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Ocean Science
Discussions

Interactive comment on "Circulation of the European Northwest Shelf: A Lagrangian perspective" *by* Marcel Ricker and Emil V. Stanev

Anonymous Referee #2

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Summary

The authors have performed a set of Lagrangian particle tracking experiments to study the water circulation on the European Northwest Shelf (ENWS). Several scenarios were simulated, with particles (passive tracers, or water masses) released at surface and seafloor, and simulated forwards for up to 1 year, plus one case with backwards simulations. A property called "density trend" is defined to aid the analysis of the spatial accumulation of particles.

General comments

As the authors themselves point out, several modeling studies have looked at the ENWS, but not so many studies have applied Lagrangian methods, at least not for

the whole area. The simulated scenarios are sensible, and the discussion contains several interesting comments and findings, though nothing groundbreaking. The main weakness of the paper is that the discussion would need a more clear structure, and be better linked to well defined motivation/objectives. But after improving the structure (i.e. major revision) and some details as discussed below, I would find this manuscript suitable for publication.

Specific comments

Line 29: Missing end parenthesis.

Lines 40-50 discusses typical current patterns. It would be helpful with a figure with arrows to better follow this description.

Line 50: Could ref to Fig2c for the comment about low salinity along coast.

Lines 50-52: This major hypothesis should be reflected also in abstract.

Lines 60-62: Sentence is a bit hard to read.

Line 75: Should mention here that vertical mixing is also not considered. This is an important point, that should also be discussed/justified.

Line 89: It is not clear whether the area of Fig 1 is identical to the AMM7 area, or if this is a subset?

Line 90: AMM7 is called a model, but perhaps "model setup" is more precise?

Line 93: Here the term "tracer" is used. It should be made clear whether tracer and particles are the same thing in this study.

Line 95: Please provide a reference or justification for the choice of eddy diffusivity. It should be commented that this is constant throughout the area (which is not true in reality).

Line 96: Eddy viscosity should be a positive number.

Section 2.2: More information should be given about the drifter type/characteristics/name, as near-surface drifters are affected by a varying degree of Stokes drift and wind drag, see e.g. Röhrs, J., K. H. Christensen, L. R. Hole, G. Broström, M. Drivdal, and S. Sundby (2012), Observation-based evaluation of surface wave effects on currents and trajectory forecasts, Ocean Dyn., 62, 1519–1533

Thus, a missing contribution from Stokes drift can possibly explain why the model currents are too slow in the comparison. Alternatively, SVP drifters (15m depth) from the Global Drifter Program could be used to validate the model current, so that Stokes drift would not be an issue. Also a plot of the complete drifter trajectories should be shown, to justify whether they cover a substantial part of the area, or just locally to their deployment location.

Line 148/Table1: The number of comparison points should be provided.

Section 2.3. This discussion is a bit messy, and does also belong in the results section, rather than under “material and methods”.

Line 184: It could be made clear (the first time) that Figure S2a refers to figure 2a in the supplements.

Line 207: could be commented that the Molinari and Kirway study is for the Caribbean during summer, thus quite different conditions.

Line 240: It should be commented (and discussed) that vertical mixing is not included.

Line 242: Should also be mentioned here that particles are released over the whole domain.

Line 244-246: The seeding locations of CR-V should also be shown on a figure

Line 248: It should be mentioned explicitly that a separate offline trajectory model has to be used for the backwards simulations, as this is not possible to do with online simulations. However, a forward simulation with this offline model should also be done

to benchmark it against the online forward simulations.

Lines 274-279: What would be the difference between “density trend” and “residence time”?

Line 278: “motionless situation” is a bit unclear, please rewrite sentence.

Line 302-304: Please clarify what is meant here.

Line 461: extra space after “Channel”

Section 3 is a bit lengthy, and hard to read due to jumping back and forth between the experiments and referring to many figures. Making it a bit more compact and structured would help.

Figures

There are a lot of composite figures/maps of the area of interest. These are quite small and hard to read when printed on A4 paper. Could whitespace be reduced somehow?

In the figure captions, the letters a), b)... should rather be placed before the explanation, and not after

Figure 2: CR and NTE should be written explicitly as “control run” and “no tides experiment”, so that the figure can be read and understood also before reading the main text. Same for other figures. Line 847: und -> and

Figure 3: a bit much spaghetti here, perhaps use even fewer than every 5th trajectory?

Figure 4: Caption is quite hard to read. The ‘+’ and ‘-’ symbols are presumably placed “by hand”? This is generally ok, but they are quite many, and sometimes slightly displaced, perhaps to avoid overlap? So in practice I don’t think these symbols work very well here. Could the point be visualized by another, more objective measure?

Figure 5: Title of lower figure is “monthly average”, but I guess it should be “yearly average”, or “average of months”

References

Please update this reference, where you refer to a discussion paper: Dagestad, K.-F., Röhrs, J., Breivik, Ø., and Ådlandsvik, B.: OpenDrift v1.0: a generic framework for trajectory modelling, Geosci. Model Dev., 11, 1405–1420, https://doi.org/10.5194/gmd-11-1405-2018, 2018.

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