Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-130-AC3, 2019

Ocean Science
Discussions
Open Access

Interactive comment on "Revisiting the DeepWater Horizon spill: High resolution model simulations of effects of oil droplet size distribution and river fronts" *by* Lars R. Hole et al.

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Received and published: 20 February 2019

[12pt]article We would like to thank this reviewer for taking the time to provide very detailed and specific comments to our manuscript. Below are our responses with the reviewers comments in bold letters.

General Comments This paper presents simulations of oil transport during the Deepwater Horizon (DWH) accident using a Lagrangian Particle Tracking model with oil fate processes (OpenOil) coupled to an ocean circulation, wave, and wind model (GoM-HYCOM, WAM, and ECMWF), run under different freshwater discharge scenarios through the Mississippi delta system. While such a study

is important and in general the modeling systems used are adequate for the task, the present manuscript lacks the information needed to critically assess the modeling results.

Very broadly, the article lacks the clarity needed to understand both what the authors have done and how their models work. In Section 2, where the modeling system is described, references are missing to external model products (e.g., ECMWF results) and the text is unclear whether standard model products are used or whether custom simulations were conducted for this work. Where custom simulations are done, there needs to be better referencing to the base model and citations to any work validating the custom model. These problems should be easily resolved by re-writing this section to become much more clear. Indeed, poor writing quality is a problem throughout this manuscript, and most of the detailed comments below stem from vague or difficult to understand sentences throughout the text.

We use ECMWF standard forecast products for atmosphere and wave. This is explained in the "Metocean forcing" section we where refer to ECMWF publications and web site where we have obtained the products. Temporal and spatial resolution is also given in this section. We have to tried to state this even clearer and used the term "forecast products" instead of "forecasts".

The greatest technical deficiency stems from the description of OpenOil and its usage. We have a long paragraph describing OpenOil with several references. We have now provided more details on how the oil droplet size distribution is prescribed in several new subsections.

The detailed comments below give several, specific issues related to the handling of this model in the text. In summary, the following elements need to be improved before the model results can be critically evaluated:

The authors need to better distinguish surface and sub-surface sources of oil

and describe the chemical properties of the oil as initialized in the model. I assume the authors use the dead-oil representation of LSC from ADIOS for both surface and subsurface sources. This is ok, but they need to clearly state this and explain what the flow rate at the subsurface source means (it neglects all gas). Yes we use the dead-oil representation of LSC. This is now clearly stated. Here, we track only surface oil particles. This is also now clearly stated.

The authors need to provide the equations for DS88 and Li17 since they are single-line, empirical formulas. This will greatly improve understanding for the reader. We agree. This is included now. Several new subsubsections are now included describing the droplet size distribution calculation.

The authors need to explain how OpenOil uses DS88 and Li17 to predict a time-evolving droplet size distribution. Both of these equations are empirical formulas for the steady-state size distribution of submerged oil under various surface wave conditions. It is not obvious how these models produce time-variable droplet size distributions. The time-dependency is a result of longer time integration in the full oil spill model. The droplet size distribution from DS88 and Li17 are here assumed to apply after stochastic wave breaking events in the model, thus corresponding to equilibrium spectra from lab tank experiments. We argue that during the further evolution in the full model, turbulence and buoyant resurfacing of particles continues to affect the total droplet size distribution because larger droplets resurface faster that small particles. Resurfaced particles are subject to subsequent wave breaking and resurfacing. The total droplet size spectrum therefore changes on the course of hours to days, which is not achieved in laboratory experiments. In the full model, oil properties also change due to further emulsification and and varying weather conditions. In the present figure 5, we start out with a random distribution and use a constant wind speed of 8 m/s over some time in order allow emulsification to evolve. The figure shows that the two formulations use some time to reach steady-state.

**Only Li17 predicts oil droplet size for a jet (e.g., the subsurface source), and

technically, this is a different equation (different fit parameters) to the surface size distribution in that paper. What equation was used for subsurface oil? How was this done in the DS88 simulations?** How droplet sizes for the surface release are described in OpenOil, is documented in a very recent paper in OS (Röhrs et al. 2018). For the bottom release, we have unfortunately overseen to describe how particle distributions are initialized, and would like to thank the reviewer for spotting this shortcoming. We have now added the following paragraph to Section 3 to clarify how droplet sizes are given: “The oil elements released at the surface are assigned random droplet radii at each entrainment incident, according to the parameterisation of size distributions from respectively DS88 or Li17, see Röhrs et al. (2018) for details. For oil elements released at the seafloor (wallhead), we use a simplistic and pragmatic approach of prescribing random radii in the range 0.5mm to 5mm, as suggested by Johansen et al. (2000). Oil elements at the sea surface (slick) are not considered to have a radius.”

