Response to Reviewers' comments on Wilson et al., "Can organic matter flux profiles be diagnosed using remineralisation rates derived from observed tracers and modelled ocean transport rates?"

We would like to thank both reviewers for their constructive feedback. We have addressed all the comments in a revised manuscript which are described below and have also restructured the manuscript in response to comments from both reviewers. The reviewer comments below are shown in bold and are followed by our response with details of specific changes made to the text where appropriate.

Response to Anonymous Referee #1:

Specific Comments:

1: (Section 2) I find the description of time stepping and time-scales associated with the inversion and 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 of figures.

We thank the reviewer for highlighting this as an issue. The units dt^{-1} are intended to reflect the remineralisation rate calculated over the model timestep. Our original intention to use this unit was to keep a focus on the fact that remineralisation rate estimates are a quantity dependent on the ocean circulation model. We have taken on board comments from both reviewers on this and have changed the units to $year^{-1}$ throughout the manuscript as we agree that this is a much clearer unit for interpretation. We have also added a clearer statement of the timestep to the text:

"The length of the time step at which the TM is diagnosed in GENIE is 0.01 year."

2: (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?

The sites were chosen somewhat arbitrarily with the intent to give a latitudinal cross section of the estimated remineralisation rates for comparison against Henson *et al.,* (2012). The profiles were chosen to display a range of solutions from those that were completely implausible (i.e., negative rates) to those that were consistent with expectations. We agree that a comparison with observations would also be useful. However, there are a number of issues with this. Given that there are a range of positive and negative values, a direct comparison against observed remineralisation rates, such as from AOUR, highlights that the estimated profiles are in error which is already highlighted by Figure 2. Equally, apparent oxygen utilisation rates will still have the spatial averaging issue highlighted in the Introduction, which would add another level of uncertainty or require the averaging of estimated rates over large regions which is difficult due to the range of negative and positive values which average to very small near-zero values.

In response to the reviewer's comment, we have added a new panel to Figure 2 showing values of 'b' for a Martin Curve power-law function fitted to the estimated remineralisation rates using the method introduced and used later in the manuscript for the discussion of DOM remineralisation. The global mean value for 'b' is -0.90, which is within the range of previous global values found by both data and modelling studies, e.g., Kwon & Primeau (2006); Henson *et al.* (2012). However, the range of fitted values is relatively large (±2.65 1 SD) showing that some flux curves unrealistically increase with depth. The added panel also provides a complete example of the method to estimate remineralisation rates and infer flux curves. This helps to define the scope of the manuscript and complements the discussion of uncertainties from other sources of remineralisation such as DOM later in the manuscript:

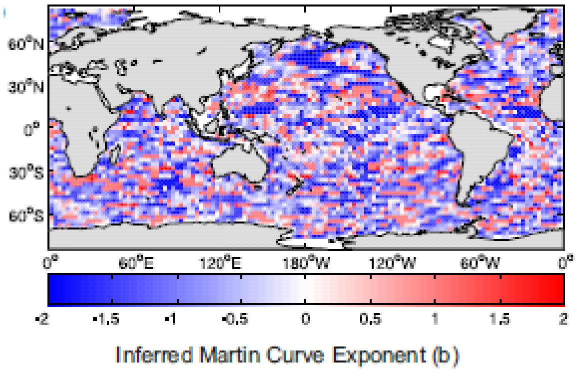


Figure 2c: Power law flux curve exponents 2b inferred from the estimated remineralisation rates by fitting a linear function to the log transformed data as in Fig. 1b.

3: (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?

This is intended to refer to the use of any TM, not just the MITgcm. We have clarified this statement in the text as follows:

"The negative ISSs and positive values for b suggest that the simple inversion of [PO_4] observations using this approach is susceptible to large errors that warrant further analysis to characterise and quantify potential uncertainties with this method when used with any modelled circulation."

