Anonymous Referee #1

The manuscript by Lee et al., has been revised according reviewers' suggestions. I only have one minor revision suggestions before possible publication:

Section 4, first paragraph, the comparison of phytoplankton uptake rates are from three different months, I suggest instead of claiming that the current measurement is in the range of other two, it is better to say that late December is the time of peak uptake rate in this region. The author mentioned late in the paragraph that phytoplankton peaks during late Dec., but note that peak time of uptake rate is usually not the same as peak time of Chl-a bloom. This needs to be clarified. Or the author can show Chl-a values in those three months to prove the time of Chl-a peak.

➔We revised them as suggested in line 221-222, page 9 and line 228-233, page 10.

Anonymous Referee #2

General Comments:
I would like to commend the authors on their revision. It is clearly demonstrated that they sincerely considered the reviewers' comments, in particular with regard to unsubstantiated statements about the future of small phytoplankton in the Amundsen Sea. I would be excited to see this manuscript published in Biogeosciences. I recommend that it be accepted, but only after subjected to minor revisions. As discussed below, there are important technical and content-related issues that still need to be addressed.

Specific Comments:
There are still numerous typos throughout the manuscript. I have highlighted a few below, but by no means is this a comprehensive list. I strongly encourage the authors to carefully check the text. Please pay special attention to using consistent terminology.

➔We carefully checked our manuscript and revised it using consistent terminology.

Results section: This section is still a difficult read. It seems the goal is to describe the same information for each measurement. I would recommend making a template for the first measurement (chlorophyll-a), and then using it for each subsequent one (e.g POC, PON, etc.). Having the information for all measurements described identically will assist the reader.

➔We revised them in a consistent way as possible as we can but some sections need different way to describe.

Lines 18-19: "recently changing ocean condition" – please be more specific re. the changes to which you are referring.

➔We specified "warming and freshening" in line 18-19, page 2.

Line 64: "a shift" – indicate that this shift is recurrent.

➔Yes, it is recurrent. We indicated that in line 66, page 3.

Line 65-66: As currently written this statement implies that the meltwater effect driving the association with cryptophytes is the lower salinity (as opposed to a meltwater constituent, light, etc.). I don't believe Moline et al. (2004) speculate on the specific meltwater effect. Consider removing "and reduced surface water salinity".

➔We removed that.

Line 75: Water samples were collected for chlorophyll-a too, right (see also line 86)?

➔Yes, it is. We revised that

Lines 75-87: This paragraph appears to be a general description of the water collection for all the measurements. Move information specific to the carbon/nitrogen uptake rates to Section

➔We moved the sentence into the section 2.2 in line 112-117, page 5.

 2.3. Consolidate these lines into a single paragraph.

➔We consolidated the section into a single paragraph.

Line 106: Please justify why small-sized phytoplankton were defined as less than 5 um (as opposed to the canonical 20 um, for example). Additionally, why were chlorophyll-a samples size-fractionated through 20 um filters if the cutoff between large and small cells was 5 um?

➔For different size-fractionated chlorophyll-a concentrations, three different pore size filters were used for more detail analysis. But, for the carbon and nitrogen uptake rates of different size, we just followed other references since small-sized cells < 5 μm are generally defined as small phytoplankton in comparison to large diatoms (> 5 μm) (Robineau et al., 1994; reference therein) in line 124-125, page 6.

Lines: 272-273: A relatively lower carbon uptake rate of small-sized cells makes sense. However, citing a paper on Arctic phytoplankton doesn't seem justified. Is there a more general source that could be cited? Are there other explanations for the demonstrated relationship that should be considered?

➔We could not find the result in the Antarctic Ocean. We removed that.

Lines 269-284: This passage appears to be operating under the assumption that there will be a shift to smaller cells in the Amundsen Sea. However, the data presented in this paper do not support that prediction. Please make that clear. If you would like to include information on the carbon uptake rates and food quality of small phytoplankton, do so explicitly within the framework of a hypothetical community shift (and give examples of where this has occurred elsewhere).

➔Actually, it is not a prediction but it is a suggestion for monitoring the contributions of small phytoplankton, which provides valuable information for understanding a potential change in marine ecosystem, line 308-311, page 13.

Technical Corrections:

Lines 52-55: Run-on sentence.

➔We revised the sentence in line 53-56, page 3.

Line 60: "drives" – consider changing to "has driven".

➔We revised it.

Lines 69-71: This thesis statement has numerous grammatical errors.

➔We revised it.

Line 76: "1 to15" – change to "1 to 15".

➔We changed it.

Lines 83-84: add "the" to "non-polynya region" and "polynya region".

➔We added it.

Line 90: "at six" – change to "at the six".

➔We changed it.

Line 101: "bottle" should be plural; change "matches" to "matched"; remove "with".

➔We revised them.

Line 102: Remove "material".

➔We removed it.

Lines 103-104: Please be more clear re. how the bottles were inoculated. As currently written it seems bottles were inoculated with carbon, as well as with a nitrogen isotope. Was that the case?

➔We revised it in line 119-121, page 4-5.

Line 107: "24 mm" – should that be "25 mm"?

➔We changed it with 25 mm.

Line 109: "GF/F (24 mm)" – should that be "pre-combusted GF/F filters (25 mm)"?

➔We revised it.

Line 115: "small phytoplankton" – until this point the term "small-sized" has been used. See also Line 116. See also the Results section. Use one or the other.

➔We revised it with small phytoplankton consistently throughout the text.

