Review #1, #2 and #3 with comments

Anonymous reviewer 1

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. Stratification

   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.

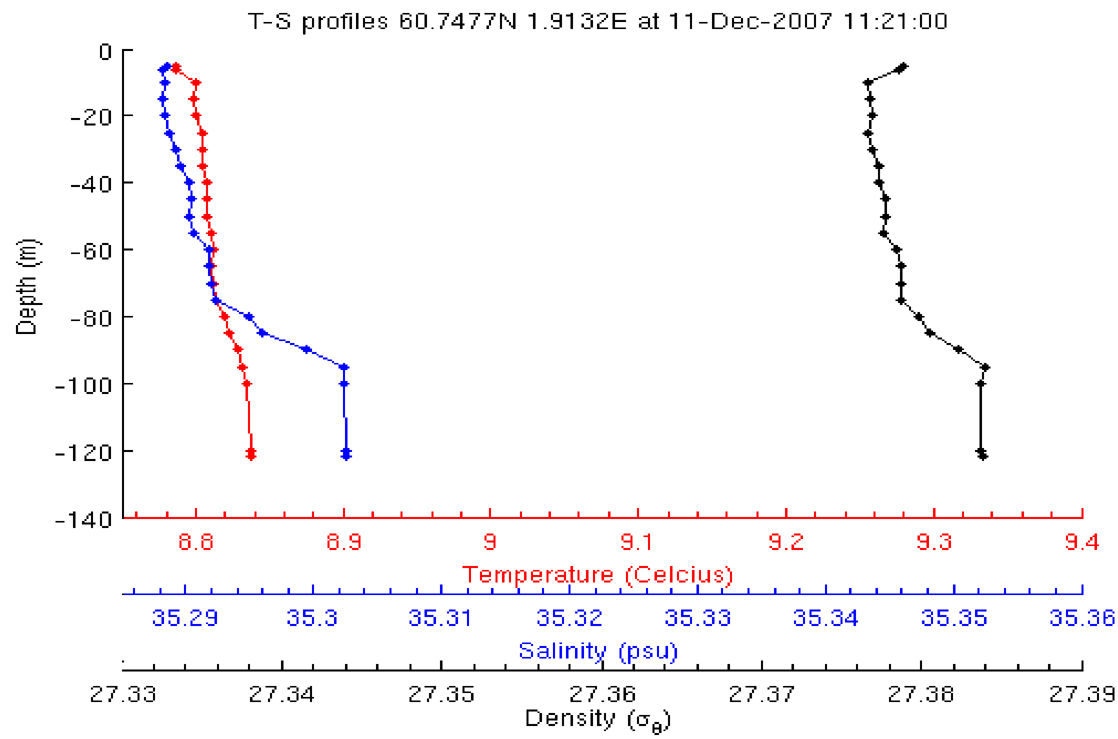
   Author reply

   We have added more about stratification in the model description in chapter 2, and included transport equation for active tracers.

   In addition we have illustrated the net shortwave radiation, net heat flux and precipitation from ERA Interim used as model input for the realistic case (see Illustration 1 below). In general there is a (weak) net cooling at the surface during this period, and some of the effects of the fluxes are adressed in section 4.

   The steady state analysis (section 3) is based on the classical Ekman theory using a constant eddy viscosity. The theory builds on an assumption that the depth of the ocean is much larger that the Ekman depth, and this was the basis on which the ocean depth was chosen for the idealized experiments. The maximum Ekman depth in the experiments (as calculated from the momentum profile Eq. 31) is roughly 50 m and we *chose a depth D that is 10 times this value (i.e., D/DE>10>>1 as is required by theory). The reason for not including stratification in the idealized experiments is i) that classical Ekman theory does not include stratification and ii) there is no steady state solution for the case with a freely evolving stratification. We expect that steady state will evolve in roughly the rotational period (according to Ekman theory), in practice we expect that it may take several, say, 10, rotational periods to come close to steady state. Having said this, for the simulations we did actually integrate for a long time (over 1 year as we wanted, say, two inertial periods between each output points) with a slowly increasing stress at the surface simply because it did not require more than a few minutes simulation. In this way we were sure that e.g. inertial oscillations did not influence our results. We have done the same simulation but on a shorter timescale and there were no visible differences.*

