Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-245-RC1, 2019

Geoscientific Model Development Discussions

Interactive comment on "Tracking water masses using passive-tracer transport in NEMO v3.4 with NEMOTAM: application to North Atlantic Deep Water and North Atlantic Subtropical Mode Water" *by* Dafydd Stephenson et al.

Anonymous Referee #1

Received and published: 13 December 2019

General Comments:

Stephenson et al. describe a promising and interesting addition to the toolbox for visualizing flow in ocean models. This tool is available only within the context of a tangent-linear and adjoint model but it has an advantage over the more commonly used method of Lagrangian particle tracking: insensitivity to the number of particles simulated. The authors then illustrate the types of results possible with their tool with two well known water masses: North Atlantic Subtropical Mode Water and North Atlantic Deep Water. These results are broadly consistent with results from other models and observations.

Finally, as an oceanographer whose expertise is in Lagrangian methods and the water masses discussed in this paper but **not** with tangent-linear and adjoint models (TAM), I found the casting of the TAM approach into a purely passive tracer framework to be exceptionally illuminating; Section 2.2 in particular helped me understand the physicality behind TAMs. In my opinion, this paper contributes both a new method for tracking water masses as well as a special, simplifying case of the TAM approach; both will be useful. I recommend this paper for publication in GMD with some minor clarifications listed below.

Specific Comments:

This paper does not explicitly compare this new method to an established baseline (Lagrangian particle tracking, standard tracer methods). In particular computational demands are cited as one reason for using this method over Lagrangian particles yet there are no run time statistics in the manuscript. In my opinion, explicit quantification of the additional computational resource overhead necessary for running the TAM and this method, and insight into how this overhead scales with model resolution, is necessary in order for other scientists to fully evaluate the merits of this method relative to other methods. In the conclusion, the authors suggest it could be used with higher resolution models (i.e. ORCA12) as an "off-the-shelf" addition. That's definitely intriguing and I think more basic information about resources is necessary to support interest along these lines. Note that here I am *not* suggesting that the authors do any particle tracking. Rather, I think it's important to document what kinds of resources are necessary to run this method. Describing these run times will also help illustrate the actual TAM tracer workflow.

Page 2: near line 25: "along with the ability to re-use a single "trajectory" run of a nonlinear model, offer an advantage over passive tracer tracking in a nonlinear OGCM." How exactly is this re-use different from re-using existing output from an eddy-resolving OGCM for different particle tracking studies? (For example, already cited in the manuscript, Gary et al. (2011, 2014) and Burkholder and Lozier (2011) used

different particle tracks from the same model output in three separate papers.)

Page 4, near line 15: The Bower et al. 2009 and 2011 studies did not use profiling floats, only RAFOS floats, which did not profile.

Page 6, near line 10: “Below the surface, it occupies a narrow latitudinal band at depths of up to 240 m (Fig. 2, red shading).” This maximum depth of 240 m is inconsistent with the greater maximum depths reported on page 9 near line 30. Perhaps the 240 m is the time mean maximum depth or a thickness? Please clarify.

Page 6, near line 15, “Additionally, eddy-induced advective velocities, although present in the nonlinear trajectory, were not included for our passive tracers in NEMOTAM.” Are these velocities the same as a Gent-McWilliams (GM90) eddy parameterization? If they were used in the model trajectory, why were they not used to move the tracers as well?

Page 12, near line 5, “This suggests that the NASMW surface outcrop is not, in fact, the dominant origin of NASMW...”. I am confused by the use of “dominant” in this statement because it seems to me that it is not consistent with Fig. 8a which shows that a significant fraction of the NASMW (up to 70%) comes from the outcrop region. Granted, Fig. 8a is on a 0-5 year time scale and other panels in Fig. 8 clearly show sources from outside the outcrop region on longer time scales, but the contributions of those other sources outside the outcrop region is still less than half of all the NASMW. Please clarify.

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