In reference to the pattern observed for the synthetic data, the TM in the initial submission included the effects of virtual salinity fluxes to account for the effects of dilution/concentration on fixed volume grid-boxes caused by precipitation and evaporation at the surface (Edwards and Marsh 2005; Ridgwell *et al.,* 2007). This leads to the spatial patterns referred to by the reviewer in Fig. 4e and 4f as well as the minor deviations from the 1:1 line in Fig. 4a. This can be accounted for by normalising a tracer concentration before multiplication with the TM and converting the ISS back (as per the approach used for biogeochemical tracers in GENIE: Ridgwell *et al.,* 2007). This removes the minor deviations to the 1:1 line and removes the spatial patterns in Fig. 4e and 4f. As the magnitude of this effect is relatively small compared to the errors found in the manuscript, this does not change the results of the manuscript. All results in this revision have been altered to take this into account. We have added a description of this to the text:

"The TM diagnosed here includes the effects of virtual salinity fluxes applied in the ocean model to account for changes in volume (Edwards & Marsh 2005; Ridgwell *et al.,* 2007). To account for this, all tracer concentrations multiplied by the TM are first normalised by salinity and converted back to a concentration afterwards (Ridgwell *et al.,* 2007)."

We have not included remineralisation rates estimated using GENIE and observed [PO_4] as we feel this would detract from the use of GENIE as a model test of the method. We have retitled this section as "Uncertainty Analysis" to clarify the use of GENIE in the manuscript.

4: (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, as well as the 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.

We have removed the "Experiment design" subtitle and have added a brief description of the biogeochemical in the text:

"In this, nutrients in the surface grid-boxes are utilised by biological activity limited by [PO_4] according to a Michaelis-Menton type limitation (a maximum rate of 1.96 μmol kg^{-1} and half saturation constant of 0.22 μmol kg^{-1}) and the ambient light levels (a linear limitation term). A fixed fraction of the uptake 66% is exported from the surface as DOM which can be transported by circulation and remineralised with a time constant of 1/0.5 year."

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

There were no large differences between the online circulation and that diagnosed in the TM, as shown by reproducing the remineralisation field. We note that the TM is not being used to run the model, only invert tracer fields.

6: (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 the WOA, as well as the variance due to regridding (e.g., Kriest et al., 2010).

We thank the reviewer for helping to clarify this point. The SDs reflect the SD of the WOA observations which were then regridded onto the coarser GENIE grid. In response to reviewer #2's comments, this has been changed to the standard error (SE) but still reflects the standard error of the WOA observations that are then regridded onto the GENIE grid. As such, the SEs we use reflect only uncertainty in the creation of the climatology from observations and not from the subsequent regridding to the GENIE grid. We have amended the text to make this clearer:

"We do not consider any additional uncertainty here that may arise through the re-gridding process, i.e. Kriest *et al.,* (2010)."

7: (Section 4.1) p. 4568. Why have a section (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.

We have restructured this section and have removed the spurious subsubsections.

8: (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?

As well as responding to the previous comment (6), we have removed some of the text in this section that might suggest the uncertainty from the re-gridding process is also considered:

Removed text –" as well as re-gridding the observations onto a model grid such as GENIE or MITGCM."

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

We agree and have changed "distributions" to "clusters"

"However, two clusters can be broadly defined in Fig 5c both with separate linear trends that correspond well with the size of the ``flux out'' term of the TM (see Table 1)."

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

We thank the reviewer for highlighting, this is a mistake and should read (A-I), as in equation 3 of the original manuscript. This has been corrected in the text.

11: (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?

Figure 5c shows that the magnitude of uncertainty increases linearly with the magnitude of the uncertainty in the observations, *i.e.*, that ISSs generated from observations with large uncertainty also have relatively higher uncertainty. It also shows a cluster of ISSs that increase linearly but by a greater amount. This cluster occurs where circulation fluxes are larger in the model, as indicated by the colour scale. Effectively, this means the residence time of tracer in these grid-boxes is much smaller, *i.e.*, there is a bigger throughput of PO_4 associated with circulation during the model time-step. Commonly this occurs where convection occurs in the model as this is where those fluxes are largest. Therefore, if the magnitude of uncertainty of observations was fixed across all grid boxes, this effect of the TM would still lead to greater uncertainty in the ISSs in certain grid-boxes. As such, this is a caveat related to the TM and not the observations.

