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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 #2

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Review of "Can organic matter flux profiles be diagnosed using remineralisation rates derived from observed tracers and modelled ocean transport rates?", by J. D. Wilson, A. Ridgewell, and S. Barker

This study explores a method whereby observed tracer data (in this case phosphate, PO4) is combined with an ocean model to estimate PO4 remineralization rates in the interior ocean. The authors ultimately conclude that the method is highly sensitive to both uncertainties in the tracer observations as well as (potential) uncertainties in the modeled ocean circulation. Therefore, the method as implemented in this work is not suitable for estimating remineralization rates in the ocean, or for estimating particle flux profiles.

While there were some interesting points raised in the manuscript, overall the work done does not appear to rise to the level of publishable beyond the current discussion format. The authors start with a reasonable idea of combining PO4 observations with a model-estimated flow field to determine PO4 remineralization rates in the interior ocean (which is essentially the circulation divergence of PO4). Anyone who has examined the divergence of observed tracer data in a model circulation field will attest that it is very noisy. The authors demonstrate this problem, and then put forth several potential reasons for this noisiness (errors in the observations or in the circulation model), but unfortunately do not go very far beyond this. 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. 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). Unfortunately this work falls somewhat short in that respect. If the authors are able to move beyond the problems identified in this manuscript, I would recommend submission of a new manuscript. I commend the authors for the hard work taken to get to this point, and hope that there is a good way to deal with the issues that the authors raise in this discussion paper.

I also found a couple potential errors and more minor issues in the manuscript, as detailed below.

Specific items: Page 4561, line 18-19: The TMI method has been used to determine rates of mass transport as well, using radiocarbon data (Gebbie and Huybers, 2011). Page 4562, equation 1: The equation appears to be wrong. The authors don't state the units of A (which are typically dtˆ-1), but there are no units of A that could make the equation correct because c has units of (mol kgˆ-1) and q has units of (mol kg dtˆ-1). So the units on the left-hand side and right-hand side are not the same. If the units of A are dtˆ-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. 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? Figure 2: c) Again very hard to interpret because of scales. Figure 3 f) How is the cost function defined? 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. Figure 5 and throughout: Should replace mol kgˆ-1 dtˆ-1 with something interpretable (like mol kgˆ-1 yrˆ-1) Figure 5: Hard to tell how large the error in the diagnosed ISS are relative to the actual ISS. 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. Page 4572, line 10 ff. The pattern of ISS in fig. 2 probably appears relatively smooth because the smooth mapped observations were used. 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. Page 4573, line 21: coarse-resolution ocean model Section 6: This section presents some interesting ideas, but unfortunately none are followed through on. 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. 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.

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