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Interactive comment on "Chemometric perspectives on plankton community responses to natural iron fertilization over and downstream of the Kerguelen Plateau in the Southern Ocean" *by* T. W. Trull et al.

T. W. Trull et al.

diana.davies@csiro.au

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Interactive comment on "Chemometric perspectives on plankton community responses to natural iron fertilization over and downstream of the Kerguelen Plateau in the Southern Ocean" by T. W. Trull et al.

Anonymous Referee #1 Received and published: 27 October 2014

This is overall a very interesting and informative manuscript (MS), as one of the many contributions from the KEOPS2 expedition. In the MS, the surveyed area over and

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downstream of the Kerguelen Plateau was clustered into 5 groups based on ocean circulation patterns and characteristics of natural iron fertilization. For each group, a wide range of original data, including POC, BSi/POC, d13C, and d15N, were measured for various plankton size groups. These measurements were further used as proxies to estimate size-specific biomass, fraction of diatoms, growth rate, and f-ratio, respectively. The authors also calculated the N and Si depletion in the water column and estimated-export production based on these calculations. Setting these data in the context of the whole KEOPS2 study, the authors gave a detailed picture of the different responses of the plankton community to various types of natural iron fertilization, namely, the punctual and high level vs. the persistent yet relatively low iron supply, and came to several interesting points, e.g., the carbon export was decoupled from surface biomass, and the export could be higher in areas with low but lasting iron supply relative to areas with high but punctual supply. The authors showed innovative utilization of several chemical proxies (although some of them have very large uncertainties), and discussed in great depth about the re- lationship between iron fertilization and carbon export. I would recommend this MS for publication on Biogeosciences, after the following comments are addressed, and a thorough proofreading is done. Comments:

1. One interesting point the authors made is that the carbon export in the area with long-lasting but low iron supply may exceed that in area with episodic and strong iron supply. I would like to see a clearer definition of the time window of the carbon export the authors are examining and comparing. It seems that accumulation of biomass and export reported in the Polar Front Plume region represent an early phase of the iron-induced phytoplankton bloom, with a large standing stock of biomass in the mixed layer waiting to be exported, while the water in the recirculation feature has experience one or several full cycle(s) of phytoplankton growth and export. Considering the lag of export after the bloom, would export in the Polar Front region be much higher, and the conclusion be very different, if the experiment were extended for one more month?

AUTHOR RESPONSE

BGD
11, C7455–C7461, 2014

We agree with the reviewer that it is not possible to know the subsequent evolution of export over the seasonal cycle, but it is possible that this would change the perspectives that apply for our observed spring period. We added text to explicitly recognize this, in the Results section 3.5:

MODIFIED TEXT

Of course observation of these variations in spring does not mean that they would have persisted into summer, and it is possible that over the full season the extent of nutrient depletion was significantly different, either towards homogeneity across the region or towards larger variations.

Is it possible to define a term T that is the days from the initiation of phytoplankton blooms to the day of sampling for each of the 5 groups, and compare the export in the unit of mmol m-2 day-1?

AUTHOR RESPONSE

We provided two time metrics in the text: "time since fertilization" and "time since biomass accumulation" but both of these can only be estimated very approximately (at best two within a few weeks), and neither provides information on when export actually began, so we prefer to make the comparisons in the context of these approximate time frames in the text and not to provide false quantification. To make the times more clear we now list both of them in a revised version of Table 1:

2. The integration depth of the Group 5 (downstream PF plumes) stations based on the S-threshold method is overall significantly smaller than other stations. The choice of the S-threshold method over the T-min method thus accounts largely for the conclusion that the export in the Polar Front plume area was smaller than that in the recirculation area. It is possible that the authors are comparing water columns without much stratification since winter mixing to water columns that have recently being stratified and shoaled? A fuller description regarding the evolution of the hydrological structure would be very

BGD
11, C7455–C7461, 2014

helpful.

AUTHOR RESPONSE

The reviewer is correct that the choice of depth for the nutrient depletion estimate has a very strong influence, in particular for these sites at the Polar Front which show salinity stratification above the depth of the winter-derived temperature minimum. And this is exactly why the Tmin depth should not be used, because stratification by horizontal mixing has re-defined the stratification and nutrient profiles between the two depths (Tmin and Sthreshold) more recently than the end of winter. Because the high biomass layer found in these Polar Frontal sites is in this shallow salinity-defined layer, and because the Fe fertilization of these waters is recent as shown by their short transit time since crossing the plateau (because the flow along the Polar Front is fast as determined from both altimetry and drifter releases (d'Ovidio et al., 2014; Park et al., 2014). We have added information on this to the text in section 3.5:

