

I appreciate the authors addressing virtually all of my comments satisfactorily, and greatly increasing the clarity and readability of the manuscript. And it remains an interesting application and one of potential great importance.

Unfortunately I am still troubled by my Comment 8 on old Line 184, which I noted at the time was my biggest concern. (And Reviewer 2's Comment 2 suggests they have a similar concern.) Specifically, the inclusion of an elevated momentum source due to increased friction velocity from the turbines. I think there is some general confusion on these matters, so some leeway can be permitted, but I would make the following points.

) I am still not sure how new equation (22) is derived, specifically the derivation of the momentum flux term ($\Delta \tau S_{Vijk} (z_{k+1} - z_k)$) in the mean kinetic energy budget, where $\Delta \tau$ is the change in $\rho u^ u^*$ induced by turbine-impacted waves. Momentum flux times mean velocity is not a tendency of kinetic energy. (Negative) vertical gradient of momentum flux times mean velocity can be a tendency of mean kinetic energy. If the momentum flux really had this (constant with height) value up to 100 m and then became zero, you would have infinite accelerations at 100 m, and no accelerations above and below.

*) In reality, vertical turbulent momentum mixing in WRF is done with a turbulence closure parameterization, usually a function of the TKE equation and some diagnostics. Momentum flux convergence / divergence is one of these terms. The only role of surface roughness / momentum stress in this framework is as a lower boundary condition such that these divergences can be computed.

*) I would also say that there is not generally a 'constant momentum flux layer' in the atmosphere -- it is more correct to say that the surface layer is defined as a layer thin enough such that the momentum flux may be treated as approximately constant. Since it generally decreases on the scale of the boundary layer height, this is generally 10% of that value. But this means that wave impacts on surface roughness can cause drag to be transmitted throughout the depth of the PBL given enough turbulent transport.

*) Less essentially, it should be noted that 100 m can be well above the surface layer in marine conditions, in which case a constant stress formulation would be inappropriate.

*) In summary, without working through all details, I would advocate for wave-roughness modifications on momentum flux to be applied at the surface only, and then either transmitted throughout the PBL by the PBL parameterization, or perhaps somehow specified to decrease with height to the boundary layer top as long as double counting is avoided.

Minor comments:

Line 276: You say that ‘the model is run for an additional 150 hours for further validation (0000 UTC 01 January to 0000 UTC 07 January)’ but 00 UTC 01 Jan to 00 UTC 07 Jan is six days = 144 hours, and the six hour period for all later analyses falls within this interval. So I am still a little confused about the time intervals involved.

Response to comment 12, line 351: I could quibble about the degree that the Taylor Yelland parameterization is iterative. While this is technically true, the iterative aspect of the scheme is in a viscous term that is actually derived from laminar theory, and in practice provides a roughness length in conditions for which wave roughness is zero. In regions of interest for wave roughness, this term should hopefully have negligible effect, in which case iteration is not needed to determine the roughness height.

However, it is certainly true that the similarity schemes and turbulence physics of WRF overall are complex, and there are a number of reasons why reductions of SWH might not increase near-surface wind speeds. What I might suggest is to just remove the reference to ‘(Taylor and Yelland 2001)’ here but leave it in Table 1.