

Interactive comment on “The biogeochemical structuring role of horizontal stirring: Lagrangian perspectives on iron delivery downstream of the Kerguelen plateau” by F. d’Ovidio et al.

Anonymous Referee #1

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This is an interesting paper addressing a very important issue. As the scientific community focuses more and more on interdisciplinary studies, the issue of adequately sampling biogeochemical processes becomes crucial for success. The authors present an interesting case study for the Kerguelen Plateau in the Southern Ocean. I do think that the impact of the paper could be substantially increased if the authors provided more details about the analyses and implementation of the methodology. As it is now, I think it would be quite difficult for other researchers to implement similar approaches for other areas, as several details about the implementation are not well described. Addressing this issue would make this a very useful paper.

General comments:

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1) The usefulness of the paper would be increased dramatically if more details were provided, which would make it easier for other researchers to apply the same methodology to other regions. For example, more details on the calculation and use of FSLE would have been useful. How was Figure 4 computed? Was there any consideration for cumulative errors in the advection scheme? How was the removal constants in section 3.4 computed? I can certainly make educated guesses of what the authors may have done, but it would be nice to have a clear and complete description of the procedures used.

2) For Appendix 1, page 799, from line 11 on: is iron homogenous over the plateau? Do the authors have enough data over large enough spatial and temporal scales to confidently answer this question? I am wondering if using the long-term average water flux is really a good idea. Can't errors be quite large, especially if there is significant spatial variability in the iron content over the plateau? I think that, at the very minimum, the authors should make a detailed uncertainty analysis here. In other words, they could introduce uncertainties in the measured and calculated fields and propagate those in their calculations to estimate how large the errors can be.

Specific comments:

1) I think the paper desperately needs a figure with the region's topography and a clear delineation of the plateau. The authors mention several times features in the measured and/computed fields (chl, age) with reference to the “northern flank” or “eastern flank” of the plateau, or the shelfbreak, but the reader has no idea where the plateau is, and what is its extension. There is also reference to another plateau (Heard), but again, there is no information about where that is located. That made it very difficult to follow some of the writing. I had a very difficult time with section 3.1, for example – the authors mention several features in the figures, but I was never sure if I was looking at the same feature they were referring to. It may be useful to add some labels to the features, so that the reader can easily identify them.

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2) Overall, I think the figures need to be improved. Fig 2: Very difficult to see the labels. The same is true for Fig. 4. I am not sure I understand the color scale in Figures 5 and 6. Does dark blue in Figure 5 really means an age of 0 days and an origin latitude of -55? It doesn't seem like that, in which case the figure is confusing/misleading. For Figure 6, panel d, I imagine that the dark red/dark blue are areas where the origin cannot be computed. If so, that should be clearly plotted in another color. In Figure 1, I think it would be useful to add a contour showing the location of the bloom (e.g., a chl contour delineating the bloom boundary). That would help give a scale of the blooms in relation to the eddies and gradients in SSH.

3) Typos: Page 786, line 26; P788, L17; P789, L3; P792, L2

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