We have amended the text to make this clearer. We have also removed the trend lines from Figure 5c as these detract from the main interpretation, have amended the colour scale to reflect a "flux out" term, and have added a panel to Figure 5 to demonstrate where these grid-boxes occur:

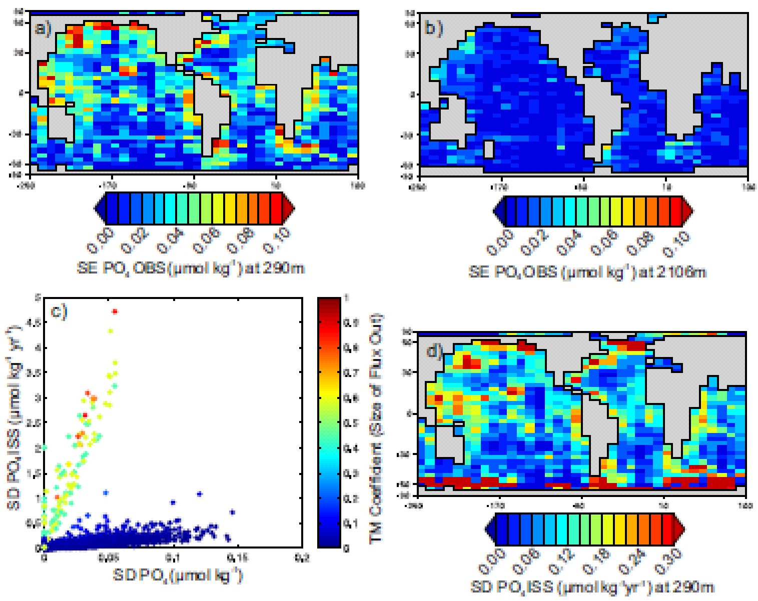


Figure 5: Assessment of the errors arising from the uncertainty in $[PO_4]$ observations. (a) The SE of $[PO_4]$ from the World Ocean Atlas 2009 (Garcia et al., 2010) 1° climatology regridded to the GENIE grid at 290m and (b) 2106m. (c) SD of all PO_4 interior source/sink estimates when the synthetic $[PO_4]$ field is randomly perturbed within a normal distribution given by the SE of observations. The colour scale indicates the size of the `flux out' term where a larger value indicates relatively larger circulation fluxes in that grid-box, (d) The SD of $[PO_4]$ ISSs at 290m.}

“This suggests that the ISS uncertainty is partly a function of the circulation diagnosed in the TM, i.e., if the observation uncertainty were fixed to a constant across all grid-boxes, ISSs in some grid-boxes will have greater uncertainty than others due to this effect.”

