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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.

Lars R. Hole et al.

lrh@met.no

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[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.

The article is too wordy. For example, the Introduction seems to focus on discussing a couple papers related to the MR rather than providing a background for investigating the DWH spill in relation to existing works that modeled the DWH

surface spill, such as the works of McFaddyen et al. (EOS, 2011) and Boufadel et al. (ES&T, 2014). In fact, only by comparing to such works that the authors can highlight their contributions. How do the results based on the more accurate HYCOM compare to results obtained using NCOM, NGOM, etc? The oil behavior appears to be an aftermath of the exercise, which is to highlight the role of the MR. One is led to wonder, what would be the fate of the oil if one assumes that the droplets are neutrally buoyant? What was the depth of mixing? In essence, there were too many things that the authors considered giving the impression that the article was put together hastily without a goal in mind. Detailed comments are below. We thank the reviewer for pointing this out. The introduction is now rewritten and with additional references to relevant studies.

"Next, the study showcases how NGoM oil pathways are influenced by river plume circulation and river induced fronts. We also investigate whether" It is preferable to stick to one tone of writing, either passive (the study discusses) or active (we investigate). Mixing the two is not common. We agree. We have now used passive tense throughout.

"As part of the efforts made by the National Oceanic and Atmospheric Administration (NOAA) to assess the extent and impact of the DWH spill, participants on this team analyzed hundreds of satellite images (microwave and optical) and produced oil extent delineations throughout the lifetime of the spill. Classifications derived from the satellite analysis of the DWH SOP can be accessed through the NOAA-ERMA website (ERM)." Which team? The NOAA team or the authors? Also, why not writing that "we analyzed satellite images (microwave and optical) of the spill from the NOAA site ERMA" ? Why using such a wordy approach? We agree. We have made this description much shorter and to the point.

"The shapefiles were used for both initialization of the oil drift simulations and for verification of results". By whom? By NOAA or by the current authors? By the currents authors. We have reformulated to make it clear.

“For the present study, we performed two simulations: one with the attributes men- tioned above, called Reference simulation, and one called No river, in which the salin- ity fronts have been removed by shutting off the river discharge, setting precipitation to zero, and turning off the assimilation of salinity profiles.” Please mention here in a sentence why the two choices were made. Assuming that one wants to evaluate the impact of the MR, how realistic would be to turn off the flow of the Mississippi? Wouldn’t using the minimum flow be more realistic? Is the goal to conduct an academic exercise to evaluate the impact of the MR on the near shore hy- drodynamics? If yes, then, this should be conducted only in a Monte Carlo framework as done by NOAA’s Barker (EOS, 2011). Otherwise, the results and conclusions would be dependent on the hydrodynamic and climatological conditions during the DWH spill.

We followed a standard methodology that is traditionally followed by model simulations (often called "twin experiments") but also by theoretical approaches (as in examining prevailing balance of forces in momentum equations by making assumptions that remove one or more equation terms). To fully evaluate the impact of an individual forcing mechanism (here the Mississippi buoyant discharge), this forcing has to be completely shut off. Same for the impact of additional forcings influencing salinity variability (precipitation in particular). All other forcings for the DwH period remain the same. The intent is exactly to keep all other conditions similar, especially the wind-driven flows and the offshore circulation (Loop Current and eddies). Keeping any salinity gradients (like using a small discharge rate etc.) would prevent these experiments from giving conclusive results.

“The physical mechanism behind this wind drift factor is not obvious, and is discussed in Jones et al. (2016).” What was the summary from Jones et al. (2016)? The increase in oil drift is well understood for anyone who worked on transport. Based on simple momentum considerations, one could learn that anything that is on the top of the water surface (and has a smaller density) would

move faster than the water beneath it if the wind is blowing (i.e., the whole idea of a sail boat !). Also, the fact that the oil viscosity is much higher than that of the water beneath it, makes the oil behaves locally as an "object". But if the authors have no idea why the oil moves faster than the water beneath it, I am not sure they should be conducting oil spill simulations using complicated models for evaporation, emulsification, dispersion, etc. Also, these processes were mentioned early on, and then were not discussed afterwards. As discussed in Jones et al. (2016), the wind drift factor need not be solely the movement of oil relative to water, but may be partly a compensation factor for the inability of any ocean model to represent the strong shear current in the upper few centimeters/decimeters of the ocean, or biases due to the truncated spectral tail of the wavemodel-derived Stokes drift, as well as unresolved Langmuir jet currents. We have updated the text to make this clearer. Emulsification and dispersion is related to oil droplet size and is hence discussed in the paper. Evaporation is shown in the mass balance plot (Fig. 4) and also mentioned in the discussion.

