

Interactive comment on “Can organic matter flux profiles be diagnosed using remineralisation rates derived from observed tracers and modelled ocean transport rates?” by J. D. Wilson et al.

Anonymous Referee #1

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General comments:

Wilson et al first use circulation fields from a global model (in form of transport matrices, TM), and observed phosphate to diagnose remineralization rates of PO₄, from which one could potentially diagnose particle flux curves. Secondly, they use the same approach with TM and simulated nutrients of another global model (GENIE), plus observations, to assess the error of this method with respect to uncertainties in observations and circulation. In a final set of experiments they investigate the role of DOM on the diagnosed flux profiles.

The idea and experimental setup of the work presented in this paper is very interest-

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ing and novel. The comprehensive analysis of different errors adds important insight into global biogeochemical model performance and evaluation. With regard to this, I definitely suggest publication in Biogeosciences. However, the form of presentation could be improved, with respect to method description (time steps associated with the TM construction; biogeochemical rates), presentation of results (this relates mainly to the figures), and the discussion, which could be more focused, comprehensive, and acknowledge some recent and older literature. See specific comments listed below. I would therefore like to encourage the authors to re-organise and revise the paper, to make these interesting results more easily accessible to a wider audience.

Specific comments:

- (Section 2) I find the description of time stepping and time scales associated with the inversion and the diagnosed ISS a bit confusing - what is the unit of dt? Alternatively: what is the time step length for the construction of GENIE's TM? This also relates to the colour scales/units of figures.

- (Section 2.1) What was the reason for choosing the different locations at which the model was evaluated? Are there data sets to compare the simulated remineralisation rates (or fluxes) to?

- (Section 2.1) "This example shows that a simple inversion of [PO₄] observations using this approach is susceptible to large errors that will likely hinder their interpretation." - This is somehow vague. Is this only related to the MITgcm circulation? The fact that there is no clear pattern for ISS in 2000m when the MITgcm TM is used together with observations, but there seems to be a pattern for the GENIE TM, when diagnosed with the synthetic data set to me is a bit puzzling. Wouldn't it be interesting, to have the same comparison (TM with PO₄ observations, as for MITgcm) for the GENIE TM?

- (Section 3) The biogeochemical model description could be more comprehensive, and easier to find; currently, it is described under the (rather vague) title "Experiment design". In particular, I think the description of particle sinking and remineralisation,

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as well as DOM remineralisation (I assume it is a first order process) could be briefly explained, and the parameters should be given here as well. This will help the reader to put the results (remineralisation vs circulation) into perspective, without having to look up another paper.

- (Section 3) Were there (large) differences between the online and offline (i.e., TM) version of GENIE?

- (Section 3.3.2) ERR-OBS: As far as I understand, WOA (1x1 degree annual mean?) was regridded onto the (rather coarse) GENIE grid. How were the SDs calculated? Are these from WOA, then averaged onto the coarse grid? Or does the calculation of SD include both the SD from WOA, as well as the variance due to regridding (e.g., Krist et al., 2010).

- (Section 4.1) p. 4568: Why have a subsection (4.1) with only one subsubsection (4.1.1)? I would suggest to have either two subsubsections (e.g., "4.1.1. Results from the GENIE online model" then "4.2.2 Twin experiment"), or to combine everything into a single subsection.

- (Section 4.2.1) See above, comment for section (3.3.2): where does the variability in the observations come from: WOA, regridding, or both?

- (Section 4.2.1) p. 4570, line 8: "distributions" to me sounds a bit misleading; what about "clusters"?

- (Section 4.2.1) p. 4570, line 11: What is "1-A"?

- (Section 4.2.1) p. 4570, lines 12-13: "This suggests that the ISS uncertainty is a function of the way the TM is constructed." - I am having difficulties to understand this reasoning. Assuming there is only little transport (even in the online model), but variability of observations in large, wouldn't this result in the same pattern?

- (Section 4.2.2), p. 4571: I may have missed an important point, but here it is not clear to me why physical transport of the online model is related to the diagnosed (ISS) rem-

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ineralisation. In particular, as far as I understand eq. 3 assumes that physical fluxes in/out of each box should equal remineralisation. If this is correct, doesn't remineralisation/(online physical transport) of only about 0.001 imply that the TM constructed is very different to the online circulation?

