

Interactive  
Comment

***Interactive comment on “Variations of net primary productivity and phytoplankton community composition in the Southern Ocean as estimated from ocean-color remote sensing data” by S. Takao et al.***

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After a careful reading of the paper entitled “Variations of net primary productivity and phytoplankton community composition in the Southern Ocean as estimated from ocean color remote sensing data”, I recommend this paper for publication in Biogeosciences Discussion.

The paper starts with a clear introduction and reminds the limitation of previous studies to assess spatio-temporal relationships between NPP, phytoplankton community com-

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position and SST in the Southern Ocean. The topic is important as it help understand how changes in phytoplankton community or SST could impact the NPP in the southern ocean. The authors also define clearly terms used in the following of the paper and their objectives.

The authors present an interesting analysis of the spatial, seasonal, inter annual variations and trends of NPP in the Southern Ocean. For the first time, large scale relationships between NPP, dominant groups of phytoplankton and SST are shown. The authors analysis focused on several oceanic fronts characterized by horizontal gradients and different hydrographic properties. They used an original variety of complementary observations, based on in situ and remote sensed measurements to perform their analysis. This approach is very interesting.

Each steps and tools are well defined and justified. Dataset and methods used are clearly defined in the major part of the paper (see minor comments below). The main result of their studies is the potential strong impact of phytoplankton community composition on NPP values. They also analyse very interesting results about different correlation between their parameters in function of the considered area and period. It will be crucial to take into account these results in any futures 'NPP studies'.

My minor concerns about this study are about the following questions :

- Why the SeaWifS period is limited to 1997-2007 only ? Good quality observations are available at least until 2009 and I think the extension of the period could strengthen the authors results, unless the authors have chosen this period only for other reasons ?
- I think the authors should also look at specific regional climatic variations such as those characterised by the SAM index, rather than focusing only on recent climate change. Could they discuss that also ?
- Could they explain why they have used matchup with +/- 2 days ? the classical definition for matchup are for 'Day 0' only.

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- In the 4.1 section the authors cite significant fraction of diatoms in the physat method based on the Alvain et al 2008 paper. A more recent study with daily measurements and a larger database have been published in Alvain et al 2012 (Optics express Vol 20) with 73% instead of 57% of good agreement with results based on pigment. This value is more in agreement with the authors result in the southern ocean. Could the authors update their paper ?

- It's not very clear if the data used for results page 4376 lines 3 to 9 (for example) are based on 3 months means or not ? Should it be possible to clarify this in the text ?

Some editing or minor concerns :

I find the organization of the paragraph 3.1.1, 3.1.2 and especially the 3.2 rather difficult to read. Should it be possible to clarify the text by a specific paragraph for each area or by the insertion of a table or something like that ?

Figure 4 : should it be possible to add the stars also in the figure b ?

Dear author, feel free to contact me if you need clarification or if you think I'm wrong. I will be very happy to modify my review if needed. S. Alvain

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