The effect of stratification is taken into account in the more realistic case of the Statfjord A oil spill in section 4, and we have added more to the discussion of stratification effects in this section. Since there were few observations of the hydrography close to the Statfjord A platform during the time of the oil spill, we had limited possibility to compare modeled profiles with CTD measurements, but we have here included the two closest observations (in time and close to the platform): one before and one after the accident (See figures on the next page). For this period of the year in this area we expect little stratification, which is confirmed by the CTD's. In addition, stratification from model runs show little stratification at this position during this time of the year (not shown). The closest observation before the accident (first figure next page) was used to initialize the model, and running the model for the period of the accident the modeled ocean column becomes more homogeneous (similar to the closest observation after the oil spill shown in the second plot on the next page). Since the two observations are quite far apart we have not done any direct comparisons of the model with the last observation, but we note that the ocean column becomes more homogeneous during the period as the last observation indicates. The CTD plots have not been included in the updated manuscript, but can be included if the reviewer insists. The initial stratification is included in Illustration 2, which also shows that how the model stratification and the concentration profiles evolve in time. A discussion of the evolution of the stratification and concentration profiles is included in section 4.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-140
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Temperature (Celcius)
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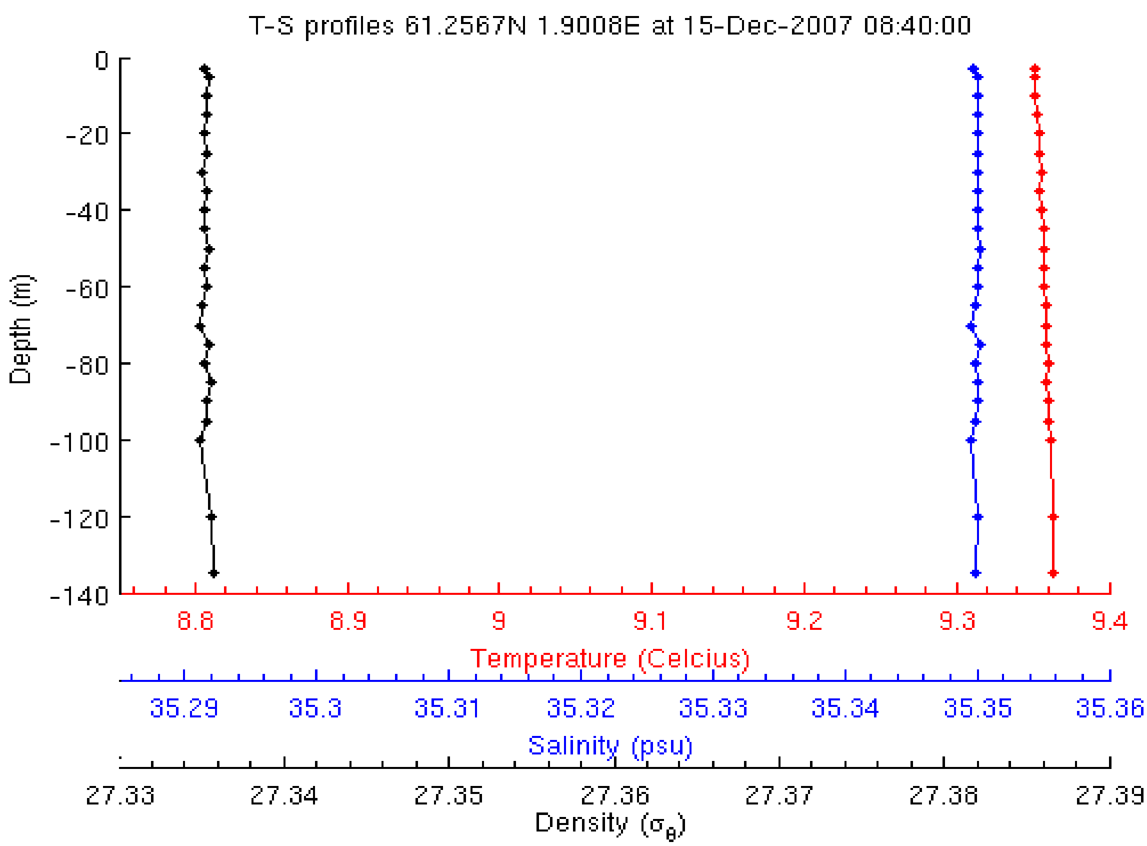
Density (σ_θ)


T-S profiles 61.2567N 1.9008E at 15-Dec-2007 08:40:00
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2. <u>Shear dispersion</u>

   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.

   <u>Author reply</u>

   This is a good point, we have added some comments on this issue in section 5:

   "Shear dispersion is not included in these experiments. In the Statfjord A case, the difference between the average drift in the upper 5 meters is about 25% higher than the average drift between 5−10 meters during the first 24 hours following the accident. The difference in results for different rise velocities (Fig. 13) to some extent also indicates the potential role of shear dispersion. We emphasize, however, that our ultimate goal is to use a similar mixing scheme in a full 3D model, which is likely to produce more realistic results. We therefore do not consider further parameterizations of the effect of shear dispersion here."

3. <u>Verification: Model vs observation</u>

   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.

   <u>Author reply</u>

   From the reviewer comments it is clear that we need to rewrite and clarify the verification of the model and elaborate some more the motivation and purpose of the study.

   The mentioned reference by the reviewer to p. 1282 is probably meant to be p.1281. In these idealized experiments the comparison between the vertically averaged model drift rate with the empirical rule is, as the reviewer comments, unreasonable and is rephrased. The intention here was to stress that the net drift also approaches a linear relationship with the wind speed at some angle to the wind direction. As the reviewer *points out, the value of the drift and the angle with the wind should not be expected to be the same as the empirical rule for the surface drift, and we have clarified this on p. 1281.*

The comparison of the depth-averaged drift from the model with the observed oil slick can be justified by the severe wind and wave conditions during the time from the start of the oil spill to the observation. Under such conditions it can be expected that the oil would drift more similar to the 1% 90° rule suggested by Reed et al. (1994) for wind speeds exceeding 6 m/s, which is not a surface-related estimate. Under such conditions the oil often becomes visible at the surface only after a calmer period, when more and more particles resurface. The wind speed during the Statfjord A oil spill was well above 6 m/s (Illustration 1), and probably the oil was quickly mixed subsurface and remained so for the majority of the time until the observation, which was made after a period of calmer conditions. This is probably also why it was observed. To clarify this point we have added to the introduction p. 1267

