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The Cryosphere Discussions

# *Interactive comment on* "Antarctic slush-ice algal accumulation not quantified through conventional satellite imagery: Beware the ice of March" *by* J. L. Lieser et al.

**Anonymous Referee #1**

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The paper "Antarctic slush-ice algal accumulation not quantified through conventional satellite imagery: Beware the ice of March" by Lieser et al. describes a combined satellite/field (and a little modeling) study of an algal bloom associated with sea ice near Cape Darnley, Antarctica. The blooms they report on are not typically observed in satellite ocean color imagery and therefore space-based estimates of ocean primary production in the Antarctic are underestimated.

While I agree that the observations they make are interesting, the way they are presented and interpreted lessens their possible impact and in fact make the paper unsuitable for publication in its present form. The problems are related to the organization,



results, and interpretation of the data. I will out line the issues with each of these aspects of the paper:

Organization The paper is currently divided into four sections (Introduction, Materials and methods, Results and discussion, and Conclusions), and seven appendices (plus supplemental information). This is far too much complexity for such a short paper. All relevant information should be moved from the appendices into the appropriate places in the main body of the paper. Furthermore, some of the main sections of the paper contain information that belongs in other sections. For example, sections 2.1 and 2.3 include results that should be moved into the Results section. The third paragraph of the Introduction also contains information that belongs in the Results section.

Results The main evidence for the ice-associated bloom consists of a true-color MODIS image from March 2 showing discolored sea ice and samples from seven stations in the sea ice-associated bloom. I tried blowing up Fig. 1 and was never able to see any evidence of discolored sea ice. This may just be an image quality issue, so if there is a better way to present this image, the authors should use it. I'm not saying the bloom was not there, I just couldn't see it in Fig. 1. The photograph in Fig. 2 shows the surface bloom nicely, but I have no way to know how far this phenomenon extended. In addition, the water samples from the bloom were collected at a depth of 4.8 m so it is not clear how representative of the surface bloom shown if Fig. 2 they are. It is very difficult to assess the relevance of these data without any information on chlorophyll distributions with depth. Did the authors have access to a CTD-mounted fluorometer that could have been used to obtain a profile of fluorescence? This would at least provide some information about how deep the surface bloom extended. I didn't find the modeling results to be very useful as included. Perhaps if they were a larger component of the paper, then they could be better utilized.

Interpretation The authors refer to the bloom they observed as an "intra-ice phytoplankton surface aggregation". The authors describe a scenario whereby phytoplankton are incorporated into frazil ice. This frazil ice eventually coalesces to form land-fast ice, which decays due to pulverization by wind and wave activity. This releases the algae back to the water column where they were observed by the authors. However, it is unclear whether the algae they observed were actively growing once released into the water from the ice. No direct measurements of algal physiology were made. It would be instructive to calculate whether the surface concentrations observed in Fig. 2 were sufficient to explain the concentrations measured at a depth of 4.8 m using the underway seawater system. Was the biomass in the ice sufficient to result in a chlorophyll concentration of 3 mg/m3 in the upper five meters? Absent this information, it is difficult to interpret the chlorophyll variability at 4.8 m (especially with the ship plowing through the sea ice and releasing its contents into the water).

Appendix 1 calculated how much chlorophyll we would expect in the ice if 1) all the algae were scavenged or 2) the algae in the ice used all the available silicate. Both approaches gave the same results, indicating that if there were much scavenging, there couldn't have been much growth in the ice. However, it seems like the important thing to know is whether these algae were growing once released from the ice. No results were presented to make this determination. If all they were seeing was ice algae released into the water column as the sea ice degraded, that is a lot less interesting than if they observed an active phytoplankton bloom or an algal bloom that was seeded with algae from the ice. (The authors need to be careful in their use of the word "phytoplankton". Algae growing in ice are not phytoplankton. Ice algae released into the water column are also not phytoplankton. Algae growing in the water column are phytoplankton).

One of the main points the authors make is that space-based assessments of primary production in these waters has been underestimated because these blooms are not detectable from space. This is already well known. Space-based measurements only quantify open water phytoplankton. They do not include algae associated with the sea ice, which all the papers on the subject recognize. However, having made the claim that past satellite studies underestimate primary production in Antarctic waters, the authors make no attempt to quantify how big this underestimate might be in their study area.



Satellite imagery like that shown in Fig. E1 could be used to estimate production over the year in their study region. Then, the productivity associated with the blooms they observed in the sea ice zone could also be assessed (even if it was simply based on the accumulated phytoplankton biomass that they observed). Then these two numbers could be compared to see how much productivity the satellite missed. This would be a much more important contribution than just asserting that satellites underestimate primary production.

Finally, much of the discussion about the factors controlling autumn bloom formation is highly speculative and should probably be removed. But to start, these are not autumn blooms. They were measured from 10 February to 10 March, which is late summer. So they are summer blooms. In any case, the authors discuss how strong winds may bring iron in through aeolian transport, but never say where the aeolian iron would have come from. The paper they cite for this is by Winton et al. (2014), which studied the Ross Sea where the Dry Valleys contain a great deal of exposed rock surfaces that could be the source of wind-blown iron. Are there bare land surfaces near Darnley Bay? They also say that the sea ice is a source of iron, which is possible, but give no information about what local iron concentrations in the ice were. As for grazing, the authors state, "Microbial grazers, such as heterotrophic flagellates, dinoflagellates and ciliates, were more abundant in the bloom, but their grazing was insufficient to limit phytoplankton growth. In the absence of samples to indicate metazoan grazer densities, it is not possible to quantify total grazing pressure for the ice-associated bloom." If it is not possible to quantify grazing pressure, how can the authors know that grazing was insufficient to limit phytoplankton growth? Finally, section 3.4 says that climate ENSO and SAM "may play a role" but doesn't say what that role is.

In short, there is simply too much missing information to make an adequate assessment of their observations. Had they made more direct measurements of phytoplankton vertical distributions and physiology, as well as the chemical and physical conditions associated with the bloom, then the story would have been much stronger. At this point, we don't even know how the biomass measured at 4.8 m relates to algae observed at the sea surface. Maybe they are the same or maybe not.

Technical issues Page 6189 Line 24. The introduction never mentions nutrients (e.g. iron) as a possible factor limiting production, but iron is a big topic throughout the rest of the paper.

Page 6191 Line 7. 10 February to 10 March is summer, not autumn

Page 6192 Lines 21-22. What were the error estimates associated with these concentrations? Lines 23-24. Why couldn't surface phytoplankton abundance be measured.

Page 6193. Line 1. The authors don't know what the phytoplankton biomass was in the bloom, which was a surface phenomenon (Fig. 2). They do know the concentration under the bloom. Lines 5-6. Were these larger cells of the same species? Line 13. Were Phaeocystis colonies or cells counted? How was the number of cells per colony determined?

Page 6194. Line 6-7. I would argue that ice-associated phytoplankton production is not underestimated. It simply hasn't been estimated (except using models) since satellite-based estimates purposely exclude the sea ice zone. Total (open water zone plus sea ice zone) production has been underestimated.

Page 6202. Line 7. I tried to view the animation but the site is password protected.

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Interactive comment on The Cryosphere Discuss., 9, 6187, 2015.