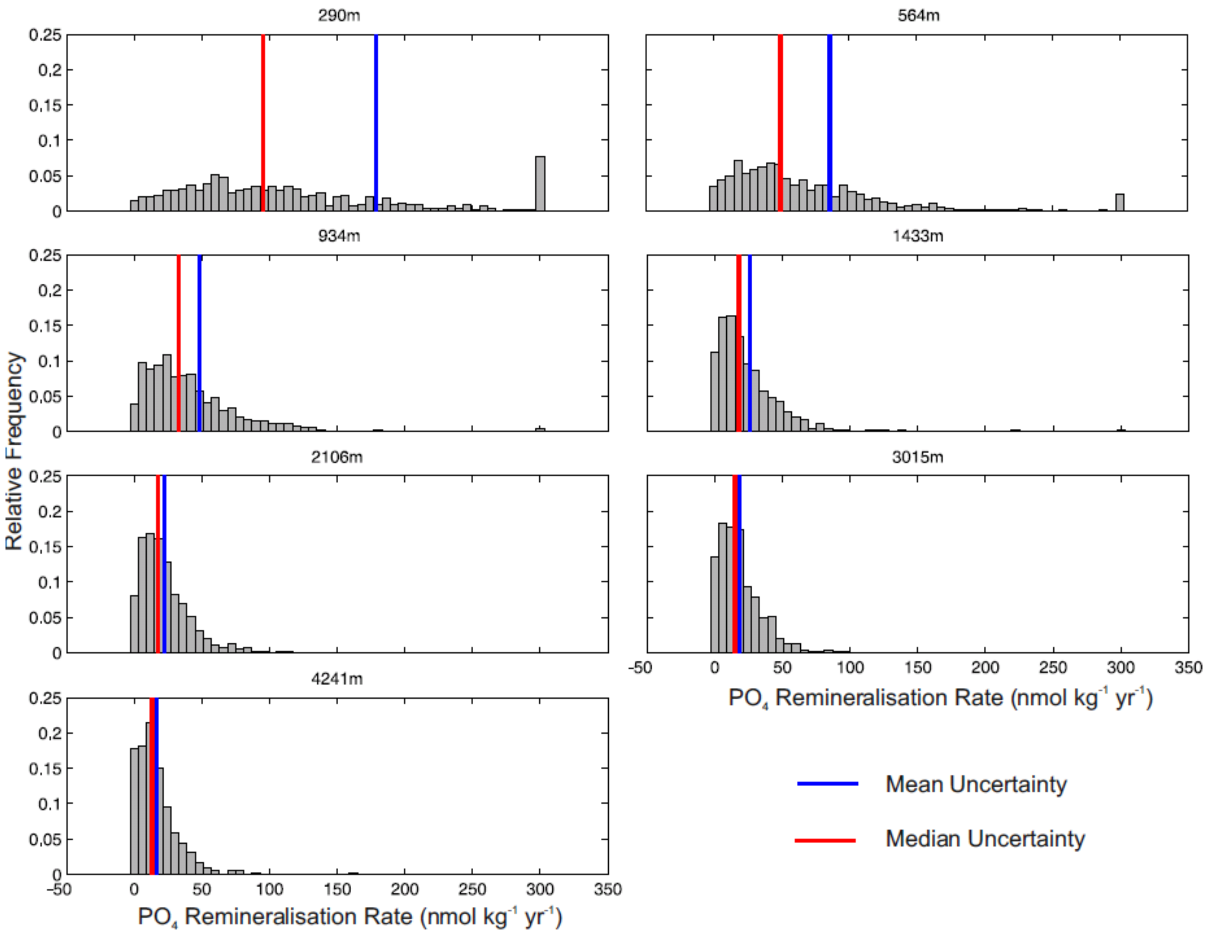
12: (Section 4.2.2), p. 4571: I may have missed an important point, but here it is not clear to me why physical transport of the online model is related to the diagnosed (ISS) remineralisation. In particular, as far as I understand eq.3 assumes that physical fluxes in/out of each box should equal remineralisation. 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?

W report the remineralisation rate as a proportion of the flux of PO_4 into the grid box. The reviewer is correct in their assertion that physical fluxes into the box minus physical fluxes out of each box should equal the remineralisation term at steady state, but here we are only considering the physical flux in term. We have added a clarification of this to the text:

“To illustrate this, we compare the steady state circulation flux of PO_4 into a grid-box with the remineralisation flux of PO_4 into each grid-box from the synthetic run (the sum of these at steady state will equal the flux of PO_4 out of the box).”

13: (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.

We agree with the reviewer that this would help. As this requires displaying the error distribution for each depth level requiring a large number of panels we have added two plots to the supplementary material showing the distribution of data in Figure 8 for each depth level with the mean and median indicated for both the uncertainties from the observations and circulation.



Supplementary Figure 2: Histograms showing the distribution, mean and median standard deviation of estimated PO_4 remineralisation rates when simulating uncertainty associated with observations as shown in Figure 8 for each depth level in GENIE. The histograms show the distribution standard deviations calculated for all grid-boxes at that depth level when observation uncertainty is simulated. The blue and red lines indicate the mean and median respectively corresponding with the values shown in Figure 8. Histograms are shown with a relative frequency as the number of samples is different between depth levels. Values > 300 nmol kg^{-1} yr^{-1} are included in the last bin.

14: (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?

We thank the reviewer for highlighting this. We are referring to systematic features that appear in Figure 2, such as the large estimated remineralisation rates in the Southern Ocean. However, our results show this could be related to both observational and model circulation error. We have amended the text to reflect this:

"Although the patterns in the surface PO_4 ISSs from the MITgcm inversion are systematic (patches of positive and negative ISSs in the Southern Ocean: Fig 2a) it is difficult to tell whether this reflects a systematic difference between observed and modelled circulation rates, or a caveat associated with convection in the model and larger circulation fluxes in these area"

(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.