To make the point of this study clearer, we have elaborated more on the motivation of this study in the introduction, and in ch. 4: One of the main motivations of using the Statfjord A oil spill was that the several of the 3D models used to predict the oil drift completely missed the observation, and predicted a drift more similar to the empirical surface-drift (3% 15°) rule by Reed et. al 1994 (for an overview see Hackett et al. (2009). An apparent reason of the models missing the actual oil drift is that there is too little mixing, and we believe that better parametrizations of the wave-related mixing and transport can improve drift models. Since these wave-induced effects are more easily isolated in a 1D model the idea was to start to implement it in GOTM and perform a thourogh sensitivity analysis before applying these effects in a fully 3-dimensional model. In this paper it was not our intention to criticize the empirical rules. In fact, when including the background current from the SVIM archive (Lien et al., 2013), the empirical rule suggested by Reed et al. (1994) of 1% 90° agrees quite well with the observation (Illustration 3). The larger drift speeds predicted by this empirical rule may well be due to uncertainties of the magnitude of the added background current. While clearly the empirical rules in practice often work surprisingly well (and in the Statfjord A case actually better than many of the oil drift models shown in Hackett et al. (2009)) clearly there is a weakness in both the need for a certain threshold value in wind speed to separate between surface and subsurface drift, as well as the lack in physical understanding of the problem.

4. Langmuir Turbulence

   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.

<u>Author reply</u>

We agree that there was too little emphasis on Langmuir turbulence in the manuscript (also the other reviewers mention this), and we have put more weight on discussing this in the revised paper, adding reference to important papers on the subject. In the introduction we have added a paragraph on Langmuir turbulence:

"Surface waves are associated with large-scale coherent structures, commonly referred to as Langmuir turbulence, which affects the mixing in the upper layer (e.g. McWilliams et al., 1997; Kantha and Clayson, 2004; Harcourt and D'Asaro, 2008; D'Asaro, 2014). The interaction of the Stokes drift with the current through a vortex force gives rise to an instability known as the second Craik-Leibovitch (CL2) mechanism, that causes Langmuir cells to develop (e.g. Craik, 1977; Leibovich, 1983). The effect of Langmuir circulation (LC) on the turbulence in the ocean mixed layer has been studied using large eddy simulations by e.g Skyllingstad and Denbo (1995) and McWilliams et al. (1997), who find that including this effect results in elevated values of turbulent kinetic energy and dissipation. While the effect of wave breaking is restricted to the uppermost meters of the ocean, Langmuir turbulence affects the entire mixed layer and is more important for mixed layer deepening (Kantha and Clayson, 2004; Kukulka et al., 2010). Recent studies have shown that this may impact the global climate system through air-sea exchanges (Belcher et al., 2012; D'Asaro et al., 2014). Stokes drift shear has been used in turbulence models as a parametrization for increased turbulence when Langmuir cells are present (e.g. d'Alessio et al., 1998; Kantha and Clayson, 2004; Janssen, 2012).However, a production term of TKE proportional to the Stokes drift shear can also be derived from Generalized Lagrangian-mean theory (Ardhuin and Jenkins, 2006), that is not directly related to Langmuir turbulence. In this study we include a Stokes shear production term in the main governing equations. It should be noted that more sophisticated models exist (Harcourt, 2013), but this leads to more model constants that need to be determined."

In section 2 we have specified which stability functions we use, and we have added a short discussion in section 5 where the modifications by Harcourt (2013) and the model by McWilliams et al. (2012) are mentioned. Since this work is part of an effort to eventually improve the operational drift models, the focus has not been on the most comprehensive parametrizations, but rather on the changes that could be added to the existing systems with reasonable efforts. Investigating the effects of the more elaborate parametrizations such as the model by Harcourt (2013) on particle transport is however a very interesting topic for future work.

As the reviewer points out, several studies indicate that Langmuir turbulence has a stronger effect on the dynamics and mixing in the surface layer than the effect of wave breaking (that is confined to a layer closer to the surface). We would like to stress that we here consider buoyant particles, which make the surface-near processes more important (and more so the more buoyant the particles are). In this context it is not clear *that Langmuir turbulence is more important than wave breaking for the net drift, and the relative importance probably depends on the rising speed of the particles.*

5. Turbulent length scale: surface boundary condition

   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 $\Pi_1 = l / g T^2$ and $\Pi_2 = l^2 \rho_w g / (T \Phi_{oc})$. The dependency on Φ_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.