MODIFIED TEXt

We believe the Sthreshold approach is the most appropriate given the observed salinity stratification, especially for the relatively weak subsurface thermal stratification observed in the Group 5 stations near the Polar Front, where it's choice makes the most significant difference from estimates based on the Tmin approach. This is because the high biomass layer found in these Polar Frontal sites is in this shallow salinity-defined layer, and because the Fe fertilization of these waters is recent as shown by their short transit time of $\sim$ 2 weeks since crossing the plateau as determined from both altimetry and drifter releases (d'Ovidio et al., 2014; Park et al., 2014). Thus attribution of nutrient depletion below the depth of the Sthreshold to iron fertilized biomass production is not warranted.

3. The authors talked at several points in the MS about the influence of lateral transports on the calculated f-ratio and export production. Considering that the influence of lateral transport may be very different in the Polar Front Plume and the recirculation

BGD

11, C7455–C7461, 2014

area, a more quantitative description about the lateral transports (e.g., timing, current in m/s) will be very helpful .

AUTHOR RESPONSE

We agree that this information is important, and we have summarized it in the context description in Methods Sections 2.1 and 2.2, provided an overview of the timing in Table 1, and included an animation of the biomass transport in the supplementary materials with a running calendar. Because the transport pathways are complex, time-varying, and their understanding requires detailed figures and discussion, it is best to refer readers to the sources of this information in the papers by Park et al., 2014 and d'Ovidio et al., 2014, as we have done in both these Methods sections and in the Results section 3.5. .

4. In the discussion (section 4.1), the authors reported that the growth rate calculated from the d13C measurements is higher in G4, then G3 and G5 and then G1 and G2. However, there does not seem to be significant difference between G1, G2, G3 and G5 on Figure 5. In addition, it seems that the model results, compared with the 13C uptake results, tend to over-estimate the growth rate by a factor of 2. Can the authors provided a little more discussion about the uncertainty of the d13C isotopic fractionation model method, e.g. , a sensitivity test on the growth rate derived from different assumptions about the cell shape and dimensions?

AUTHOR RESPONSE

We agree with the reviewer that this issue was insufficiently addressed and we have added several sections of new text that describe the large uncertainties in our calculated growth rates and emphasize that the overall conclusions do not rely upon them alone. For the full details, please see our extended response to Reviewer2 on this issue, which includes these new sections of text.

There are some minor issues the authors may need to consider:

BGD

11, C7455–C7461, 2014

1. It is probably more proper to move Section 2.2 and 2.3 to the Chapter 3 (Results) s they are reporting actual data in great details;

AUTHOR RESPONSE

Because this results come from other papers, as cited, we prefer to keep them in the Methods section along with all the other information on oceanographic context.

2. Line 27, pg. 13847: what is the difference between A3-1 and A3-2?

AUTHOR RESPONSE

We added text to explain that these names reflect two visits to the same site.

3. Line 26, pg. 13850: do you mean “plateau <= Polar Front plume”?

AUTHOR RESPONSE Yes, thank you, and we corrected this typo as suggested.

5. Line 24, pg. 13857: Missing digit after “8.”?

AUTHOR RESPONSE

Yes, thank you, and we corrected this typo to show the full value of 8.0.

6. Line 18, pg 13861: what does the 13C-POCrs mean for the heterotrophic dominated size fractions?

AUTHOR RESPONSE

Heterotrophs tend to have 13C-POC values similar to their prey, with additional contributions from low 13C lipid reserves for organisms that form them. We added text as follows:

MODIFIED TEXT

The presence of lipid-rich zooplankton in the two largest size fractions is another probable cause of their low 13C-POC values, based on low 13C-POC values for zooplankton collected with nets during KEOPS2 (Carlotti, 2014).

BGD

11, C7455–C7461, 2014

7. Figure 1. a) Latitude and Longitude on the left-bottom corner of the figure is not very readable. Could you put the numbers out of the box? b). Is it possible to show the location of the Station R on this figure?

AUTHOR RESPONSE

We made both changes as requested.

8. Figure 2. Kerguelen and Heart Island on this map are not very distinguishable from the clouds. Is it possible to mark the islands using darker color?

AUTHOR RESPONSE

We made this improvement as requested.

8. Figure 3. The x-axis in the middle panel is log(size), while on other figures it shows "filter size". It seems to be more straightforward to use "filter size".

AUTHOR RESPONSE

We made this improvement as requested.

Interactive comment on Biogeosciences Discuss., 11, 13841, 2014.