We have moved this section to form part of the discussion so that the uncertainty from DOM is presented and discussed in full. We have also expanded on this in the introduction. We hope this provides a more logical structure of the manuscript.

16: (Section 5) Why have a subsection 5.1 if there is not subsection 5.2?

We have removed subsection 5.1 and have restructured the manuscript.

17: (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.

The reviewer is correct that Martin et al., (1987) noted and used this method. Our use of Stanley et al., (2012) is due to their application to estimated rates derived directly from tracer observations whereas Martin *et al.,* (1987) applied this to sediment trap observations of particulate fluxes. We have added Martin *et al.,* (1987) to the text.

18: (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 in the ocean, where it then (quickly?) remineraliased, deep convection in the Southern ocean should result in steeper flux profiles, i.e., higher flux exponents (e.g., closer to -0.5).

We thank the reviewer for highlighting this and agree with their point. Figure 9b does show this occurring in a few water columns in the North Atlantic where the fitted values of 'b' actually increase (a deepening of the flux curve). This is consistent with convection in the model in that region. Here, DOM is efficiently advected into the ocean interior and remineralised deeper than in other regions, such as in the Southern Ocean. The uncertainty created from DOM does not, therefore, always lead to a shallower bias in estimated flux curves. We have amended the text to reflect this as follows:

"In a few grid-boxes in the North Atlantic the estimate of the flux curve exponent increases. This has a strong correspondence with the deepest convection (Figure 3.4f) where DOM is transported deeper into the water column before remineralisation. This highlights that an additional source of remineralisation not restricted to vertical processes can alter the flux curve in an unpredictable way."

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

We agree with the reviewer and have removed "high resolution".

20: **(Section 6) p 4574, line 3-6: "Previous methods have relied on relating multiple 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?**

Specifically, estimates of remineralisation rates could be derived from [PO_4] and [NO_3] for example, on the assumption that the error from the model circulation will cancel out. On consideration of the reviewer's comment, we have removed this from the discussion.

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

We have added an additional subsection in the discussion to discuss the constraints on the circulation uncertainty.

22: (Section 6) The meaning of the last few sentences to 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 spinup the model thousands of years). Their model also included DOM, and they provided constraints for its production and decay parameters. Decades, ago, Bacastow and Maier-Reminer (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, in its role in the regulations 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 etal., 2007).

We have restructured this section to explicitly first consider the results of the manuscript before then discussing further work with a more detailed description and expanded citations.

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

Typically there are 15 grid-boxes for each column of the GENIE TM, reflecting dye travelling to neighbouring grid-boxes above/below, to the east/west and north/south as well as neighbouring grid-boxes on the vertices due to the Gent-McWilliams parameterisation. The number of grid-boxes is either reduced where there is a boundary, e.g., the surface or sediment, or increased where there is convection. The 8 grid-boxes in Table 1 were intended to show a reduced version, but in hindsight as the reviewer alludes to, this is not clear.

In response we have reduced the example in Table 1 to 7 boxes, i.e., a central box and 6 neighbours and have included this in the caption text. We have also labelled each box with compass direction relative to the central box to help clarify the example. This also facilitates the comparison with the TMI method (Gebbie and Huybers 2010) in the discussion:

Table 1: Example of using a transport matrix to calculate PO_4 remineralisation (mol kg^{-1} yr^{-1}) in one grid-box from PO_4 concentrations (mol kg^{-1}) given in **c**. Grid-boxes, taken from a row of the TM, are arbitrarily numbered, where the 1 is the central grid-box where the calculation is taking place. The example shows a simplified situation where there are 6 neighbouring grid-boxes with their relation to the central grid-box given by the directions in brackets. Each coefficient in the 6 boxes represents the flux of PO_4 into the central box from that grid-box. The coefficient in the central grid-box for **A** (see Eq. 1) represents the amount of tracer left in the central grid-box after one timestep whilst the