   Author reply

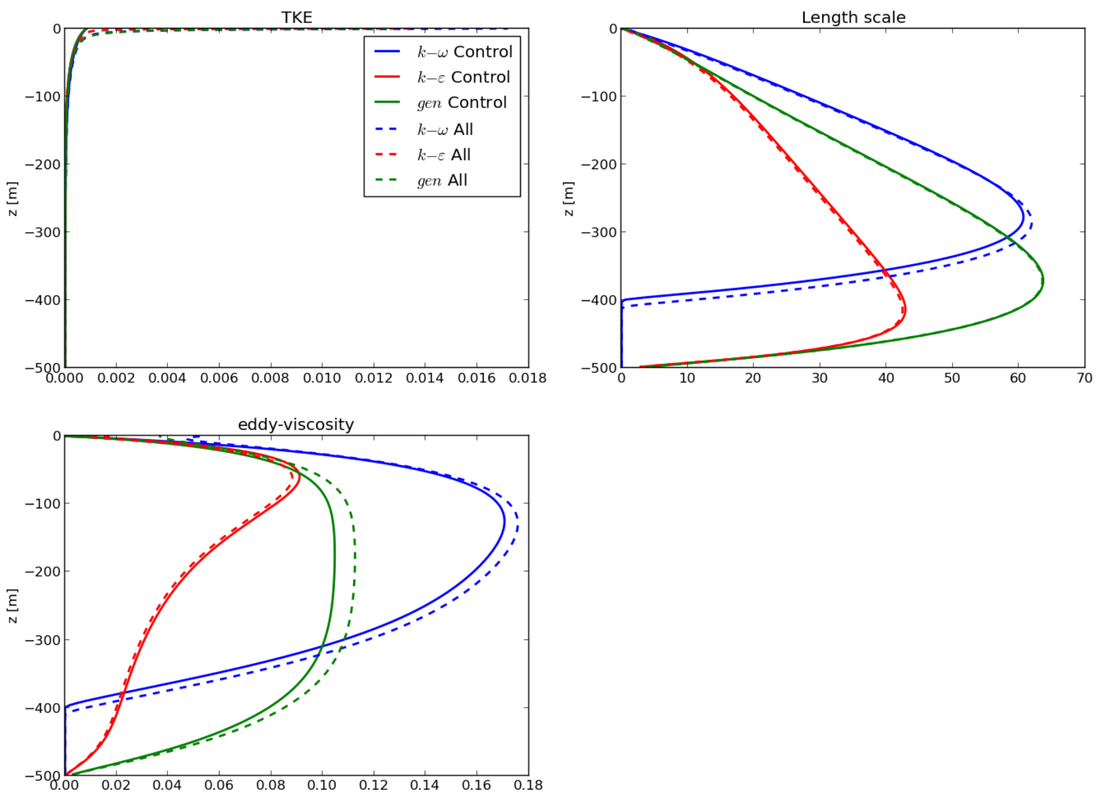
   As the reviewer points out, the physical motivation for defining a new boundary condition is perhaps a little weak, and for the present study reasonable results can be achieved using more standard expressions. Therefore we have changed this part of the analysis and rather used the value suggested by Soloviev and Lukas (2003), i.e. $z_0 = 0.6 \mathrm{H}_s$.

6. The k-omega model and free-stream sensitivity

   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.

<u>*Author reply*</u>
The strong mixing well below the Ekman depth in the steady state experiment (section 3) is a consistent feature of the model and applies to all three versions of the two-equation models (k-omega, k-epsilon and the gls). While it is not important for momentum it may be important for the mixing of tracers and buoyant particles. Thus, rotational effects that are not included into the 1-D model formulation may become important for the case of weak (or unstable) stratification that may reach considerable depth into the ocean during winter. If this is physically sound or not is beyond the scope of the present study but principally it will require a time scale of eddies that is larger that the rotational period. Anyway, we find it worthwhile to mention this feature of the mixing model.

Plots for Steady state experiment with tau = 0.25 (not included in the revised manuscript):



7. Minor points:

- Eq. (4) **k** undefined

  Wavenumber vector **k** *has now been defined and to avoid confusion between wavenumber and TKE we have rather denoted wavenumbers by κ*

- P. 1272, line 13: Symbol $\hat{k}$ (hat{k}) undefined

  This is the unit vector in the z-direction, and is defined on p. 1269, line 22

- Eq. (6): Symbol υ 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.

  Definition added to p. 1272. The fluctuations indicate the turbulent fluctuations, and do not include fluctuations due to waves. Stratification effects are included, and active tracer equation added (see also point 1).

- 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.

  We have provided a reference and some comments to justify this below eq. (8):

  "In principle the part of the momentum flux in (4) that comes from wave breaking is distributed in the upper few meters of the ocean, but the explicit form of this wave breaking stress is not known, and there is no clear consensus on how it should be distributed (e.g. Janssen, 2012). However, results from Saetra et al. (2007) indicate that this has little effect on the currents, and in this study the total momentum flux is given as a boundary condition at the surface as in (8)."

- Eq. (9) . Clash of notation: Symbol k is already used for the (magnitude of) the wavenumber 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 since 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 z is pointing upwards (also equation (11)).

  The wavenumber is now defined by κ, to avoid confusion. The variables b' (buoyancy fluctuation) and p' (pressure fluctuation) have now been defined on p. 1272.

  Several authors use the same notation, $\overline{w'k}$, (e.g. Janssen, 2012 as well as Grant and Belcher, 2009 who we cite), but strictly speaking the reviewer is right. We have rewritten the transport term, using: $\frac{-1}{2}\frac{\partial}{\partial z}\overline{w'(u'u'+v'v'+w'w')}$.

  The sign of the buoyancy flux is wrong, as the reviewer points out, and has been corrected in (9) and (11).

- Eq. (18): Symbol ν_t' is undefined. What is the initial condition for the concentrations?

  The symbol ν_t' denotes the eddy diffusivity introduced in (11). The concentration is initialized by uniform distribution in the vertical at the time of the oil spill (Illustration 2b). It should be noted that the oil leakage was from a loading hose at some depth below the surface and not onto the surface. The "real" initial oil profile is therefore unknown. By initializing with a uniform concentration profile we assume the oil was evenly distributed whitin the ocean column, which we believe is the most realistic.