- (Section 4.2.3) A plot of the error distribution would be interesting, and help to better understand the reason for distinguishing between median and mean.

- (Section 4.2.3) p. 4572, line 9-12: "The patterns in the surface PO₄ ISSs from the MITGCM inversion are systematic which may suggest that errors are predominantly related to the ocean model (Fig. 2a) although this is less the case for the deeper ocean (Fig. 2b)." - How do the surface PO₄ patterns (Fig 2a?) point towards errors caused by the MITgcm?

- (Section 5) I find this section potentially very useful; however, discussing this before the background of remineralisation rates of DOM and POM used in the online GCM may be even more elucidating.

- (Section 5) Why have a subsection 5.1 if there is no subsection 5.2?

- (Section 5.1) page 4572, line 24: "which is converted to a flux curve by adding 1 (Stanley et al., 2012; ..." - The relation between flux and remineralisation was already noted by Martin et al., (1987; their eqn. 7), so I suggest to cite their paper.

- (Section 5.1) page 4573, lines 10-11: I cannot follow the authors' conclusion "The DOM bias in GENIE occurs predominantly in the high latitudes where DOM is efficiently advected into the ocean interior." - If DOM was advected deep into the ocean, where it then (quickly?) remineralised, deep convection in the Southern ocean should result in steeper flux profiles, i.e. higher flux exponents (e.g., b closer to -0.5).

- (Section 6) p 4573, lines 21-22: not everyone would call a 2.8x2.8 degree model a "high resolution model".

- (Section 6) p 4574, lines 3-6: "Previous methods have relied on relating multiple

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tracers together such that the model transport 5 terms cancel out e.g. Anderson and Sarmiento (1994); Sarmiento et al. (2002) and is a method which could be applied using the TM." - What is the meaning of this sentence?

- (Section 6) The assessment of circulation-based error via salinity could be in a separate subsection.

- (Section 6) The meaning of the last few sentences to me is quite unclear. I would strongly suggest some more in depth discussion of the results of the present study before the background of other model studies: for example, Kwon and Primeau could constrain b of a very simple model from PO₄ data (without having to spinup the model for thousands of years). Their model also included DOM, and they provided constraints for its production and decay parameters. Decades ago, Bacastow and Maier-Reimer (1991) set up models similar to the one used in this study, and carried out experiments with different sinking speeds and DOM/no DOM. Finally, DOM, its role in the regulation of nutrient fields, and its interplay with circulation has a long "history" in modelling, and this has been examined in many studies (e.g., Najjar et al., 2007).

- Table 1 and its caption - please explain, why there are 8 boxes. "The amount in italics is the estimated remineralisation" - is this the number in the lower right corner?

- Figure 1: Is depth relative to z_0 ?

- Figure 2 (and other figures): Some of the panels are very small. In some figures (e.g., 2b) it is very difficult to distinguish positive from negative values. The units are different (e.g., mmol m⁻³ dt⁻¹ in Fig 2 vs nmol kg⁻¹ dt⁻¹ in Fig 4 and 6), making it difficult to compare the different figures. Sometimes the units on colour bar don't seem to be correct (e.g., Fig 3c,d; no time constant for flux in figure, but in caption), which is quite confusing.

Technical corrections:

p. 4564, line 12: "susceptible"?

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p. 4568, line 2 "matrix"

p. 4575, line 5: "Redefining"

Literature:

Bacastow, R., and E. Maier-Reimer (1991), Dissolved organic carbon in modeling oceanic new production, *Global Biogeochem. Cycles*, 5(1), 71–85.

Kriest, I., S. Khatiwala, and A. Oschlies (2010), Towards an assessment of simple global marine biogeochemical models of different complexity, *Prog. Oceanogr.*, 86, 337–360, doi:10.1016/j.pocean.2010.05.002.

Najjar, R. G., et al. (2007), Impact of circulation on export production, dissolved organic matter and dissolved oxygen in the ocean: Results from Phase II of the Ocean Carbon-cycle Model Intercomparison Project (OCMIP-2), *Global Biogeochem. Cycles*, 21, GB3007, doi:10.1029/2006GB002857.

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