

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 #2

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Review of “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 Stephenson et al

In this manuscript, the authors present an analysis of the pathways of NADW and NASMW in the NEMOTAM model, focussing on the long time scales and basin-scale pathways in their 2degree resolution simulation.

While the manuscript is well-written and easy to follow, I am not entirely sure it fits the

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scope of GMD in its present form. Most of the manuscript is about the physical interpretation of the water mass pathways, and there is relatively little information about the technical details of the implementation. On the other hand, the low resolution (even for climate models) means that it is unclear how representative/relevant the results are to the physical oceanography community. Since there is hardly any comparison to observational evidence of the pathways (CFCs? C14 dating?), I suspect the manuscript in its present form would raise questions in a physical oceanography journal too.

I think, however that it should be feasible to rewrite the manuscript to a more traditional GMD manuscript for the audience here to find it interesting. In particular, I think the authors would then need to:

- 1) Add much more information about the technical details of the model. What is its memory/cpu usage? How does it scale?
- 2) Add more information about the implementation of the model. What exactly is meant with a ‘perturbation’ (page 4, line 28)? A perturbation to what? How does the result depend on the choice of non-perturbed state? Is that irrelevant because of the assumption of linearity? How good is this assumption of linearity anyways? When and where does it break down? How big are the resulting errors?
- 3) Add much more validation of the model implementation. Do the TAM results indeed agree qualitatively with a full (nonlinear) tracer experiment in the ‘normal’ model? And how would this change when changing resolution?

Furthermore, I have the following smaller comments:

- page 1, line 2: Be more specific what is ‘probabilistic’ about the tool. Is it the diffusive component?
- page 1, line 5: Be clear that the fact that tracer is removed upon contact with the surface is a choice?
- page 2, line 1: ‘bijective’ is a not very common word. Explain?

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- page 2, line 8: How many of these floats (order of magnitude) have been deployed?
- page 2, line 15 (and later): it is not true that an 'infinite' number of virtual particles are needed. Depending on machine precision, the tracer concentration is also not simulated to infinite accuracy.
- page 2, line 24: what exactly is meant with a 'probabilistic pathway'?
- page 3, line 9: The recent Bower et al review paper on Atlantic pathways (<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JC015014>) is not mentioned here?
- page 5, line 2: perhaps very briefly explain bra-ket notation to oceanographers?
- page 6, line 4: what is the effect of the non-resolvement of the ice layers in NEMO-TAM? Why are these not implemented? Is it technically impossible?
- page 9, line 23: why not impose an Ertel PV criterion, as is very often done? Is that possible within the TAM framework?
- page 10, line 12: I don't understand the meaning of the word 'mechanically' here
- page 13, line 5: How sure are the authors that this indeed is a lognormal distribution? I would have liked to see a goodness-of-fit analysis. There are other distributions that produce similar-looking PDFs
- Figure 8: I am somewhat surprised that some of the NASMW originates near Greenland? Wasn't one of the conditions that the temperature was higher than 17C? Does that occur within the model in the region near Greenland??
- Figure 10: This bar chart is somewhat confusing because the blue bars raise higher than the striped ones. I assume they are individually normalised? Would it not make more sense to put them on the same y-scale?
- page 16, line 3: There is plenty of evidence that NADW is not formed annually, but

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only in some years. That the model does have annual formation clearly is a bias. This should be clearly stated

- page 23, line 26: So where is the rest of the NADW, if it doesn't account for 100%?
- page 25, line 16 (and other places): here, the authors present their model results as if they realistically describe the ocean physics. However, with such a low resolution and known biases, I would prefer to see many more placements of caveats like 'within this model's context' etc
- page 26, line 15: Long runs are not necessary for NEMOTAM to work, but only for the applications chosen here right? If the authors would have focussed on other applications, they could have used shorter runs?
- page 26, line 18: How about 'looping' the NEMO fields? Would that work?

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