- P. 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)?

  For the experiment described in section 3 we have neglected the effects of stratification in order to compare with classical Ekman theory, and an approximate steady state is then reached by increasing the wind stress forcing linearly very slowly (see discussion below point 1). Notably steady state could have been reached by simply using a constant forcing, but then the model would have needed even longer time to adjust. In our experiments we ran the model over a year, but in earlier tests we found that in principle a couple of weeks were enough (with slowly increased wind stress).

  For the realistic case in section 4, the model was run with wind and wave forcing that was first gradually ramped up from rest (over 24 hours), and then run with constant values for several months to reach a quasi-steady state until the time of the nearest CTD profile before the accident. At the time of that measurement, the model was restarted with the T and S profiles from the CTD used as initial profile and initial velocities from the spinup. From that point the model was forced by wind, waves, radiation and precipitation (as shown in Illustration 1).

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

  This is right and has been changed accordingly.

- Fig. 7, lowest panel: How does αu_{star}^3 compare to these values?

  The Craig and Banner parametrization is now added to the figure (with $\alpha = 100$, see Illustration 1).

Anonymous reviewer #2

I have read with interest the paper in Subject. Indeed, there is the need to further investigate the effect of wave breaking and Stokes drift on near-surface mixing situations.
The fact of intersecting this with the oil issues is an interesting idea. Unfortunately, the authors do not succeed in making the fundamental of these processes clear and their assumptions and conclusions are heavily questionable. As it looks now, the paper is not acceptable and should be very revised or rejected. I encourage a resubmission after a very deep revision that is taking intop consideration state-of-the-art and fundamental papers on these topics.

1. Page 1268: The authors appear to ignore earlier pioneering literature on various topics. For example, McWilliams et al. (JFM 1997) were the first to talk about "Langmuir turbulence" and the influence of Coriolis-Stokes term. However, this reference is missing and instead the credit seems to be given to Rohrs et al. (2012). In the same way, Kantha and Clayson (2004) were the first ones to include Stokes shear production in mixing models. Yet Eq. (9) is referenced to Grant and Belcher (2009) which appeared five years later with the same equations as in Kantha and Clayson. In any case, contrary to assertion by the authors, Kantha and Clayson (2004) and Kantha et al. (2010) do not use the vortex force term per se, but includes Stokes production term in both the TKE and turbulence length scale equations.

    We have made sure to focus more on Langmuir turbulence (See also reply to reviewer 1), and the references that are mentioned have been cited in the updated version. We are aware that the Kantha and Clayson (2004) and Kantha et al. (2010) model, does not use the vortex force. It was not our intention to give this impression, and we have rewritten part of the introduction to be more specific in the text to underscore what we mean. Our impression is that many authors (including the ones mentioned above and Janssen (2012)) include the Stokes shear term to take account of increased turbulence when waves are present, and in effect that this is a parametrization of the increased levels of turbulence due to Langmuir turbulence (Janssen (2012) at least says this). In our model the Stokes shear term is added to the TKE and length scale determining equations in a similar manner as Kantha and Clayson (2004), except that we use a different two-equation model (GLS).

2. The authors appear not to be well focused about Langmuir turbulence. They should refer to McWilliams et al. (1997) and Kantha (2012) review. Huang and Qiao (2010) formulation is questionable, as recently shown by Kantha et al. (JGR, 2014) and so the authors' citing the reference as proof of how the dissipation rate behaves is not a strong subject. They have to get to Kantha et al. 2014 paper and either rebut to those findings, or change your current statements.

    Langmuir turbulence has been treated in more detail (see prev. point and reply to reviewer 1, point 4). The Huang and Quiao (2010) reference on page 1268 has been removed since it is of little relevance to the current study and since we agree with the reviewer (and Kantha et al., 2014) that the formulation by Huang and Quiao (2010) is heavily questionable. See also next point.

3. Last, it is hard to believe that the law of the wall scaling prevails when waves are breaking strongly. Further support should be given to this discussion.

   We think the reviewer has our reference to Sutherland et al. (2013,2014) on page 1268 and on page 1285 in mind when he states this. We realize that the comment about law of the wall-scaling was unprecise and we have therefore rewritten the introduction without this reference and changed the text on page 1285 to

   "....Such observations have led to several studies proposing scaling laws of ε *other than the classic law of the wall (e.g. Anis and Moum, 1995; Terray et al., 1996;Huang and Qiao, 2010). Observations in the mixed layer using free rising profiler in some cases match the law of the wall scaling quite well, but general agreement is found with the Langmuir turbulence scaling of Belcher et al. (2012), provided the vertical scale is given by the mixing layer depth rather than the mixed layer depth (Sutherland et al., 2013, 2014). The scaling suggested by Huang and Qiao (2010) has recently been questioned on physical grounds by Kantha et al. (2014)."*

4. Page 1269: It is more justifiable to calculate wave-related quantities such as Stokes drift from the wave spectrum; here the authors seem to be on the right track. If the oil stays on the surface, then it is the surface current that determines its drift and so the 3% rule and 15° angle are plausible. But when the oil is mixed down into the mixed layer by very strong mixing as in Statfjord 2007 spill, it is not surprising that its drift speed comes down and the angle is closer to the classical Ekman transport at right angles to the wind.

