This paper compares different ways to represent surface gravity waves in simulations of the atmospheric boundary layer with an LES framework. The most accurate method is the wave-phase-resolved moving-boundary formulation which the authors refer to as the moving-wave model (MOW), wherein the grid follows the wave surface and explicitly computes the resulting pressure forces. This is compared to two wave phase-aware models that assume a flat sea surface but include wave amplitude and phase effects through addition of effective bottom stress terms that account for the unresolved pressure variations. These are referred to as the wave drag model (WDM) and windward potential flow model (WPM) that employ different methods to estimate the pressure effect. The most novel aspect of the paper is the implementation of these three schemes in the same WRF-LES code base, thus allowing for a fair comparison of the methods without subjecting the comparison to differences in numerical methods related to the underlying LES approach. The results should be very useful for the WRF community, and the idea is an excellent framework for comparisons of different parameterizations in ocean and atmospheric models. The paper should be published after the authors respond to the following comments.

Oliver Fringer
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1) The paper reads as if the authors set out to prove that the phase-aware models agree with the phase-resolved model, leading to overly subjective evaluation of the results with phrases like, “slightly overestimates”, “show good agreement”, “exhibit slight deviations” , “with minor differences”, “agree closely”, “slight deviations”, or “slightly underestimate”. Since the methods are already published in the peer-reviewed literature, there is no need to prove their accuracy one way or another. This paper should be used as an opportunity to present an unvarnished and quantitative comparison of the models so that future users are aware of the pitfalls for the methods.
   a) The most important figure in the paper is Figure 4a which compares the nondimensional form stress as a function of wave age and steepness for the different methods. The plot is too small, and the axes emphasize the variability due to wave slope rather than that due to wave age, for which it is noted that “WPM and MOW show good agreement across all wave steepness levels”. This plot should only compare results with ak=0.10 since that enables a fair comparison between all three models as well as to results in the literature. There is no need to show results of the phase-aware models with different ka because there is no validated solution. The variability with steepness and the fact that the phase-aware models agree can be quantified by simple scaling analyses. I don’t think Figure 4b is needed or it should be in a separate section (see #2 below).

b) Another example is the comparison of the contours of wave-induced streamwise velocity in Figure 7. There is no reason to believe that these should be the same among the three methods given the fundamentally different character of the bottom boundary condition when comparing the MOW to the other methods. Nevertheless, the authors go to great lengths to show how they are similar through use of rescaled color axes.

2) Introduction of the exponential decay function to improve the WDM formulation seems very ad-hoc and not very useful because all it appears to do is eliminate the negative values of the form stress for high wave age. I would recommend eliminating it unless there is a clear quantitative justification for its form and the coefficients.
3) The differences between the WRF-LES-WDM and LESGO-WDM results in Figures 2 and 3 are attributed to "differences in the numerical schemes employed by the LES codes." Given that the premise of the paper is precisely to avoid these types of differences through comparison of methods in the WRF framework, this validation should be removed as it detracts from the novelty of the paper.
4) This is more of an aside given that these terms are likely already established in the literature, but it's odd to me that the models are referred to as phase-resolved vs phase-aware. Since the phase-aware models amount to linearizations of the bottom boundary conditions by imposing effective pressure stresses at z=0 rather than the kinematic boundary condition at z=h, this is a finite-amplitude effect, not a phase effect. Thus, it is the amplitude effect that is not resolved, while the phase effect is resolved. My interpretation may stem from use of the term to distinguish between phase-resolved wave models like FUNWAVE, SWASH, and NHWAVE and action balance models like SWAN and WAVEWATCH III which are phase-averaged.
5) The phase-aware models appear to be presented using the formulations in the original papers which not only makes it difficult to directly compare them, but the same variables are used to represent different quantities from one method to the next. The authors should write all methods using the same variables and note where they differ. For example, in the paper $u_r = u - u_s$ for MOW and WPM while $u_r = u - c$ for WDM. I suggest using different variables for these two relative quantities, noting that $u_r$ for WDM is the velocity in a wave-following frame. One way to unify the methods is by writing all three in terms of the total bottom stress

$$\tau_{i3}^{Total} = \tau_{i3}|_{wall} + f_{pi},$$

where the effective bottom stress due to pressure, $f_{pi} = 0$ for MOW and, for WDM and WPM,

$$f_{pi} = -C_D U_i U^{\Delta} \left(n_k \frac{\partial h}{\partial x_k}\right)^n H\left(n_j \frac{\partial h}{\partial x_j}\right),$$

where, for WDM, $n = 1$ and

$$C_D^{WDM} = \frac{P}{1 + Q(ak)^2} ak$$
$$U_i^{WDM} = u_i$$
$$U^{\Delta,\mathrm{WDM}} = |\vec{u} - \vec{c}|$$
$$n_k^{WDM} = \frac{\vec{u} - \vec{c}}{U^{\Delta,\mathrm{WDM}}}.$$

