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

(replies in *italic*)

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 #2

Received and published: 14 December 2015

The authors thank the editor and Referee #2 for the comments on the manuscript. We note the reviewer's appreciation of the novel aspect of the work even though the presentation of the manuscript appears inadequate.

The topic of large and vast areas of algal incorporation and potential growth in forming pack ice is one that is important and likely deserves to be published. That said, there are many loose and confusing terms, unsubstantiated assertions and analyses presented in the present form of the ms that do not have a place in the published and peer reviewed literature.

To start, the title seems to a bad attempt at providing a cute and meaningless phrase to possibly garner attention. There is nothing to "beware of" when ice forms and it has algae in it. The attention garnered by providing such a meaningless title lends one to believe that the authors are not to be taken seriously.

We can remove the Shakespeare reference from the title.

This sentiment is further supported by the fact that the manuscript makes many fundamental mistakes in the use of precise and meaningful terms used in the profession and additionally makes poor undisciplined inferences/assertions.

For instance, the first sentence of the ms abstract is not correct (and could be deemed insulting to any ocean going bio-oceanographer that has completed many surveys regarding phytoplankton blooms that were not observed or constrained by satellites).

We can reword the offending sentence to clarify our meaning. We thank the reviewer for pointing out the ambiguity.

The entire section regarding the factors that control the autumn bloom appears to be fanciful speculation and should be removed. The information at hand has no relevance to understanding the in-situ light conditions, or possible controls due to light, nutrients, grazing or other factors that might effect cell growth and/or mortality in its many possible forms. The authors state that light limitation is not prevalent in Antarctica.

We cannot find this statement in our manuscript. We do state the importance of light for limitation of growth a number of times, include a section on light (section 3.3.3).

This is another example of a nonsensical statement due to the fact that for 1/2 the year there is no light. Moreover, the authors list a section regarding nutrient availability and only discuss possible sources of iron- (yet later in the modeling analysis there is nitrate and silica considerations in model runs).

We refer to other nutrients, in addition to iron, in the section on nutrients 3.3.4.

The assertion that this loose aggregation of ice crystals provides a matrix that excludes grazing also is fanciful guesswork. Without data in-hand one might expect quite the opposite- whereby grazers would likely be attracted to the concentrated biomass (which has been observed on several occasions).

The reviewer makes a very good point – however we were being intentionally speculative to explain why the bloom exists and survives. We do acknowledge limitations in the opportunistic bloom underway sampling and the lack of grazer data.

The authors need to recognize and make distinct in their writing the difference between primary production and biomass. Figure E1 is a prime example of this fundamental mistake where MODIS color image denotes Chla biomass- NOT primary production (which is a rate).

This was a typo in the figure caption of E1 which should read 'Chla biomass' instead of 'primary production'. There is nowhere else in the manuscript or appendices where this occurs.

Moreover, throughout the ms there seems to be confusion and an assumption that the biomass in the ice pack that was observed during the ship cruise and that which is detected in satellites is a result of primary production and algal growth. The data to reach this conclusion is simply not convincing.

The topic brought to bear is not the first time this phenomena has been observed. Buck and Garrison in the 80s observed this phenomena and published several papers on the scavenging of algae during pack ice formation. Several authors have built on their observations and experiments in subsequent works.

We thank the reviewer for alerting us to the work by Buck and Garrison from the 1980s.

The rationale for having model scenarios conducted and presented in Appendix A is entirely unclear. The physics that this model simulates (e.g. thermodynamic ice thickening) are not likely to be remotely relevant to the scavenging and growth of ice algae in an ice field during its initial formation phases. Simple calculations of growth with different starting biomass concentrations due to scavenging and potential nutrient supply being provided by wave-pumping and water movement would yield similar insights.

The ms should have figures that have panels of the bloom development- the reliance of accessing an animation at a remote site make the accessing of the information tenuous.

OK. In place of the animation we could produce a series of figures in an appendix.

The animation is nice, and in fact suggests that a better constrained analysis of the dynamics of the system from that shown in Figure E1 to Figure 1a would likely yield better insights on the potential magnitude of the scavenging and potential for primary production and subsequent algal growth and additional biomass accumulation during the ice formation than has been presented.

Additionally, from the coloration in the image Figure 2 it is very likely that the Chla concentrations in the ice matrix are orders of magnitude larger than what was collected via the intakes at 4.8 meters and the authors should recognize this aspect of their observations and analyses.

We thank the reviewer for this support and we do acknowledge limitations in the opportunistic bloom underway sampling.

The assertion/statement that estimates of primary production are off because the biomass detected in the autumn needs to be developed. If the biomass detected in the autumn is there largely because of scavenging and not in-situ primary production then primary production estimates might be just fine.

Good point – however we will not know this until a full scale underway sampling through one of these blooms is conducted.

Table d1- they need to change the units for reporting biomass to micrograms C per liter as the units they are using when converting numbers and bio-volume to estimates of Carbon results in many zeros being reported when these are not actually zero- by choosing the wrong units and rounding there is a loss of potentially useful information.

The units were chosen to reflect the resolution of the data. Thus, rounding of the value to zero reflects the number of significant figure justifiable from the frequency with which a specific taxa was observed. Such zero values are caused by the rare (sometimes lone) occurrence of an individual of a particular species in 1 of the 20 replicate field counts performed.

The analyses of the autumn ocean color and its relationship to Climatic forcing/El-nino etc. is intriguing and I would like to see a more disciplined/organized presentation of this information. A figure with panels of ocean color in the autumn period to support the assertion- that these autumn blooms were there during La-Nina/and positive SAM is missing - yet NECESSARY!

It was beyond the scope of this work but is part of an ongoing project.

I do feel the novel aspect of the work needs to be presented, and although many of the review comments presented are highly negative- I would like for the authors to get this information into an organized form that is not so speculative and is more appropriate for publication.

We thank the reviewer for this encouraging remark.