   This is true, but even so several oil spill models used after the Statfjord A accident predicted a drift not very far from the 3% 15° empirical rule (compare e.g. Hackett et al (2009) Fig. 2. with the observation plotted in Illustration 3). One of the reasons for this is probably that there is too little mixing since the wave induced mixing and transport is poorly represented in these models. Here we wanted to investigate how the parametrization of wave breaking, Stokes shear production and the Coriolis-Stokes force presented affect the drift of particles of different buoyancies, to see if and possibly how much this had the potential to improve the existing oil drift models. We have elaborated this some more in the new version.

5. Minor points:

   - Page 1271: Mention McWilliams (1997) in addition to Polton et al. (2005)

     We have now mentioned McWilliams (1997) on p. 1271

   - Page 1272: Mention Kantha and Clayson (2004) in addition to Grant and Belcher (2009)

     We have added a citation to Kantha and Clayson (2004).

   - Page 1273: Mention Kantha and Clayson (2004) in addition to Umlauf and Burchard (2003) since they also included Stokes production term

     We have cited Kantha and Clayson (2004) for the inclusion of a Stokes shear production term in the length-scale determining equaiton.

   - Page 1279: See comments on page 1269.

     See reply to point 4.

- Page 1285: When there is strong wave breaking, it is hard to expect law of the wall near the surface. Law of the wall may prevail deeper below the wave breaking zone.

    See comments below point 3.

- Fig. 2: Best plotted to the same scale

    Since plotting the normalized particle depth for rise-velocities of 100m/d for wind stress up to 1 N/m^2 would make it very difficult to see where the blue and red lines cross, we plotted the two panels with different x-axes (as denoted in the caption). We would like to keep it this way, but if the reviewer insists, we can change this to show up to 0.5 N/m^2 for both cases.

- Fig. 10: I would show the wind direction.

    Good idea, We have added a wind barb showing mean wind strength and direction during the period (see Illustration 3).

- Eqs (4)-(5) and (9): Symbols are sometimes undefined and sometimes used twice. Please go through turbulence notation more carefully.

    Similar comment in minor points by Reviewer #1. Have gone through and corrected.

Anonymous reviewer #3

This paper deals with the dispersion of pollutants by waves. The authors particularly investigate the effects of the mixing induced by wave breaking on drift. First, they validate their modeling strategy with well-known solutions. Second, the Statfjord case is set-up. The authors found that the effects of wind and waves play an important role in oil dispersion. On the whole, the paper shows results in agreement with former ones. The state of art is poor and must be rewritten. Some of major publications are missing like the one of McWilliams et al (1997). The authors could pursued their study by the improvement of the parameterization used for the mixing induced by waves. Indeed, I think their study lacks of new things. So, I recommend a major revision.

> <u>*Author reply*</u>
>
> *We have added more text about Langmuir turbulence, and we have included key references. The changes are described in detail in response to reviewers 1 and 2. As far as we know, the application of particles with a rise speed is new in this context. It represents a very real life problem for describing drift of e.g. oil an fish eggs, as is commonly described/forcasted by operational institutes. Thus we argue that this is a new and important study, and results from this study will find its way into these communities.*

Specific comments from the reviewer:

- Page 1280: In contrast with the arguments of the authors, I think a discussion on the value of X is fundamental because comparisons between the values given in Rascle et al. (2006) and in Csanady (1982) were performed (Figure 1). How your X value is computed? I have not found the value of X in the manuscript.

  The reviewer notices that the value of X is important for model vs theory comparison. We notice that the agreement between theory and model is quite good for depth averaged variables such as Ekman depth and the characteristic concentration depth, but worse for the surface velocities (and surface concentration of particles, not shown). We notice that different values are needed for surface values and for depth integrated quantities: the reason for this is likely that the turbulent diffusion coefficient is far from constant (especially close to the sea surface). Thus surface values are more controlled by near surface features of the turbulence while bulk quantities are more controlled by a depth averaged mean. Anyway, when the depth profiles of momentum and particles are similar, we expect that they will experience the same depth profile in the eddy mixing, and we use this point (i.e., DC/DE=1 for the all wave effect model) to determine the value of X, and it is thus the same in Figs 2 and 3 but depends on the rise velocity. A more elaborated analytical theory needs to be developed for more detailed theory vs model comparison but this is beyond the aim of this study.