The fate processes considered in the model are applicable to surface oil. They can also be used for subsurface oil when dead oil is introduced into the model (which I believe the authors have done). However, dissolution, which is neglected in the present study, was a significant fate process during DWH and occurred very quickly. The authors are only justified to ignore it because they track only the low-solubility components of the oil. We agree with the reviewer. We use the flow rate of Crone and Tolstoy (2010) ignoring gas and highly volatile compounds. This is now clearly stated in the text.

After these important issues are resolved, the results can be critically evaluated. Throughout the results, the authors should carefully connect droplet sizes to mech- anisms. For example, in point 25.) below the authors state that small droplets were subject to greater wind forcing. In what way? Perhaps this is true, but the mechanism is unstated. My reading of this does not make sense because I would assume smaller particles move away from the sea surface, where they

are less impacted by winds. But, another reading could be that smaller particles are generated by stronger winds. Yet, the manuscript does not really explain it either way. We have now improved our discussion of the figures involving droplet diameter and wind speed (present Figs 6 and 7). What we meant was that these figures show that the droplets exposed to the highest wind speed have the smallest diameter, in accordance with theory. We do not mean that smaller droplets are subject to more wind drift.

Overall, my conclusion is that this is an important study conducted by researchers who are clearly capable of handling the ocean circulation under different freshwater inflows. They should spend more time polishing the text and much more carefully explain the droplet size modeling, handle the complex thermodynamics of the oil and gas, and more carefully assess and describe the important fate mechanisms. The remainder of this review addresses specific elements of the manuscript text. 1. In the abstract, the authors describe the droplet size analysis and the key result (that the results are robust to the choice of algorithm). However, for the effects of river inflow, the abstract lacks the key result. It would be best to also summarize the main outcome of that work within the abstract. We agree. This is included now.

2. The introduction is a collection of quite detailed sentences that do not flow together well. While each sentence is well referenced, I wonder whether the normal reader can follow these statements. For instance, for the Taylor Energy study, the paper reads, "The drifters deployed during the experiment...efficiently described three major transport pathways;" yet, the text does not introduce or describe the drifters before this key result statement. It would be better to say earlier in the paragraph that the Taylor Energy study included deployment of surface drifters and explain whether they were all released at the source? all at the same time? on what dates or in which season? what layer of water is tracked? As is, the reader really needs to know more about this study than is stated to understand

the text. This is a specific comment that can be generalized to most of the statements in the introduction. This part of the paper could be significantly improved by more careful and tight writing. The introduction has been rewritten with several more references, trying to be more concise and to the point.

3. Page 2, Lines 21-25. This section is a little misleading. In the abstract and later in the paper, the reader will realize that oil droplets are initialized at the surface and at the seabed. The model does include the key fate factors for surface oil, but does not include the key fate factor for the subsurface oil (dissolution). This section should be revised to remove this ambiguity. As is, it sounds like the model includes the key fate factors for all oil droplets released in the model; this is only true for the surface released oil or the list of factors stated in this sentence is incomplete (it should also include dissolution). We agree. The have now rewritten this paragraph and explained that dissolution is not included.

4. Page 2, Line 33. Include a citation with a link to the ERMA website and the date the data were accessed / downloaded. Not sure what is meant. This was already in the reference list, but the link is now given in the text.

5. Page 3, Line 2. "We used the oil thickness classifications derived from the satel- lite analysis for our modeling study." How were these used? The next sentence says that particles were seeded without respect to thickness. At least give the reader some hint here how thickness was used and where in the manuscript this concept will be expanded. Our apologies. This statement was included by mistake. We do not consider oil film thickness in this study.

6. Page 3, Line 6. It seems that this sentence needs a citation to the source where the HYCOM model data, or at least numerical grid and initial conditions, were downloaded. The statements starting on Line 9 hint that the model simulations used in this paper may have been generated by the authors for this paper. Is that the case? If so, this should be explicitly stated. GoM-HYCOM is also an

operational model, so a reader may assume that standard HYCOM products were used. The last sentence seems to make this more clear (maybe the authors use their own output). I recommend revising this paragraph to make it even more clear that the authors performed custom simulations using this modeling system, state what elements are from standard GoM-HYCOM products, clearly explain the initial conditions, and explain whether data assimilation is turned on for any of the present simulations.