Grid-Box	**A**	(**A** – **I**)	*c*
1 ('flux out')	0.9816	−0.0184	2.3439
2 ('north')	−0.0007	−0.0007	2.3430
3 ('south')	0.0086	0.0086	2.4334
4 ('east')	0.0002	0.0002	2.3529
5 ('west')	0.0005	0.0005	2.3615
6 ('above')	0.0097	0.0097	2.4433
7 ('below')	0.0001	0.0001	2.3369
	1.0000	0.0000	**0.0011**

coefficient for **A-I** (see Eq. 2) is the flux out, equal to 0.9816-1. The sum of coefficients is shown underneath with the estimated remineralisation (**q** in Eq. 2) in bold calculated as the sum of the element wise multiplications of **A-I** and **c**.

24: Figure 1 – is depth relative to z0?

The depth scale is absolute. The plot is plotted on the MITgcm grid, so z0 is ~25m making it appear like depth is relative to z0 as it is close to zero. We have added a reference to this in the caption of Figure 1.

25: Figure 2 (and other figures); Some of the panels are very small. In some figures (e.g., 2b) it the very difficult to distinguish positive from negative values. The units are difference (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.

We have replotted Figure 2 using a more appropriate colour scale that highlights positive vs. negative values with a white zero value. The different scales are a result of trying to show both spatial patterns, as the focus of this manuscript, and changes with depth, as we are dealing with water column remineralisation. Unfortunately, large differences in magnitude make it difficult to plot on the same scale. We have highlighted this in the figure captions where this occurs. We thank the reviewer for highlighting the units on the colour bars of Figure 3c and 3d and have amended them to match the caption.

Response to Anonymous Referee #2

General Comments:

1: In order to have a good publishable result, the authors should undertake additional work to develop this method so that it can yield robust estimates of remineralization rates.

The intended scope of our manuscript was to introduce a potential approach to exploring spatial variability in particulate organic matter fluxes estimated from remineralisation rates, and to identify and quantify the sources of error associated with this approach. Ultimately our error assessment shows that the potential errors are very large, and as the reviewer notes, our manuscript does not develop the method to the point of yielding robust estimates of remineralisation rates. Firstly, we feel that this is outside the scope of the manuscript and would warrant another manuscript to fully describe any further development. We highlighted a potential first step in accounting for the errors associated with using modelled circulation rates based on previously published methods using conservative tracers to constrain the error. However, the number of conservative tracers available would lead to an underdetermined problem, even if we applied a 7-point stencil as per Gebbie & Huybers (2010). We did look into various methods, including adding additional constraints based on the range of circulation rates in the 54 member ensemble, but found they could not reliably constrain the errors or relied heavily on assumptions. As such, we focused on identifying and quantifying the sources of error to provide a resource for future work. Secondly, by maintaining a scope on quantifying errors, we can also explore uncertainties associated with using robust estimates of remineralisation rates to infer flux profiles, e.g., Martin *et al.,* (1987) and Stanley *et al.,* (2012). Ultimately, our analysis suggests that even with robust estimates, there are still significant uncertainties that will affect the interpretation of flux curves. This makes the manuscript relevant to a wider range of studies, including those that are observation based, and also highlights other approaches such as optimising ocean biogeochemical models are an important next step.

We have extended the discussion of the limitations to constraining the sources of error identified in the manuscript as well as restructuring the manuscript in line with other comments from both reviewers to clarify the scope of the manuscript.

2: Many similar "inverse" models that have been developed and applied successfully to elucidate aspects of the ocean's biological pump functioning (some cited in this paper and others not).

There are inverse approaches that we have not touched on in this manuscript. Notably, there are a number of studies that describe the biological pump function in a model and fit values to infer aspects of the biological pump, e.g., Schlitzer *et al.,* (2002); Kwon and Primeau (2006); Kriest *et al.* (2012); Yao and Schlitzer (2013). As per the response to **(1)**, we have tried to maintain a focus on directly estimating remineralisation rates in order to infer particulate organic matter flux curves. We have expanded the discussion with additional citations to better describe potential alternatives to the method we have presented and how they are related to each other to help stimulate further developments in this area.

