In this paper, the effect of wave breaking and Stokes drift on the mixing in the near-surface layer is studied with a focus on the vertical and lateral transport of buoyant particles (mostly oil droplets from an oil spill). Many of these effects have been investigated in detail in previous studies but, as far as my limited knowledge on the modeling of oil spills allows me to overlook this, the combined study of wave effects, vertical mixing, and its influence on the effective lateral transport of an oil patch seems to be new. So there is some moderate progress that may make this paper eventually worth being published. The present version, however, contains numerous inaccuracies and questionable assumptions, and therefore requires a major revision before the paper should be further considered for publication. These are my major points:

1. Upper ocean mixing depends crucially on stratification. In the context of this paper, it is particularly important to note that the lateral transport of oil droplets in the near surface layer depends on the depth to which these particles are mixed down. This depth, however, is extremely sensitive with respect to the presence of thermal or haline stratification, even if stratification is only weak. This point seems to be completely ignored here. No transport equations for temperature and salinity are discussed in combination with the governing equations (6), and although buoyancy terms are added to the TKE budgets in (9) and (10) it remains unclear on which basis these terms are computed and how important they are. In the idealized simulations described in Section 3, stratification effects seems to be ignored, which is highly unrealistic in view of the "500 m deep ocean column" used in these simulations. Also for the more realistic simulations of the oil spill described in Section 4, it is not sufficient to only mention that "observations show very little stratification" as they authors do (line 3, page 1284).

   Therefore, the authors should improve (the description of) their model to include stratification effects, and demonstrate the effect of this. In particular, in the case of the realistic simulations, vertical profiles should be used to illustrate the initial stratification, its temporal evolution, and its effects on the vertical droplet distribution.

2. In this study, a one-dimensional model is used to study the lateral transport of suspended oil droplets. One thing that is overlooked here is that shear-dispersion may have an important effect on the lateral spreading of the oil spill. This effect is not included in any one-dimensional model. If the authors believe that shear-dispersion is not important here, they should provide an estimate for this, and explicitly show that shear-dispersion can be ignored. Otherwise, they should find a way to include it.

3. One thing that really puzzled me with this manuscript is the way the authors verify their model results. The only available data for the localization of the oil slick seem to be from an overflight 2 days after the accident (page 1284). In the manuscript, these data, obviously only representing the surface signature of the oil patch, are compared with the depth-averaged transport estimate in (31) that depends on the vertical distribution of both velocity and concentration. This comparison between surface and depth-averaged quantities doesn't seem to make much sense. Similarly, in numerous places in the manuscript (e.g., on page 1282, lines 21-24), the classical surface drift speed estimate of 3 % of the wind speed is criticized as being too large compared to

the transport velocities estimated from the model. The latter, however, is based on vertically averaged properties, and I find it little surprising lead this leads to smaller values and a larger deflection with respect to the wind direction. I suggest that the authors directly diagnose the surface transport velocities from their model, and compare this to the airplane data and other surface-related estimates.

4. It is known that Langmuir turbulence has a much stronger effect than wave breaking for the dynamics of and mixing in the surface layer (e.g., D'Asaro et al., 2014). The inclusion of Langmuir turbulence, however, requires a modification of the stability functions introduced in (12), as suggested, e.g., by Harcourt (2013). The authors seem to be completely unaware of this complication, and they do not even mention which stability functions they use for their study. The required modifications of the model for the inclusion of Langmuir turbulence are probably too severe to be done even in an extensive revision of the paper. The authors should, however, make themselves acquainted with the available literature in this segment, and carefully describe why they decided not to include Langmuir turbulence, and which limitations are implied by this regarding the quality of their predictions.