We agree that that paragraph was not totally clear in the initial version. We did use model outputs from our own configuration of HYCOM over the Gulf of Mexico, which is data-assimilative. This is now stated more clearly in the revised version: "In the cases presented here, the ocean circulation fields come from a data-assimilative, high-resolution (1/50o, 1.8 km) configuration of the Hybrid Coordinate Ocean Model (HY-COM) in the Gulf of Mexico (GoM), developed by the authors (GoM-HYCOM 1/50)." We provide additional information about the initial conditions: "The model is initialized in October 2009 with fields from the operational Global HYCOM (GLB-HYCOM) simulation run at the Naval Research Laboratory at the Stennis Space Center (GLB-HYCOM expt_90.8, HYC) [. . .]." Finally, we now clearly indicate where data from both simulations using our Gulf of Mexico model configuration can be accessed on the GRIIDC data server: "The outputs from both simulations are available at the Gulf of Mexico Research Initiative Information and Data Cooperative (GRIIDC), doi: 10.7266/N7NG4NPC."

7. Much of the text could be much better polished. A good example is the sentence starting on Page 3, Line 9, "The HYCOM model has..." In this sentence, the different layers are described. Use parallel grammatical structure: "Isopycnal layers in stratified water", "sigma terrain-following layers in sharp topography", then edit the next phrase to read "isobaric layers in the mixed layer and very shallow areas." Finally, the format of the citation needs to be within parentheses, e.g., (Bleck, 2002). While this does not impact the scientific merit of the paper,

these types of careless mistakes distract from the impact of the paper and make illustration of sloppy writing throughout the manuscript. We have corrected the sentence following the reviewer's suggestion. It now reads: "The HYCOM model has a flexible, hybrid vertical coordinate system, in which the distribution of vertical layers is optimized: they are isopycnal in stratified water columns, sigma terrain-following in regions with sharp topography, and isobaric in the mixed layer and very shallow areas (Bleck, 2002). This unique utility in combination with the special treatment of freshwater inputs (Schiller and Kourafalou, 2010), also make HYCOM advantageous in areas with complicated topography, such as the GoM, and strong freshwater outflows such as the MR discharge, allowing the development of detailed process studies around the outflow regions, where plume dynamics are dominant (Androulidakis et al., 2015)." We have corrected the typo in the reference to Bleck.

8. Inconsistent usage of ECM and ECMWF (Page 3, Lines 18 and 25). Also, provide a citation for where and when the ECMWF data were downloaded. ECMWF is now used throughout, and the link is also provided in the text.

9. Page 3, Line 28. Oil simulations were performed with a 3-hourly time step; yet, the text says that ECMWF forcing data have a 12 hourly time step. How was this data sub-sampled? Sample and hold? Linear interpolation? Other? We used linear interpolation between time steps for waves. Atmospheric data are provided with 3 hour time step. This is now stated in the text.

10. Page 4, Line 1. Provide a citation to the data source for the WAM data that were downloaded from ECMWF. Reference to ECMWF was already given in the reference list and is now given in the text.

11. Section 2.2. As one reads this entire section it is very difficult to separate model- ing work done by the present authors from modeling work conducted elsewhere and used by the present authors. Perhaps it would be helpful for each model product to start with a statement that either reads something like,

“Wave proper- ties were downloaded from WAM model results available from ECMWF (citation),” or “We simulated wave forcing using the WAM model.” Also, the acronym WAM is undefined. We have rewritten according to the reviewers suggestion. The acronym WAM is now clearly stated in the metocean section and we cite ECMWF wind and wave data separately.

12. Page 4, Line 18. “Droplet size spectra” should probably be “Droplet size distribu- tion”. Reserve the term “spectrum” to refer to energy distributions. Page 4, Line 18ff. The Delvigne and Sweeney (1988) model must predict two parameters of the droplet size distribution: a characteristic droplet size and a variance parameter. The text appears to discuss the variance parameter, as given by a power-law relationship. I think this section would be much easier to understand if the equation is provided, e.g., P DF = ad b . Then, the authors can also discuss the proportionality constant (i.e., a) and the state variable of the power law would be defined (presently, it is undefined, and I assume it is droplet size, d). We have now written “droplet size distribution” throughout. Much more details are provided on the oil droplet distribution including several equations.

