

## ***Interactive comment on “Nitrogen uptake by phytoplankton in the Atlantic sector of the Southern Ocean during late austral summer” by W. R. Joubert et al.***

**Anonymous Referee #1**

Received and published: 22 June 2011

### General comments

This paper concerns the assessment of nitrogen uptake, used also to derive potential export production, in different hydrographic regions encountered along a transect in the Atlantic Sector of the Southern Ocean. The paper contributes to the understanding of the biogeochemical cycle of nitrogen and, as a consequence, also of carbon in this part of the Southern Ocean. The Southern Ocean is a key area for atmospheric CO<sub>2</sub> sink and the paper provides new data to address a relevant scientific topic connected with the biological carbon pump. The work is based on the long and well-established stable isotope technique (<sup>15</sup>N-nutrient uptake) which provides instantaneous values of nutri-

C1651

ent uptake, which are time- and space-dependent and which, however, are not easy to extrapolate to general information. To overcome this limitation, the authors use also information derived from a different approach based on <sup>234</sup>Th flux. The two approaches give different results and the authors mention some of the reasons. However, a more detailed presentation of the principle and the meaning of both approaches would be highly appreciated, including advantages and disadvantages of the methods, in order to better understand the differences between <sup>15</sup>N and <sup>234</sup>Th based estimates. Method description should be improved according to the “Specific comments” below and some results (absolute and specific nitrogen uptake) are not clearly presented and their significance not sufficiently discussed. Discussion and conclusions should better highlight the contribution of the results obtained to the understanding of the role of the studied area to biological CO<sub>2</sub> uptake. The conclusions confirm the variability of the f-ratio (new vs total production) in different hydrographic conditions due to nutrient and iron availability and light conditions, which has been reported in other areas of the Southern Ocean. The comparison between the outcomes of the research with data from other regions should be given more attention. The title and the abstract clearly reflect the content of the paper. However, as the discussion should be improved (see “Specific comments”) in order to better highlight the importance of the results, also the abstract should be accordingly modified. The number and the quality of references cited is appropriate. As a general comment, the paper presents new data for the area, however an improvement in the discussion of the results obtained is necessary, also when comparing data from other regions of the Southern Ocean, in order to emphasize the contribution of this paper. In particular, as results are presented for different hydrographic regions, it is expected to recognise which region may contribute more to biological CO<sub>2</sub> uptake and export and for what reason.

### Specific comments

Introduction: a more detailed presentation of the principle and the meaning of estimates based on nutrient-uptake and on <sup>234</sup>Th deficit would be highly appreciated.

C1652

Methods: Fig. 1: please provide a larger map showing the general position of the studied area. Are surface temperature and salinity data presented in Fig. 2 derived from a thermosalinometer measuring surface data in continuous? It is not mentioned in the method description. In the Result section (e.g. page 4929 l. 22) the depth of the mixed layer is cited, please describe how you identified the mixed layer depth. For the same reason, as the depth corresponding to 1% light is indicated, describe how was light availability measured. Was there a PAR sensor associated to the CTD? Nitrogen uptake: it is written that tracer additions were approximately 10% of ambient nutrient concentrations; however, from the concentration of the inoculum it seems that  $^{15}\text{NH}_4$  additions were greater than 10% in most cases. Was the amount inoculated constant or was it modified during the cruise according to the expected natural concentration? Was new production in  $\text{mmol C m}^{-2} \text{d}^{-1}$  calculated from  $\text{NO}_3$  uptake using the in situ C:N ratio? It is written in the caption of Fig. 7, but it should be briefly mentioned also in the Methods section.

A list of abbreviations used in the paper could facilitate the reading and understanding of the text. Please note that APF has not been defined.

Results: Fig. 3: please improve the quality as labels are not easy to read. White dotted lines are shown in Fig.3, but their meaning is not described in the caption. You could add: "lines as in Fig. 1." SaccF and SBdy, cited in the result description (p.4926 l. 4-6) are not clearly defined in the figure (Fig. 3) (see previous comment). Page 4924-4925 l. 20-24; l. 1-3 silicate concentrations in STZ and SAZ seem similar, however from the comments in the results it seems that the SAZ is more depleted. Both absolute nitrogen uptake (N) and specific uptake (VN) are presented, however, unless a more in depth consideration on the significance of the two variables and on the spatial variability of N and of VN are presented, one of the two figures (Fig. 5 or Fig.6) can be removed, without losing information.

Discussion: Variations in nitrogen uptake dynamics are ascribed to several environmental parameters such as nutrient availability, mixed layer depth, light availability and

C1653

iron concentration. However, some of these parameters (mixed layer depth, light availability, iron concentration) are not presented in tables or figures. I suggest to insert in Table 1 the depth of the mixed layer and the depth of 1% light penetration, or to superimpose a line on Fig. 3. Has iron concentration been measured during the cruise or its role is derived from the literature? Page 4928 l. 25: why "alleviation of iron stress"? Are there any data on iron concentration during the cruise available? Page 4929: some areas in the Southern Ocean are either Silicate or Nitrate limited (see e.g. Goeyens et al, 2000 Nutrient depletions in the Ross Sea and their relation with pigment stocks. Journal of Marine Systems). From your data, it seems that SAZ and PFZ are Silicate limited. Could this sustain the planktonic size structure? A more extensive discussion on the meaning of estimates based on nutrient-uptake and on  $^{234}\text{Th}$  deficit would help to better understand the importance of the comparison. The relevance of the results obtained should be better highlighted by giving more attention to the comparison with data from other regions of the Southern Ocean (Table 2), in order to understand the role of the studied region to biological  $\text{CO}_2$  uptake and export. In particular, as results are presented for different hydrographic regions, it is expected to recognise which region may contribute more to export production and for what reason. This important information does not appear clearly from the discussion and conclusion and should be better emphasized.

Conclusions: Some conclusions seem not to be supported by data, but rather based on the literature (page 4932 l.7). References: Page 4937 l.15 and p.4939 l. 2: I do not think that an article "in preparation" could be cited

Technical corrections

some typographic errors (p. 4932 l. 8) Page 4941 l. 1 Table 1:Caption Page 4951 Fig. 6: In caption specific uptake per hour (V, h<sup>-1</sup>) on Y axis per day V d<sup>-1</sup>.

---

Interactive comment on Biogeosciences Discuss., 8, 4917, 2011.

C1654