For WPM, n=2 and

$$C_D^{WPM} = \frac{1}{\pi}$$
$$U_i^{WPM} = U^{\Delta,WPM} \frac{1}{|\nabla h|} \frac{\partial h}{\partial x_i}$$
$$U^{\Delta,\mathrm{WPM}} = \left|\vec{u} - \vec{C}\right|$$
$$n_k^{WDM} = \frac{\vec{u} - \vec{C}}{U^{\Delta,\mathrm{WPM}}}.$$

This more clearly shows how both methods are similar in how they parameterize the pressure force on the flow by the waves. Additionally, it is clear that, for the two methods:

$$U_i U^{\Delta} \left(n_k \frac{\partial h}{\partial x_k}\right)^n \sim \begin{cases} u_* |u_* - c| ak & WDM \\ |u_* - c|^2 (ak)^2 & WPM \end{cases}$$

which shows why the steepness is included in $C_D^{WDM}$ so that both methods are of the same order of magnitude. Writing the methods in this way avoids the need to define the drag force per unit area, $F_{d,i}$, for each method (it might be useful to write the equations as they appear in the papers in the appendix).

a) The drag forces per unit area are dimensionally inconsistent since, as written, they are in units of force per unit volume.

b) The addition of the directional term ($\nabla h/|\nabla h|$) in Equation 24 based on the need to include sgn(u-c) isn't clear, since the direction of $\nabla h$ is not necessarily related to that of $\vec{u} - \vec{c}$. Furthermore, ($\nabla h/|\nabla h|$) is a vector whereas Equation 24 is already written for vector component $i$. It would also be helpful simply to include this term in the description of the WDM method rather than as an entire paragraph in a different section. The details of why the form in the original WDM paper does not have the term can be included in an appendix.

c) I suggest using scaling to estimate the magnitude of the terms in each phase-aware formulation to show why an exponential decay is needed for the WDM model to behave correctly for large wave age.

d) The wall stress terms should also be written in a consistent way since they can all be written as $\tau_{i3}|_{wall} = C_d U_r u_i$. An estimate of the magnitude of $\frac{\tau_{i3}|_{wall}}{f_{pi}}$ would be helpful to understand the relevance of this wall stress term for the phase aware models. It would also be helpful to evaluate the extent to which using the filtered velocity vs the original velocity makes a difference when evaluating the bottom stress. The reader may see the use of the filtered velocity as necessary when in reality the differences are negligible.

   e) Evaluation of the log law at a height $\Delta z/2 - h$ to compute the drag coefficient in Equation 16 appears to be inconsistent with the idea that the phase-aware models represent a linearized representation of the wave kinematics by evaluating everything at z=0 rather than at z=h. Furthermore, the factor $\Delta z/2 - h$ implies a limiting grid size below which the method fails, which makes the method fundamentally inconsistent with the philosophy of LES which should converge to the "exact" filtered solution with grid refinement.

6) The grid resolutions for all three methods should be identical. Indeed, the phase aware models might have the advantage that they can be used with coarse grids (although I don't see how this could be the case). However, with different grid resolutions it is impossible to attribute differences due to the parameterization or the numerics, which is precisely the premise of the paper.
7) Following the previous comment, the time-step size should be the same for all three models. All viscous or turbulent stress terms (including the bottom drag) are discretized implicitly in WRF to ensure stability in the presence of possibly very small layer heights. Similarly, the phase-aware models should be implemented implicityly in WRF if they are to represent a useful contribution to the code base. Otherwise, it is easy to see how the time-step size needed for stability of the phase-aware pressure terms could become vanishingly small for small layer heights, strong winds, fast waves, and/or steep slopes.
8) It is stated that the phase-aware models reduce the vertical grid resolution requirement by approximately 10-20%. This conclusion could only be substantiated with a rigorous grid resolution study and subsequent quantification of error reduction as a function of simulation time for the different methods.
9) I don't see how the MOW approach costs four times as much as the other approaches for the same grid resolution. There is essentially very little overhead when computing the terms needed for the moving bottom, especially since the metric terms are computed at every time step regardless of whether the bottom moves. I am also not aware of the need for more precision when the bottom moves in WRF.
10) Minor:
   a) Equation 2: The SFS tensor is typically defined in terms of spatial averages.
   b) Please report the Courant number when describing the time-step size needed for stability. I note that WRF incorporates a splitting technique in which the acoustic modes are linearized at each time step and advanced forward in time with smaller

time steps with sub-stepping.