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Geoscientific Model Development Discussions

Interactive comment on "Numerical integrators for Lagrangian oceanography" *by* Tor Nordam and Rodrigo Duran

Anonymous Referee #1

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In this manuscript, the authors discuss the accuracy of different numerical integrators that are typically used in Lagrangian oceanography. They compare the efficiency and accuracy of a large set of these integrator algorithms in numerical flow fields with different spatial resolutions in the North Sea, off the coast of Norway.

The motivation of this manuscript is a really good one; many applications of Lagrangian oceanography choose an integrator without much idea about its efficiency and accuracy. This manuscript would therefore be very welcome to the field.

However, I feel that on a number of points, the manuscript could be further improved and clarified before I can recommend publication. In particular, I have the following major comments

1) The authors make the case for higher-order spatial interpolation, as if that is always better. However, there is no discussion at all about how the order of spatial interpolation is related to the order of the advection schemes within the OGCM from which the data is derived. Intuitively, I'd say that the most appropriate interpolation would be the scheme that most closely mimics the model advection scheme. I'd suggest the authors discuss this.

2) The authors also but then quickly step over the problem of interpolation near land. Higher-order interpolation would mean that the halo of land is further extended into the ocean (as very briefly mentioned in line 362). I feel that this should be given more discussion. How would this problem compare to the error that the 'consistency of order p of the numerical method is no longer satisfied when the derivatives are not continuous' (lines 222-223)

3) There is no discussion at all about the widely-used Analytical advection scheme of e.g. https://doi.org/10.5194/gmd-10-1733-2017. How does that scheme compare to the integrators discussed here?

4) The authors mention that they ignore diffusion (line 74). However, in most applications diffusion will be included in the computations. I wonder whether the errors caused by the finite-sized set in the Wiener process are not much larger than any errors in interpolation as discussed here. It would be good if the authors could comment on this.

5) I wonder why the authors don't test their method on (complex) flows where an exact solution is known. Quite a few of these flows have been used in the literature, including e.g. the Bickley Jet. That would save them a lot of challenges in defining the 'exact' solution

6) I am not sure if comparing ends points is the most appropriate metric. Why not compare the along-trajectory differences, which is commonly used in the field (e.g. https://doi.org/10.1029/2018JC014813), so that the full trajectories are taken into ac-

count

7) The authors spend a large amount of attention on their 'special-purpose integrators' (section 3.3). However, they don't mention that most implementations of Lagrangian integrators would quite naturally implement such special-purpose integrators, simply because they don't store all time slices in memory so that they need to stop integration on the time of each time slice in order to load the next one

And then also as minor comments

- line 4: clarify here that this is interpolation in space and time?

- line 16: 'computations on data from atmospheric models'?

- line 30: 'For all these applications'?

- line 36: 'capable' is an anthropomorphism; hyperbolic points are not capable of anything.

- line 36: By whom is this recommended?

- lines 43-54 and 126: It might be very useful to include a table with details of the different integrators, so that readers don't need to dig into the literature themselves to find out what the specifics are of each of these

- line 55: 'very common' is perhaps too strong? Lagrangian oceanography is still a bit of a niche

- line 93: why is x not bold here?

- line 104 and other places: Would errors e and E not always be absolute values?

- line 172: At least summarise where these values 2.5 and 0.8 come from

- line 176: How does this extent to staggered grids (e.g. Arakawa-C) which are often used in oceanography?

- line 191: The word 'quite' is somewhat vague here

- line 224: give some examples relevant to Lagrangian oceanography of these cases?

- Figure 1: Also show (some of) the trajectories here, to give readers a feeling for the extent of dispersion?

- line 320: it is unclear at what depth the particles are released, and also whether they are advected in 2D or in 3D

- line 329: is 'transport' the best word here?

- line 363: explain what kind of padding is done

- line 373: explain why this creates additional discontinuities

- line 381: what is the standard deviation/variability around this median? Are the differences between the runs larger than the variability within the runs?

- line 388, 389 and 390: even though the authors make a good point about comparing number of computations instead of runtime, here they still mention that runs are 'longer' and report runtime in seconds.

- Figure 4: I'm a bit confused by the number of points on each of these lines. Why do some have 11 points, even though according to table 1 there were only 9 time steps and 10 tolerances tested?

- Table 2: Would this data be easier to parse in a figure instead of a table?

- line 493: mention what is 'special' about these special-purpose integrators (e.g. 'that don't step across time grids' or something like that)

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