Geoscientific Model Development Discussions

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Reply on RC1

Elias J. Hunter et al.

Author comment on "ROMSPath v1.0: offline particle tracking for the Regional Ocean Modeling System (ROMS)" by Elias J. Hunter et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-400-AC1, 2022

Dr. Clavel-Henry,

Thank you for your comments on our manuscript titled "ROMSPath v1.0: Offline Particle Tracking for the Regional Ocean Modeling System (ROMS)". While the overall impression from these comments is positive, you note some areas for improvement. Specifically there is room for quantitative results related the test cases presented and discussion from relevant literature to give context to the results. We endeavored to respond to these concerns and address these issue point by point below. Note that the order of these comments have been rearranged somewhat. Your comments are in bold.

"L.93-94: In that statement, I am curious about one thing: what about ROMS models that have a small spatial extent and, somehow, have a less pronounced curvilinearity; thus, potentially small errors in the coordinate interpolation? Would the performance of ROMSPath be still better than LTRANS? That is something I would have liked to see discussed as it has significant consequences for the choice of the software."

Yes, a configuration for LTRANS is possible where ROMSPath and LTRANS show similar results. This, however, requires careful consideration of domain size, location, study objectives, and choice of geographic reference. We will note this in the revised manuscript. However, a systematic examination of the conditions when LTRANS output matches ROMSPath output is outside the scope of this study.

"L.338-339: Is it relevant to write about a result when neither the methods nor supporting graphs are shown? It confused me because I am not sure what you refer to by this statement. I suggest removing these two sentences or to provide an annex with methods, results, and discussion. "

It is relevant in that it addresses the questions posed about L.93-94 above regarding the sensitivity of LTRANS results to basic configuration concerns. An LTRANS simulation with a reference coordinate horizontally distant from the grid showed results extremely divergent from the run with a reference coordinate selected using the recommended criterion. We thought this an unfair comparison as the LTRANS manual warns against it. We will remove the reference to the simulation and replace it with a comment on the careful consideration

of LTRANS model parameters.

"Figure 1: this figure should be put in a supplementary file. It is not a graph showing novelty and can be easily found on the website of ROMS.

We will add it as a supplementary figure. Even though it available elsewhere, it is also represents a major change to the LTRANS code. "

"L.114: Your hydrodynamic refinement ratio is 7:1. It is stated that a ratio higher than 5:1 can degrade the model performance (e.g., doi: 10.1016/j.pocean.2004.07.017 and within references). Was the hydrodynamic model verified on that point?"

While the Barth et al (2005) recommends a refinement of 3:1 or 5:1, the Spall and Holland (1991) reference suggests 7:1 is acceptable. And a 7:1 ratio was successful in previous work, such as Warner et al (2017). We will note this in the manuscript. While a skill assessment of the ROMS output is outside the scope of this study, aspects of the estuarine circulation (tides, exchange flow, temperature, etc) were evaluated and found satisfactory. This will be the subject of upcoming work.

Barth, A., et al. (2005). "Two-way nested model of mesoscale circulation features in the Ligurian Sea." Progress in Oceanography 66(2-4): 171-189.
Spall, M. A. and W. R. Holland (1991). "A Nested Primitive Equation Model for Oceanic Applications." Journal of Physical Oceanography 21(2): 205-220.

Warner, J. C., et al. (2017). "Inner-shelf ocean dynamics and seafloor morphologic changes during Hurricane Sandy." Continental Shelf Research 138: 1-18.

"Table 1: 1) I need a rationale on why "2", "30", and "90" days transport duration and the particle number of "3285", "6000", and "32000" have been selected. "

There are three sets off conditions listed in table 1. A) A 2-day run with 3285 particles. B) A 30-day run with 6000 Particles. C) A 90 day run with 32000 particles.

Case A is used to illustrate the vertical clustering issue with LTRANS relative to ROMSPath. 3285 is the number of particles evenly distributed throughout the water depth of the initialization point. While the simulation was run longer, 2 days is sufficient to illustrate the clustering issues. While the clustering is easily identifiable after 6 hours, the added time simply highlights the point.

Case B is used for a number of cases, in sections 3.3.3 and 3.3.4. Most related to nesting. 30 days was chosen as it was sufficient time to particles to enter and leave the snaildel domain. 6000 particles was used as a starting point for these runs, to minimize computational time while maintaining a coherent patch for analysis. With more particles added if needed. 6000 particles proved sufficient for coherent particle patches after 30 days.

The run time for Case C was increased to 90 days to allow for the possibility of particles traversing leaving the shelf. The number of particles in Case C was increased to 32000 due to the 90 day run time, as a decrease in coherence as the footprint of the particles is expected as time increase. Previous work suggested particle numbers in the 20k-30k range would suffice. 32000 was chosen for computational reasons unique to our computing cluster.

We will add this information to the manuscript for clarity.

"2) For the vertical experience (i.e., Vert. LTRANS and Vert. ROMSPath), I got confused. Please, indicate the depth range and also indicate that the release is made of evenly distributed points along a segment instead of "Line" (For example: Evenly distributed points between X and Y depths). As for "Point", please, indicate the coordinates instead of "point."

We will clarify these in the table and in the text. And see below.

