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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.

Anonymous Referee #3

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1 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.

The greatest technical deficiency stems from the description of OpenOil and its usage. 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).
- 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.
- 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.

- 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?

- 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.

After these important issues are resolved, the results can be critically evaluated. Throughout the results, the authors should carefully connect droplet sizes to mechanisms. 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.

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.

2 Detailed Comments

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.

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.

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).

4. Page 2, Line 33. Include a citation with a link to the ERMA website and the date the data were accessed / downloaded.

5. Page 3, Line 2. “We used the oil thickness classifications derived from the satellite 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.

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.

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

it more difficult for the reader to understand. As in point 2.) above, this is just one illustration of sloppy writing throughout the manuscript.

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.

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?

10. Page 4, Line 1. Provide a citation to the data source for the WAM data that were downloaded from ECMWF.

11. Section 2.2. As one reads this entire section it is very difficult to separate modeling 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 properties 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.

12. Page 4, Line 18. "Droplet size spectra" should probably be "Droplet size distribution". Reserve the term "spectrum" to refer to energy distributions.

13. 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., $PDF = ad^b$. 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).

14. Page 4, Line 26. I am uncertain under what conditions Li17 would give $d_{50} = 100\ \mu$m. 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).

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. 3% of the wind is a typical value 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.

16. Top of Page 5. Unusual usage of a colon at the end of the first paragraph. Replace with ""...the sum of oil entrainment by breaking waves and vertical turbulent diffusion." Also, "inter facial" should be "interfacial". Finally, the present grammatical 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.

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 100% of the methane discharged from the DWH and for about 25% of the total mass of petroleum spilled. 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.

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 it—it is the flow rate of dead (black) oil, neglecting gases and highly-volatile compounds.

19. Page 5, Line 24. “Although...” I think you mean “Because...”

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.

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.

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.

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 results? 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 different, 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.

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.

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

"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.

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.

27. Page 8, Line 5. The conclusion is that the droplet size distribution has a significant 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 different size distribution, additional simulations with a different size distribution would be needed.

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.

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