

Interactive comment on “Satellite observations of new phytoplankton blooms in the Maud Rise Polynya, Southern Ocean” by Babula Jena and Anilkumar Narayana Pillai

Anonymous Referee #2

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General comments The authors reported phytoplankton blooms in the Maud Rise Polynya, Southern Ocean, which was unseen earlier in spring seasons from entire records of ocean color satellite data acquisition. On the basis of satellite data from CZCS, SeaWiFS, MODIS, and VIIRS they showed the bloom appeared for the first time in the satellite records since 1978. The linkage between the observed bloom and the oceanic pCO₂ condition were studied using Argo data. The low pCO₂ values in the polynya was possibly due to the presence of chl-a bloom with high NPP, which has potential to drive CO₂ fluxes from the atmosphere to the ocean. The observed biological pumping process in the polynya could play an important role for lowering the atmospheric CO₂ through transferring of atmospheric CO₂ to the ocean. I would

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suggest the authors to revise the manuscript according to the following comments.

Specific comments Authors have shown the upwelling of high saline and warm water leads to melting sea-ice. While the conclusions are supported by the evidence, do the early loss of sea ice cover should lead to the warming of mixed layer through radiative heating particularly increase of shortwave radiation in the ocean surface in spring period? So the authors are suggested to look into any unusual enhancement of shortwave radiation in the polynya region during the study period compared to the long-term prevailed condition?? Any satellite or reanalysis/modeling product is adequate for this analysis.

Even though some of the works are now included by the authors for the analysis of mixed layer warming (comments from other reviewers) on the Maud Rise, the short-wave heat input in absence of ice cover is crucial. Yet the manuscript is structured superbly with scientific understanding and hard to find technical flaws. Some data are known to have uncertainty in the polar waters specifically remote sensing based primary productivity (level-4) viz. vgpm, eppey-vgpm, cbpm models, that lacking validation with ship measurements. But the uncertainties in remote sensing methods are apparently quantified by the authors using in-situ NPP estimated using ^{13}C tracer from the Indian scientific expedition to the polar waters.

Other minor comments: Line 15: Make expansion of 'MODIS' Line 140: It should be 'covered by the sea-ice' Line 170: Describe about the white contours in figure 2. Line 230: Are there any studies that quantifies carbon fixation by prymnesiophytes (*Phaeocystis antarctica*) and diatoms in Antarctica sea ice? Line 230: Diffuse attenuation coefficient at 490 may be included here (see the comments from the other author) and discuss in the text.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-282>, 2019.

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