Biogeosciences

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Discussions

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

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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 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 relationship 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? 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?

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 helpful.

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

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?

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

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

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

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

4. Line 24, pg. 13857: Missing digit after "8."?

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

6. 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?

7. 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?

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".

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