5. In Eq. (17) the authors use a dimensional argument to come up with an expression for the turbulent length scale at the surface. I think, however, that the dimensional analysis has not been carried out correctly. The authors are looking for a non-dimensional relationship between 5 dimensional quantities: ϕ_{oc}, l, T, g, and ρ_w. All dimensions can be constructed from 3 SI units: kg, m, and s, which implies a non-dimensional relationship between 5-3=2 non-dimensional products, e.g. between $\Pi_1 = l/(gT^2)$ and $\Pi_2 = l^2 \rho_w g /(T\phi_{oc})$. The dependency on Π_1, however, is ignored here, and I don't see any reason why this should valid. Since I find the physical motivation of Eq. (17) somewhat obscure anyway, I suggest dropping this part of the analysis and working with a more standard expression for the value of l at the surface.

6. Finally, a more technical point that may, however, turn out to be important. The k-omega model used in this study is known for its "free-stream" sensitivity. This means that the turbulent diffusivity below the transition from the turbulent mixed layer to the non-turbulent interior shows an excessive and unphysical dependency on the prescribed minimum or background values of k and omega. This may lead to unrealistically high diffusivities in a region that is in fact non-turbulent. This is briefly mentioned in the description of the GLS approach by Umlauf and Burchard (2003) along with some references from the engineering literature. I mention this here only because on the top of page 1282, the authors point out that they find mixing of particles down to very deep regions with small turbulent activity - because the diffusivity is very high. This sounds suspiciously like one of the symptoms of the free-stream sensitivity problem mentioned above. These model runs should therefore be repeated with the k-epsilon model (which does not exhibit this problem) to see if the high diffusivities far from the surface are a robust result (should be easily done by changing a few lines in the GOTM input files). I would like to see vertical profiles of k, length scale, and diffusivities from both runs. It may turn out to be preferable to recompute the results with the k-epsilon model. Adding mild stratification and modifying the minimum values for k and omega may also solve the problem.

Minor points

Eq. (4): Symbol ***k*** undefined

Eq. (5): Symbol *F* undefined

Page 1272, line 13: Symbol $\hat{k}$ undefined

Eq. (6): Symbol ν undefined. Also, the meaning of primes and the overbars should be explained. Do the fluctuations (indicated by a prime) include fluctuations due to waves? What about stratification effects? Are they completely ignored here (hopefully not)? Otherwise, transport equations for temperature and salinity should be supplied.

Eq. (8): The momentum input from breaking waves may sometimes be spatially distributed across the upper few meters of the water column. Here, however, it is assumed that all momentum input occurs directly at the surface. Please provide a reference to justify this.

Eq. (9). Clash of notation: Symbol *k* is already used for the (magnitude of) the wave number vector. Also, the advective TKE flux (term in brackets on the right hand side) is incorrect: The expression $\overline{w'k}$ doesn't make sense because the variable *k* is already an averaged quantity. The variables *b* and *p* are undefined, and the sign of the buoyancy flux is wrong (assuming that *z* points upward).

Eq. (11). Sign of buoyancy flux is incorrect (see previous point).

Eq. (18). Symbol ν'_t is undefined. What is the initial condition for the concentrations?

Page 1276, lines 20-26. What means "steady state conditions" in the context of a "500 m deep ocean column". I could imagine that it takes a pretty long time until a steady state is reached. What is the spin-up time for these runs (and for the realistic ones described in Section 4)?

Eq. (25). Did the authors mean C(z-> - ∞) = 0 instead of the second expression in this equation? This is at least what Eq. (26) suggests.

Fig. 7, lowest panel. How does the estimate αu_*^3 compare to these values?

References

D'Asaro, E. A., J. Thomson, A. Y. Shcherbina, R. R. Harcourt, M. F. Cronin, M. A. Hemer, *and* B. Fox-Kemper (2014), Quantifying upper ocean turbulence driven by surface waves, Geophys. Res. Lett., 41, 102–107, *doi:*10.1002/2013GL058193.

Harcourt, R. R., 2013: A Second-Moment Closure Model of Langmuir Turbulence. J. Phys. Oceanogr., 43, 673–697. doi: http://dx.doi.org/10.1175/JPO-D-12-0105.1