"According to our mass balance calculations for the DWH spill, using the Light Louisiana Sweet oil type from the NOAA oil library and environmental conditions as described above, it seems reasonable to assume that 80% of the oil mass is removed from the surface after 10 days. This is within the range in our simulations that is typically 60 to 95% (see examples of mass balance plots further down). The simulations for May 2010 are initialized by seeding 48730 super-particles in a polygon obtained from NOAA shapefiles. Each particle represents initially 1 m3 oil. A continuous point source at the sea floor seeds an additional 8460 particles (8460 m-3) per day dur- ing the simulation. After June 3rd these numbers are increased by 20% to 10368 m3 day-1." The work by Boufadel et al. (2014, EST) specifically addressed this issue. See, for example, their Figure 2. They found that the oil disappears of the surface at around 20% per day.

We thank the reviewer for pointing out this publication. However, Boufadel et al used a constant removal rate, while we use a removal rate depending on wind and waves and the specific oil composition, as given by the Adios oil library. As the reviewer probably know, the residence time of the oil at the surface is highly variable and dependent of oil type and wind speed. Very few field observations are available (a new study is in prep by some of the current authors). We believe that 20% remaining oil after 10 days is representative for the conditions during our simulations. This estimate is only used to decide the amount of oil to be seeded in the polygon at the first time step.

" The OpenOil simulation shown on top in Fig. 3 is carried out using the classical DS88 oil droplet size distribution (Delvigne and Sweeney, 1988)." What was the thickness of the "Surface Layer" in the simulation? 1.0 m? 10.0 m? Obviously, this affects the meaning of concentration. The top layer in the ocean model is 1m. This is now pointed out the the manuscript.

"Fig. 3 lower panel shows the results from repeating this simulation using the new Li17 formulation. In Fig. 7 we show the mass balance during seven days for the Li17 simulation. There is virtually no difference between DS88 and Li17 and only the former is shown here."Why discussing Figure SEVEN prior to Figures 4, 5, and 6? Should Figure 7 be made as Figure 4? We are sorry, the figure numbering is carried out automatically by the Latex software. The order of appearance has been corrected now.

Table 2 provides oil deposited on the shorelines, and the amount needs to be compared with the work of Boufadel et al. (2014, ES&T). Our results are not directly comparable to Boufadel et al. (2014) since they looked at different periods than us and the used constant daily removal rates from the surface (0,10%and 20%). We are using a varying removal rate depending on wind speed and wave breaking as described in our manus and by Röhrs et al (2018).

How are the authors deciding what is on the surface and what is submerged? What was the cutoff depth? I find it unusual that they did not provide such in-

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formation considering that GOMRI works including their own showed the importance of depth (on the order of centimeters) on the hydrodynamics. Every particle has an individual, varying depth for each time step as resulting from buoyant uplift and turbulent mixing. We have no cutoff depth since we have a point source at the sea floor.

"A realistic description of droplet formation is required to describe the effects of an oil spill on the environment" True, but there is no citation for this statement. We now refer to Boufadel et al. (2014) and North et al. (2011).

"The parts of the oil spill at the surface is more hazardous to birds and the beach communities, while the small, submerged parts will have a substantially larger surface to 30 interact with water and plankton (Carroll et al., 2018)." The authors cite a paper on the impact to the Northeast fisheries in spite of the HUGE amount of papers dealing with the DWH spill. It is pretty strange. Maybe they can start with the review by Short (2017, Archives of Environmental Contamination and Toxicology). Maybe they can peruse the GOMRI website? We now refer to Short (2017)

"To the best of our knowledge, this is the first time the importance of the effect of river fronts on oil slick transport in the gulf has been demonstrated using high resolution models." The work of the authors themselves addressed the impact of the MR on coastal hydrodynamics. Kourafalou, V. H. and Androulidakis, Y. S.: Influence of Mississippi River induced circulation on the Deepwater Horizon oil spill transport, Journal of Geophysical Research: Oceans, 118, 3823–3842, 2013. Le Henaff, M. and Kourafalou, V. H.: Mississippi waters reaching South Florida reefs under no flood conditions: synthesis of observing and modeling system findings, Ocean Dynamics, 66, 435–459, 2016. Kourafalou and Androulidakis (2013) used a purely hydrodynamic model, they did not use an oil drift model. Their results were based on model salinity (for the river plume signal) and data derived oil patches (for the surface signature of the oil). However, we have now reformulated to "To the best of our knowledge, this is the first time the importance of the effect of river fronts on oil slick transport in the gulf has been demonstrated using high resolution forcing and a fully fledged oil drift model."