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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 #2

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In this manuscript results from the KEOPS2 survey in the vicinity of the Kerguelen Islands are presented. The purpose of the study is the understanding of the impact of natural iron fertilization on productivity and biogeochemistry of the Southern Ocean. These studies are highly relevant to our understanding of the impact of changes in the SO biological pump on past (Glacial/Interglacial) and future atmospheric pCO2. Here results on the size-fractionated composition of particulate organic matter (BSi, POC, PON, ∂13C and ∂15N) as well as estimates on nutrient utilization and, by comparison with standing stocks, export are presented. Further, ∂13C and ∂15N of particulate size

fractionated organic matter is used to estimate growth rates and f-ratios of the different size classes in the community. Results are interpreted to infer the impact of different intensities in iron fertilization (based on hydrography and location) on community structure, and the impact of community structure on biogeochemistry.

I commend the authors on their effort to interpret the data, but must confess that I am not too convinced by the manuscript. Most of the data interpretation is based on indirect evidence itself based on assumptions that are possibly not valid (see also comments below). Further, there are better and more direct methods to study both community composition and export. Although I concur with the main conclusions of the study (i.e. high biomass and productivity does not necessarily lead to high export and is dependent on the community composition), this is already well known and the use of bulk parameters (as presented here) adds little to our understanding. Finally, when studying export (highly dependent not only on whole community but possibly on behavior of individual species), there is a temporal component not taken into account (i.e. most of the export does generally not occur during the growth phase of a bloom) and is possibly masked by the large spatial variations in the area of study. As the paper seems somewhat to be an attempt at synthesising results from the whole study, I would recommend the authors incorporate in their results and discussion other measurements (submitted in separate papers in this issue) in a more explicit manner.

Additional comments:

Lines 311-318: In the description of the community how were non-diatom protists (including heterotrophs important in the < 210μm size fractions) assessed? These tend to be more delicate and probably damaged during filtration.

Growth rates estimates from ∂13C of POC are based on the assumption that cells do not use bicarbonate. From previous laboratory studies, bicarbonate use is common and highly variable at a species-specific level (also dependent on light regime). I am not sure that any of the growth rates estimates given here are reliable. Also the au-

thors failed to refer to the studies on this topic: Burkhardt et al. (1999) Geochimica et Cosmochimica Acta, 63: 3729-3741, Burkhardt et al. (1999) Marine Ecology Progress Series, 184: 31-41; Rost et al. (2002) Limnology and Oceanography, 47(1): 120-128. I also fail to see large differences in growth rate estimates for the different groups (Fig. 5).

I am not sure of the logic in separating some of the stations in 2 groups (groups 1 and 2) as they are in a similar location and could be used to infer temporal development.

Lines 685-690: I am not sure I agree with the authors on the method used: estimating nutrient consumption from nutrient profiles is valid under the assumption that there is no significant impact of lateral transport. If there is horizontal exchange (or mixing), especially in an area with strong horizontal gradients such as in this study, nutrient consumption estimates are highly uncertain. Using the Tmin as a criterion, helps to at least constrains the temporal scale of the estimate (i.e. from previous winter), while using other criteria does not. Hence robustness of the estimates given here can hardly be assessed and I doubt values for the different groups can be compared.

Given the robustness of the different estimates and the variability (which might be related to both temporal and spatial patterns) I could also argue that there are no significant differences in organic matter (based on N) export between systems. When looking at figure 8 roughly half of the N uptake is lost (either through grazing or sinking). This is consistent with the fact that growth estimates are in the order of roughly one doubling every 3 days while biomass accumulation (from satellite Chla) indicates a doubling very week (between 28/10 and 6/11).

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