I am uncertain under what conditions Li17 would give d 50 =100 μm. That is a very small size that would result from high mixing energy and low interfacial tension. Rather than state “depending on oil and environmental conditions”; instead state, “for an oil with X interfacial tension and Y viscosity, in a sea state of Z, ...”. As is, this formulation is very misleading since it is difficult to get oil droplets this small without intervention (e.g., chemical dispersant application). Please see comment above. Our results are for example in accordance with North et al (2011).

15. Page 4, Line 34. “physical mechanism...is not obvious.” On the contrary, the physical mechanism for wind drift is well known and understood. Oil typically occupies a region very close to the water surface or on the surface (e.g., slicks). Numerical models of ocean circulation have large vertical layer thickness at the

surface (about 50 cm or more) relative to the oil (about 1 mm to 10 cm). If the numerical model completely resolved the surface boundary layer, there would be no added wind drift. Because the circulation model integrates the top 50 cm to 1 m, there needs to be an empirical wind drift added. 3value only because circulation models have very similar vertical layer thicknesses in their numerical discretization. If a model had a unique surface discretization, a different empirical factor would be needed. We agree. What we meant is that the size of the wind drift factor can be discussed. More details from Jones et al (2016) and discussion are provided now.

16. Top of Page 5. Unusual usage of a colon at the end of the first paragraph. Re- place with “”...the sum of oil entrainment by breaking waves and vertical turbulent diffusion.” Also, “inter facial” should be “interfacial”. Finally, the present gram- matical construction makes it seem as if the ADIOS mechanisms are part of the processes describing vertical transport, which they are not. Instead, ADIOS is a 0-D model that predicts oil fate. We agree. This paragraph has been rewritten.

17. Page 5, Line 18. Dissolution is by no means a “long-term weathering process.” Dissolution is largely complete in the time span of minutes to hours and much less than one day. Dissolution is ignored in surface fate models because it is typically about 10 times less important than volatilization. For oil not in contact with the air/water interface (e.g., oil discharged at the seafloor), dissolution is the dominant fate process, and is the main fate mechanism for 100discharged from the DWH and for about 25spilled. This dissolution occurred as oil droplets and gas bubbles transported from the riser to the sea surface, a process that took about 4 to 12 hours duration (see papers by Ryerson in GRL and PNAS). Dissolution cannot be listed in this sentence as a long-term fate process, nor can it be completely ignored for DWH. The present authors may ignore dissolution if 1.) they assume it is fast and 2.) they track the insoluble components of

the oil. We agree with the reviewer. As seen in the mass balance plot (now Fig. 4), most of the oil we track is at the surface. Hence the volatile compounds evaporate fast (as seen in Fig. 4), and we track the heavy compounds.

18. Page 5, Line 23. The flow rate provided is the flow rate of crude oil (dead oil), neglecting the release of light compounds (e.g., gas). The sentence reads as if the 0.1 m 3 /s is the in situ flow rate of oil and liquid petroleum at the well head. The meaning of the flow rate needs to be very carefully defined. Since the model neglects dissolution, I believe this is the correct flow rate to use at the seafloor. I am just asking the authors to carefully define itâĂŤit is the flow rate of dead (black) oil, neglecting gases and highly-volatile compounds. We follow Crone and Tolstoy (2010). They estimate a liquid oil fraction of 0.4 and get a flow rate of 0.1 m-3 sec. We now state clearly that gas and highly volatile compounds are not included here.

19. Page 5, Line 24. "Although..." I think you mean "Because..." We agree. Corrected.

20. Page 5, Line 28. Rather than submit a new estimate for the removal rate of oil from the surface, can the authors not cite another study? Does the Oil Budget Calculator (OBC) contain this estimate? Many subsequent papers have confirmed the estimates in the OBC. We think it is most consistent to use a removal rate in line with our model study. It will vary from day to day with wind and wave conditions. This removal rate is only required for estimating the amount of oil present at the surface when the model is initiated (from shape files). Most other studies have used constant removal rates as far as we can see.

21. Page 5, Line 31. Define "super-particles". I know what they are, but they are not defined as far as I can tell in the paper. We now just write particles through out the manus.

22. Page 5, Line 32. What is the droplet size used for the particles seeded at the

seafloor? Only the Li17 equation can predict droplet sizes from a jet discharge, but technically, the Li17 equation is two equations: one for a jet and one for surface oil. Did the authors use both? Later, results are compared between Li17 and DS88. For the particles released at the seafloor, this comparison should be meaningless as DS88 does not provide estimates for a jet. As such, this comparison becomes difficult to assess. This is now explained. For oil elements released at the seafloor (wallhead), a simplistic and pragmatic approach of prescribing random radii in the range 0.5mm to 5mm was used, as suggested by Johansen (2000).