3: The TMI method has been used to determine rates of mass transport as well, using radiocarbon data (Gebbie and Huybers, 2011).

We have added this citation to the text:

"This is because the TMI method reflects the pathways of ocean transport but not the rates of transport Gebbie & Huybers (2010), although Gebbie & Huybers (2012) additional information concerning rates can be estimated when combining the TMI method with radiocarbon data.

4: equation 1: The equation appears to be wrong. The authors don't state the units of A (which are typically dt^-1), but there are no units of A that could make the equation correct because c has units of (mol kg^-1) and q has units of (mol kg dt^-1). So the units on the left-hand side and right-hand side are not the same. If the units of A are dt^-1, the correct equation is dc/dt = A*c + q. Page 4563, equations 2 and 3. Again, appears not to be correct (see above). For (3) it should be q = -A*c.

We thank for the reviewer for highlighting the inconsistency between the equations and the units. Equation 1 corresponds to equation (2) in Khatiwala (2007) for the discretised advection-diffusion equation. In the method of Khatiwala et al. (2005), the finite difference tendency is calculated from a model:

$$\frac{d\mathbf{c}}{dt} = \frac{\mathbf{c}^{n+1} - \mathbf{c}^{n}}{\Delta t} = \mathbf{A}'^{n}\mathbf{c}^{n} + \mathbf{q}'^{n}$$

for which the units given for **q** in the manuscript (mol kg^{-1} dt^{-1}) would be correct, and as the reviewer highlights the units of **A'** will be dt^{-1}. Equation (1) of the manuscript is derived by rearranging the above equation for $\boldsymbol{c}^{n+1}$:

$$\mathbf{c}^{\mathrm{n}+1} = (\mathbf{I} + \mathbf{A}'\Delta t)\mathbf{c}^{n} + \mathbf{q}'^{\mathrm{n}}\Delta t$$

In our manuscript, we do not diagnose the finite difference tendency in GENIE, just the tracer distribution resulting a unit flux at the next time step so our matrix $\mathbf{A}$=$(\mathbf{I} + \mathbf{A}'\Delta t)$. $\mathbf{A}$ is now unitless, and the source/sink term ($\mathbf{q} = \mathbf{q}'^{\boldsymbol{n}}\Delta \boldsymbol{t}$) is mol kg^{-1}. This is now the equation in form given by Khatiwala (2007). Therefore, the units stated in the manuscript are not correct and should mol kg^{-1} for $\mathbf{q}$.

Rearranging equation (1) in the manuscript for **q**, assuming for steady state that $\mathbf{c}$=$\mathbf{c}^{n+1}$=$\mathbf{c}^{n}$, i.e., the tracer concentration does not evolve through time, gives:

$$\mathbf{q} = (\mathbf{A} - \mathbf{I})\mathbf{c}$$

The coefficients in matrix $\mathbf{A}$ can be conceptually understood as mapping how the tracer concentration at any one point on the model grid changes due to the net effect of model circulation during one timestep. $(\mathbf{A} - \mathbf{I})$ can be conceptually understood as calculating the net change in a tracer due to the model circulation in one timestep. We have added this to the supplementary material.

5: Figure 1: (a) Labeling one curve as high-latitude and one as low-latitude is a bit misleading, since these are not based on actual data, and the differences in observed particle flux attenuation from high-lat vs. low-lat regions is not so cut and dry. (b) is impossible to interpret due to x-axis scale. c) Is this just a repeat of (b) on a log scale?

We thank for the reviewer for this feedback. We have removed panel b from Figure 1 leaving only the remineralisation rate profiles on a log scale. This complements the description of the method and Introduction. We have also removed the profiles from Figure 2 in light of this feedback and have replaced it with a map of fitted 'b' values derived from the estimated remineralisation rates. This facilitates a more direct comparison with previous studies and avoids complications when referring to high/low latitudes.

6: Figure 2: c) Again very hard to interpret because of scales

Please see response to **(5)**.

7: Figure 3 f) How is the cost function defined?

The cost function is the average of number of grid-boxes in each water column that are mixed by convection. Higher values indicate deeper convection on average. We have changed the figure caption to reflect this.

