg 1978) or impulse function. Differentiation with respect to x, where $\hat{\eta} = \hat{\eta}(x)$, and then integration with respect to z' yields zero, not infinity as BA11 would have it. A seemingly small change—replacing z' by $-|z'|$—in Eq. (14) such that

$$\delta(z') = \lim_{K\to\infty}\frac{K}{2}\exp(-K|z'|) \tag{2b}$$

can also be an acceptable impulse function. As in Eq. (2a), the integral of Eq. (2b) with respect to z' after differentiation with respect to x is zero and is so by inspection.

In their section 3, I believe that BA11 solve the two-dimensional, sigma-coordinate equation

$$\frac{\partial UD}{\partial t} + \frac{\partial U^2 D}{\partial x} + \frac{\partial \Omega U}{\partial \varsigma} = -\frac{\partial}{\partial x}(DS_{xx}) - gD\frac{\partial\hat{\eta}}{\partial x} + \frac{\partial\tau}{\partial\varsigma},$$
$$\tau = \frac{K}{D}\frac{\partial U}{\partial \varsigma}, \tag{3a,b}$$

where $\tau(0) = \tau(-1) = 0$ and from Eq. (1)

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$$S_{xx} = kE(F_{CS}F_{CC} - F_{SS}F_{SC}) + \frac{E}{2}\delta(\varsigma)$$
$$= \frac{2kE}{\sinh 2kD} + \frac{E}{2}\delta(\varsigma). \tag{3c}$$

Steady state is obtained. The "sigma" coordinate $\varsigma = (z - \hat{\eta})/D$. Here, as derived in Mellor (2003), U is the current velocity plus Stokes drift. I use the symbol Ω instead of W to distinguish it from the Cartesian vertical velocity. Also, I write Eq. (3a) in flux form, which, with the continuity equation (Mellor 2003), is convertible to Eq. (18) of BA11. The form of Eq. (3a) is convenient and readily integrates from $\varsigma = -1$ to $\varsigma = 0$, yielding

$$\frac{\partial M}{\partial t} + \frac{\partial}{\partial x}\int_{-1}^{0} U^2 D\, d\varsigma = -\frac{\partial}{\partial x}(D\overline{S_{xx}}^{\varsigma}) - gD\frac{\partial \hat{\eta}}{\partial x}, \tag{4}$$

where

$$M = M^T \equiv \int_{-1}^{0} UD\, d\varsigma \quad \text{and}$$

$$D\overline{S_{xx}}^{\varsigma} \equiv \int_{-1}^{0} S_{xx} D\, d\varsigma = E\left(2\frac{c_g}{c} - 1\right) + \frac{E}{2}, \tag{5}$$

with c and c_g being the phase and group speeds, respectively. The two terms on the right of Eq. (5) correspond to those in Eq. (3c). The second term on the left of Eq. (4) was converted to $\partial(M^2/D)/\partial x$ by Phillips with the help of some fairly restrictive assumptions as to the profile shape of $U(\varsigma)$. BA11 apparently accept Eq. (3a), derived in Mellor (2003), as a valid equation; it is not clear how BA11 regard the last term in Eq. (5) or its origin, the last term in Eq. (3c).

I can understand BA11's concern over the singular term in Eq. (3c). It follows directly from Longuet-Higgins and Stewart (1964), however. It is a contribution to wave momentum due to the (hydrostatic) pressure imbedded in the crest and trough at the wave surface and given by the expression

$$\overline{\int_{\hat{\eta}}^{\hat{\eta}+\tilde{\eta}} g(\hat{\eta} + \tilde{\eta} - z)\, dz} = \overline{\int_{0}^{\tilde{\eta}} g(\tilde{\eta} - z')\, dz'} = g\overline{\tilde{\eta}^2}/2 = E/2, \tag{6}$$

where, here, overbars represent phase averaging and $\tilde{\eta}$ is the instantaneous wave elevation relative to $\hat{\eta}$. In a depth-dependent context, the term is clearly concentrated at the surface; thus, the delta representation. When integrated, $E\delta(z - \hat{\eta})/2$ is subsumed in Eq. (5) and loses its singular identity.

Thus, if the surface of a flow is wavy then there must be a surface contribution to the phase-averaged momentum equation due to the wave's intrinsic surface pressure field. This effect is missing in the solution labeled "exact" in BA11's Fig. 3, which, therefore, is incorrect in my opinion. A more thorough discussion of the representation of wave-induced pressure is in Mellor (2011).

The existence of a concentrated surface forcing term in the momentum equation does dictate a realistic need to include a subsurface viscous or eddy viscosity stress term. Note that such forcing acts similar to a surface wind stress, which also requires a subsurface viscous or eddy viscosity term. Thus, instead of the exact solution, the long-dashed curve in BA11's Fig. 3 may be correct, although I have no knowledge as to upstream and downstream boundary conditions and other details.

A question does arise as to whether the U in Eq. (3b) should include or exclude the Stokes drift in contrast to U terms in Eq. (3a), which do include Stokes drift. I hope to answer that question in a paper now in progress.

BA11 seem to suggest that models such as those that are based on Eq. (3) and some form of $S_{\alpha\beta}$ that is based on Airy wave solution are not valid. Their position seems to be that a full Laplacian-type solution involving 10 vertical modes is needed. I disagree with that doomsday speculation.

Another question concerns the validity of Eqs. (3) and (1) in shallow water with a bottom slope; the question is, how shallow or how steep is the slope? Let k, a, and h be wavenumber, amplitude, and water depth, respectively. Whereas, in the development of Eqs. (3) and (1), terms of $O(ka)^2$ were retained and terms of $O(ka)^4$ were discarded, it can be shown (details available on request) that terms of $O[ka(\partial h/\partial x)/\sinh(kh)]^2$ should be less than or equal to $O(ka)^4$; that is, $(\partial h/\partial x)/\sinh(kh)$ should be small: comparable to or less than the wave slope (ka).

3. Summary

The BA11 criticism of Mellor (2008) is correct and is partially the subject of the recently published corrigendum to Mellor (2003).

Their generalized definition of a Dirac delta function is incorrect, does not conform to standard usage (Lighthill 1958; Greenberg 1978) or my interpretation, and leads to an erroneous conclusion.

BA11 object to the existence of a surface-trapped, wave-related pressure contribution to the momentum balance. It is, however, implicit in the derivations of Longuet-Higgins and Stewart (1964) and Phillips (1977) and is explicitly part of their vertically integrated wave radiation stress.

The basis of BA11's skepticism as to the utility of schemes that are based on a wave radiation stress term to couple wave dynamics to general circulation dynamics is unclear to me.

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