23. Page 6, Line 5. One would not expect much difference among the models since they are both calibrated to the same experimental data. Perhaps the authors can explain a hypothesis why these models might be expected to give different re- sults? For instance, though both may predict the same mean droplet sizes, they use different distributions. If the fate of oil in the tails of the distributions is differ- ent, then maybe one would expect differences. But, since Li17 is calibrated to the same data as DS88, I would not expect a difference a priori. And, if differences are only seen in the tails of the distributions, one should consider whether this involves much mass. A long section explaining the differences between the formulations is now provided.

24. Page 6, Line 8-12. Both the DS88 and Li17 models are steady-state. I cannot understand how they could give different results after 1 hour to their results after 24 hours. The only way their results can change is if the ocean forcing changes, which may be happening, or if they are used iteratively with changing surface oil conditions. However, the text completely obscures this fact and implies that the models are unsteady, producing time-variable results. Li17 and DS88 are not and do not. This must have to do with the way OpenOil uses these in a real, time-evolving simulation. In that case, more details of OpenOil are needed, and because this is a critical conclusion of the paper, a citation to OpenOil would not be adequate: the key mechanisms / methods used to generate time-variable

droplet size distributions from steady-state equations must be explained. The time-dependency is a result of longer time integration in the full oil spill model. The droplet size distribution from DS88 and Li17 are here assumed to apply after stochastic wave breaking events in the model, thus corresponding to equilibrium spectra from lab tank experiments. We argue that during the further evolution in the full model, turbulence and buoyant resurfacing of particles continues to affect the total droplet size distribution because larger droplets resurface faster that small particles. Resurfaced particles are subject to subsequent wave breaking and resurfacing. The total droplet size spectrum therefore changes on the course of hours to days, which is not achieved in laboratory experiments. In the full model, oil properties also change due to further emulsification and and varying weather conditions. In the present figure 5, we start out with a random distribution and use a constant wind speed of 8 m/s over some time in order allow emulsification to evolve. The figure shows that the two formulations use some time to reach steady-state.

25. Page 6, Line 15. Small particles will be easily entrained into the ocean interior and away from the surface. The text here says the small particles have probably Discussion paper"been subjected to more wind action in the last 12 hours." This sentence needs explanation. What affect of the wind? Do the authors mean the particles are smaller because the wind generated larger waves, yielding more energy, resulting in smaller particles? It does not sound like it. The paragraph reads to me that the authors are claiming the smaller particles experience greater wind drift. I would expect larger particles to experience greater wind drift as they will be closer to the surface. Please edit for clarity. This has now been rewritten: ...MAFLA shelf, probably because the oil particles have been subject to more wind and hence wave action and natural dispersion in the last 24 hours

26. Page 6, Line 25ff. These results are meaningful in so far as the oil droplet sizes are understood. The above comments need significant clarification for the oil droplet sizes before this section can be rigorously evaluated. We agree. Much

more detail is now provided

27. Page 8, Line 5. The conclusion is that the droplet size distribution has a signifi- cant effect on horizontal and vertical distributions and a wind speed range of 5-7 m/s is noted. This appears contrary to the results, which show two different size distribution models yield very similar results. Perhaps the authors mean that the fate of the oil is linked to the size distribution and they are not claiming the two models give different results. While I also agree size distribution is critical, this paper does not show that result; instead, it shows that two reasonable models give similar results. To conclude that results would be much different with a differ- ent size distribution, additional simulations with a different size distribution would be needed. We agree that this statement is misleading and have rewritten accordingly stating that "Our results indicate that the two different formulations for oil droplet size distribution give similar results for both vertical and horizontal distribution of the oil".

28. I also note that this paper ignores all of the prior work using far-field Lagrangian Particle Tracking models by Elizabeth North's and Claire Paris's research groups. While North generally uses SABGOM circulation forcing, Paris also uses GoM-HYCOM. This work should be summarized and critically reviewed in the Introduction. Key results of this work that confirm or deny similar conclusions in North's or Paris's papers should also be highlighted throughout the Results and Discussion. We thank the reviewer for pointing this out. The papers by North, Paris, Barker and others are now properly cited in the introduction, results and discussion