8: Figure 5 and associated discussion: The use of random errors for the PO4 field is not appropriate here. The errors are significantly spatially correlated – which probably has important implications for inferring the remineralization flux. It would also be more appropriate to use the standard error, rather than the SD.

We have replaced the standard deviation of the observations with the standard error. The errors are intended to be illustrative but spatially correlated errors will need to be considered for future work. We thank the reviewers for highlighting the issue of spatially correlated errors and have added this to the text:

"We note that errors in the observations may be spatially correlated which will warrant consideration in future work."

9: Figure 5 and throughout: Should replace mol kg^-1 dt^-1 with something interpretable (like mol kg^-1 yr^-1)

We have replotted all figures with units of yr^{-1}.

10: Figure 5: Hard to tell how large the error in the diagnosed ISS are relative to the actual ISS

The errors are generally much larger than the diagnosed ISSs such that plotting relative standard deviation to highlight the relative error size makes it difficult to highlight the effect of coefficients on the diagnosed ISSs. A comparison of the size of errors versus the ISSs can be seen in Figure 8. We have therefore kept Figure 5c but have added a description of the size of the errors to the text:

"The resulting variability in the PO_4 ISSs, as characterised by the SD, are relatively large compared to the ISS values themselves, around 1--3 orders of magnitude larger than the ISS values (see also Fig. 8)."

11: Page 4571, lines 16-18. The authors identify exactly the problem with this approach. So there needs to be some way to move beyond or modify this point-by-point approach

Please see response to **(1)**

12: Page 4572, line 10 ff. The pattern of ISS in fig. 2 probably appears relatively smooth because the smooth mapped observations were used.

We thank the reviewer for highlighting this. The smooth mapped observations are used because missing data values will be propagated by the transport matrix resulting in a sparse number of remineralisation estimates. We have added this point to the manuscript:

"The relative smoothness of the ISS estimates may reflect the use of the climatology which has already been smoothed (Garcia *et al.,* 2010)."

13: Section 5.1 This is an interesting section showing the effect of DOM on the inferred particle flux profiles. However, it's a bit out of place here because the particle flux profiles cannot be diagnosed using the method the authors present.

We feel that a discussion of DOM, or more generally sources/sinks that are not restricted to the vertical water column, is still relevant to the scope of the manuscript. We are aware of papers that have used remineralisation rate profiles to infer particulate flux curves, e.g., Feely *et al.,* (2004); Stanley *et al.,* (2012); Sonnerup *et al.,* (2013), but there has not been an assessment of this particular uncertainty. Given that the manuscript focuses on moving on from the AOUR approach to estimate the spatial patterns in particulate organic matter fluxes, the uncertainty from integrating remineralisation rates vertically we feel that this fits within the scope of the study. We have restructured the manuscript to better reflect its scope.

14: Page 4573, line 21: coarse resolution ocean model

The MITgcm is a high resolution model in comparison to GENIE, we have amended the text in the manuscript to reflect this:

"…in an example inversion using a circulation field from a coarse resolution ocean model"

15: l Section 6: This section presents some interesting ideas, but unfortunately none are followed through on.

Please see response to **(1)**

16: Page 4575, line 10 ff. The method of Gebbie and Huybers is basically exactly this. They just adopt a 7-point stencil for fluxes between boxes so that the problem can be solved.

We have added this to the text:

"It would be interesting to see if a simplified TM with fewer coefficients, such as matching the method of Gebbie & Huybers (2010) by adopting a 7-point stencil, could use this approach"

17: Figure 8: I found this to be an odd way to represent these results. Also it is very hard to see the PO4 remineralization rate on this scale

We have expanded the discussion of the figure to reflect specific examples and to clarify that we do not expect a strong correspondence between the two:

“Comparing the errors from the inversion of the synthetic [PO_4] field using the same TM shows that the two have some visible similarities (Fig. 9e and f). For example, there are correspondences in the deep South Atlantic (Fig. 9d and e) and the subtropical regions of the Pacific at 290m (Fig. 9c and e). We do not expect an exact correspondence but the visual similarities support the idea that they are related via errors in the model circulation.”

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