Section 3.3: I think that you should add in each section if you used both the parent and child hydrodynamic models (i.e., DOPPIO and SnailDel) to track particles or just one of the hydrodynamic models. See below)

We attempted to make this clear in the table, under the nested column. Nested=yes means both Doppio and Snaildel output is used. Nested = no means Doppio only.

- **In line 286, you said you used the DOPPIO model for online tracking of particles (i.e., ROMSFloat). Did you also only use DOPPIO fields for particle tracking with LTRANS and ROMSPath?**

Yes.

2. In sections 3.3.2 and 3.3.4, did you use DOPPIO and SnailDel, or just DOPPIO?

Both.

Overall the clarity regarding the different model configurations is lacking. We will add more detail to the table, and to the text in the revised manuscript.

Sections 4.1 and 4.2: the main result (or global outcome) from the tests should be put on the first line of the paragraphs. In these two sections, I had an introduction of the figures instead of the main findings.

We will restructure these in the revised manyscript.

Section 4.1: the results from ROMSPath being closed to the online simulation ROMSfloat should be a valorised outcome of the manuscript. I expected a few comparisons with peer-reviewed studies that could have compared online and offline particle tracking simulations. Consider also my first comment (for L.93-94).

I am not entirely clear about what is meant by the term "valorised". But we can comment on comparison with other peer reviewed studies.

While there is extensive literature on Lagrangian analysis of ocean, there are few studies directly comparing online vs offline particle tracking for the same model run. e.g. Wagner et al. (2019) and Cassiani et al (2016). However, these compare offline particles dispersion or online tracer dispersion. Typically the choice of offline vs. online is due to the practical considerations of computing time and storage space.

Cassiani, M., et al. (2016). "The offline Lagrangian particle model FLEXPART–NorESM/CAM (v1): model description and comparisons with the online NorESM transport scheme and with the reference FLEXPART model." Geoscientific Model Development 9(11): 4029-4048.

Wagner, P., et al. (2019). "Can Lagrangian Tracking Simulate Tracer Spreading in a High-Resolution Ocean General Circulation Model?" Journal of Physical Oceanography 49(5): 1141-1157.

L.352-354: Please, note that this is a nonshown result that took half the paragraph of section 4.2. I think this result is interesting to have at least a supplementary figure and a short explanation of the method in 3.3.2.

We will add a figure similar to Figure 5 as a supplement. The method is no different than the other figures, simply a configuration change.

"Section 4.3.: 1) Considering the results relied only on visuals, I would have appreciated, in complementary, to have quantitative information such as a spatial aggregation index or the surface that contained 95% of particles at day X and per scenario. It would quantify the idea of "more horizontal dispersion"(L.366) and at least put some contrast between figures 6e and 6g."

We will add a dispersion coefficient to the panels in Figure 6. Calculated based on Lacase (2008). See new figure 6. The Dispersion coefficient increased consistent in the text. See attached

LaCasce, J. H. (2008). "Statistics from Lagrangian observations." Progress in Oceanography 77(1): 1-29.

"2) Regarding the particles advected in the estuary with 'Nest/No Turb' but not with 'No Nest/No Turb', a small discussion would be welcomed. I don't know the surface of the Delaware Bay but I easily guess that the resolution of the Doppio is too coarse for capturing the water circulation as the SnailDel can do. Hence the importance to do particle tracking simulation using the parent and child grid of hydrodynamic models in intertidal zones.

Yes, the Doppio resolution is too coarse (7km). We need at least 1km to resolve the relevant Delaware Bay circulation. We will add a few sentences to this effect.

"Figure 6: Please, be considerate of colour-blinded people and avoid having green and red on the same graph."

Of course, thank you for pointing it out.See attached

Section 4.4: Here too, I would appreciate some elements of discussion including comparison with peer-reviewed studies. This is an interesting result, which, beyond including it as a Model development, can have consequences for particle modelling in shallow marine areas in the future.

It is long known that stokes transport has an impact on coastal/estuarine circulation. We will add some discussion.

(Monismith and Fong(2004)) and in particular Delaware Bay(Pareja Roman et al. 2019)

Monismith, S. G. and D. A. Fong (2004). "A note on the potential transport of scalars and

organisms by surface waves." Limnology and Oceanography 49(4): 1214-1217.

Pareja□Roman, L. F., et al. (2019). "Effects of Locally Generated Wind Waves on the Momentum Budget and Subtidal Exchange in a Coastal Plain Estuary." Journal of Geophysical Research: Oceans 124(2): 1005-1028.

Feng, M., et al. (2011). "Ocean circulation, Stokes drift, and connectivity of western rock lobster (Panulirus cygnus) population." Canadian Journal of Fisheries and Aquatic Sciences 68(7): 1182-1196.

Kumar, N. and F. Feddersen (2017). "The Effect of Stokes Drift and Transient Rip Currents on the Inner Shelf. Part I: No Stratification." Journal of Physical Oceanography 47(1): 227-241.

van den Bremer, T. S. and O. Breivik (2018). "Stokes drift." Philos Trans A Math Phys Eng Sci 376(2111).

Kumar, N. and F. Feddersen (2017). "The Effect of Stokes Drift and Transient Rip Currents on the Inner Shelf. Part II: With Stratification." Journal of Physical Oceanography 47(1): 243-260.

Please also note the supplement to this comment:
https://gmd.copernicus.org/preprints/gmd-2021-400/gmd-2021-400-AC1-supplement.pdf