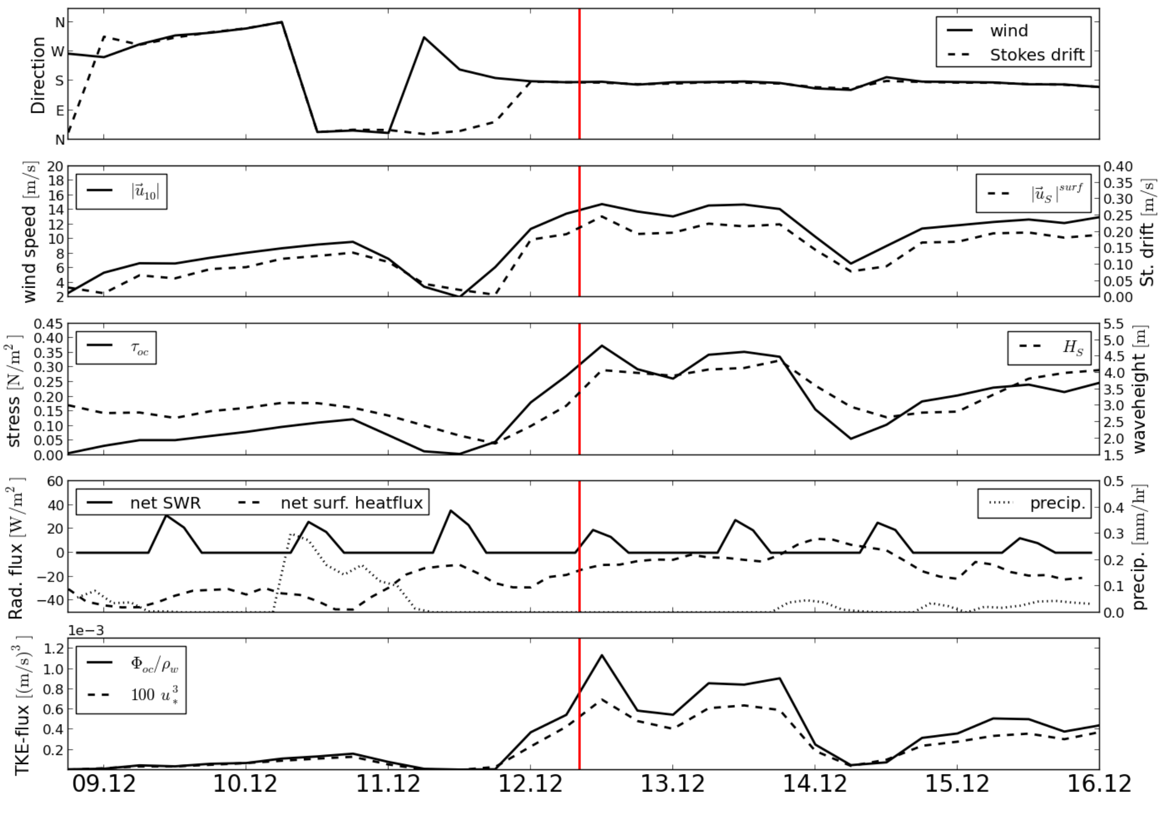
- Page 1292, Figure 2: What are the dashed lines? Can you specify this in the legend.

  The dashed lines are theory, i.e., Eq. 28 for figs 2 and Eq. 30 for fig 3. We have now specified this in the legend. The value of X is now given in figure text.

- Page 1293, Figure 3: same as previous remark.

  We have specified this in the legend

Figures that are new or changed in manuscript



1: Fig 7 in manuscript:

Wind and wave conditions from ERA Interim before, during and after the Statfjord A oil spill. The incident time of the oil spill is indicated by the vertical red line. From top to bottom, the panels show: Wind and Stokes drift direction; wind and Stokes drift magitude; effective momentum flux to the ocean and the significant wave height; net short-wave and net surface heat flux and precipitation; TKE fluxes into the ocean as calculated from the wave spectrum and from Craig and Banner (1994) parametrization with   = 100.

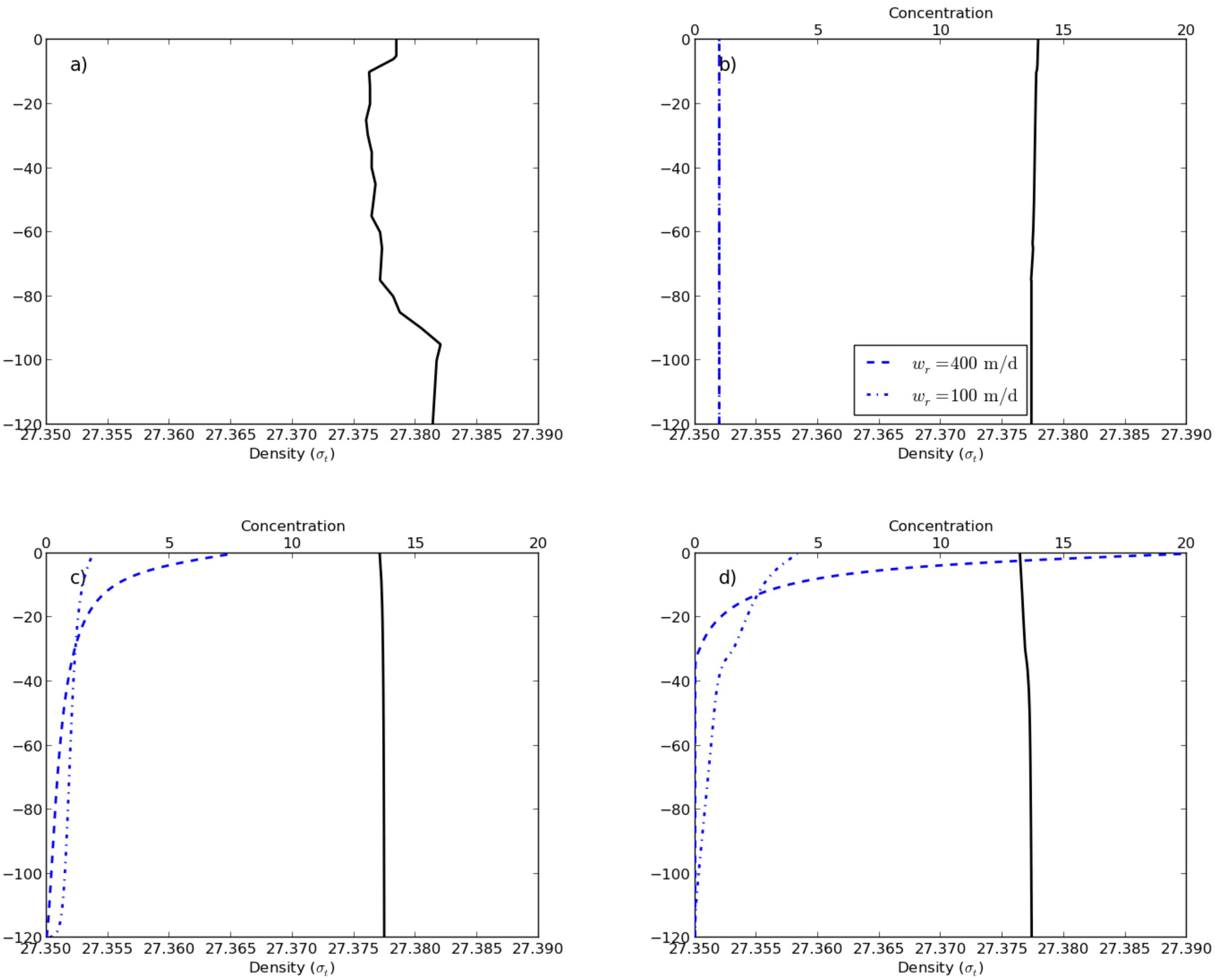


Illustration 2: Time evolution of density (black lines) and particle concentration profiles (blue lines): a) Initial density profile (2007-12-11 11:21), b) Approximately at the start of the oil spill (2007-12-12 08:30), c) 2007-14-14 00:00 d) 2007-14-14 14:00.

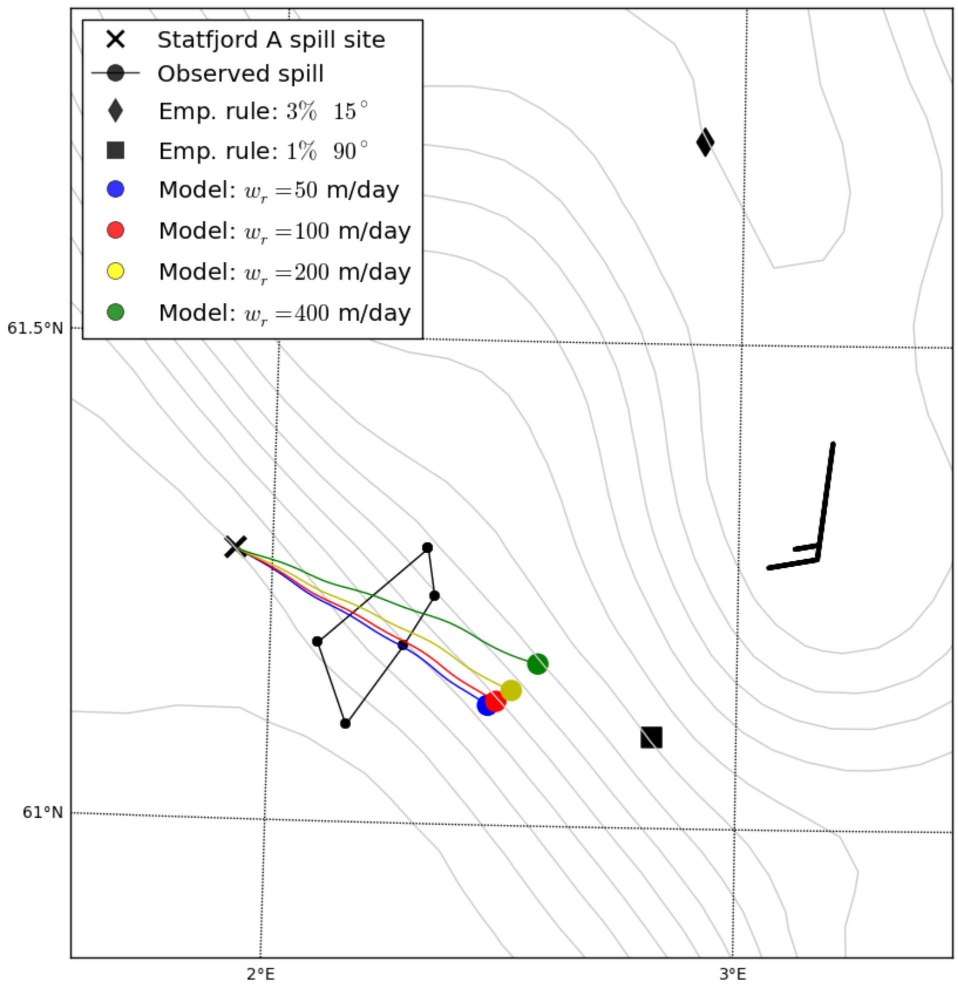


Illustration 3: Figure 10 in manuscript: Wind barb illustrating mean wind from the start of the spill until the observationis added.Mean location of oil predicted by the model for different rise velocities of the particles. Also shown is the mean locations predicted by empirically based relations between the drift and the wind vector. The observed oil slick is shown with coordinates from observation (2007-12-14 13:48 UTC) connected with lines.

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