Lines 123, 133: "depth-integrated total" vs. "total integral" – make consistent.

➔We revised it with depth-integrated total consistently throughout the text.

Line 126: Use either "to" or "-" consistently to indicate ranges. Fix elsewhere.

➔We revised it with depth-integrated total consistently throughout the text.

Lines 134-135: Where is the mean +/- SD?

➔We added that in line 155, page 7.

Lines 137-140: List statistics for non-polynya and polynya regions in the same order for all measurements.

➔We revised it with depth-integrated total consistently throughout the text.

Line 140: Why is there no comment regarding the statistical difference between non-polynya

and polynya regions as with the other measurements?

➔We added the result from statistics analysis in line 161-163, page 7.

Anonymous Referee #3

Major comments

The paper presents another in their series of papers by these authors looking at the phytoplankton community in the Amundsen Sea. This is a little-known region of high productivity and the contributions of small phytoplankton to overall biomass, productivity and elemental cycling is timely, especially given the prediction that this fraction of the community is predicted to become increasingly important with changing climate. Furthermore, the increasing interest in the decline of glaciers in the region due to basal melt, this region could be an interesting site to track the effect of declining salinity on the phytoplankton community. Thus I deem this paper timely and topical.

Title Does not sufficiently cover the content of the paper which deals with standing stocks (Chl a & POC), productivity and nutrient draw down.
➔We modified the title as "Small phytoplankton contributions to the standing stocks and the total primary production in the Amundsen Sea".
The manuscript is currently formulaic in its presentation. With each parameter measured being subjected to the same comparisons between small and large phytoplankton, inside and outside the Amundsen Bay polynya. Much of this presentation of data would be usefully presented in a table, freeing the authors up to consider the similarities and differences among sites and sizes. In addition, the authors consider few other environmental factors, including currents/gyres in the region, effects of the time of year, and successional maturity over the summer and where their study fits in this seasonal progression.
➔Tables have pro and con. Specific values for different phytoplankton sizes at different sites are not a main concern here. For the purpose of the overall patterns for each measured parameter, figures would be better. Based on the results, we summarized them in Table 1.
This kind of study is at early stage. At this stage, it is rather difficult to consider many other environmental factors related with the results in this study. But, since we have collected more data, we will further consider later based on multi-year data set.
The statistical analysis is rudimentary, consisting of t-test among pairs of variables and I wonder if this has led to "over-analysis" of their data and they provide no indication of the statistic methods they employed in their methods section. I am no authority on this topic and I suggest they should seek some advice on the best statistical approach for their study. It may be better to perform a one-way ANOVA to compare sites inside and outside the polynya using the concentrations and rates they have measured (this will give overall significance of difference). They could then use post-hoc Tukey tests to determine which specific variables are significantly different.
➔We used t-test for statistics analysis since the number of data set is small (only four stations in the non-polynya region vs 8 stations in the polynya region for comparisons)! T-test could be better for those small numbers of data. As I mentioned above, after more data are available, more powerful statistics will be used based on multi-year data set.
I am also a little uncertain why the authors consider site 3 (and 3.1) to be outside the polynya given that it consistently shows biological characteristics characteristic of polynya sites. This site is on the fringe of the polynya, experiencing approximately 30% ice cover, which is not high. At best it is in the transition zone between the two distinctly different environments. The authors might also wish to consider the currents /gyres in the region and the potential for carriage polynya-like phytoplankton communities to this site.
➔For grouping them, it would be more arbitrary to group them depending on the results

from this study. Instead, St. 3 and St. 3-1 were on the fringe of polynya area, experiencing approximately 30% sea ice cover. Following the definition of polynya as an area of open water within sea ice zone, we grouped them into non-polynya regions in page 5. But, we explained the sea ice conditions at St. 3 and St.3-1 in line 98-102, page 5.

Minor comments
It is unclear when the ice cover image refers to - the time of the survey?, the preceding winter? Please clarify.
➔We indicated the year for the ice cover data in the Fig.1.
There are instances repeating the results (e.g. lines 208, 224, 241).
➔We removed and revised them.
Lines 271-3. I think the argument should be reversed, namely that the proportion of the production contributed by the mall cells increases as the overall productivity declines. It has long been known that there is a fairly consistent background of nano- and pico-plankton upon which is superimposed the bloom of large cells, especially diatoms. Thus my suggestion that you reverse your argument.
➔ Yes, it is true. We totally agree with that. But, this case is different from a seasonal decrease in primary productivity and thus higher contribution of small phytoplankton. These data were collected from different locations during the similar period in 2013. Spatial difference (no seasonal trend) between small contribution and total productivity is our main concern here. So, lower productivities come from higher contributions of small phytoplankton during the similar period based on Fig. 7 because of lower productivity of small phytoplankton.
Captions overall need to be more fulsome e.g. caption to Table 1 should perhaps be "Percent contributions of small cells to depth-integrated total concentrations of chlorophyll a, POC, PON and uptake of carbon and nitrogen".
➔We revised all captions with more details.
Fig 1. Marine Byrd Land should be Marie Byrd Land - as above please specify the time of the ice concentration.
➔We changed it with Marie Byrd Land and indicated the time of the ice concentration in Fig. 1. and the text.
Fig 7. The points on your regression are not normally distributed on the X-axis, rendering your regression invalid. You need to log the % productivity vales as well or find another way to compare the productivity contributed by small cells with changing uptake rate.
➔Actually, we could not distribute the data normally since the data were obtained from field measurements in 2013. For a better regression as you suggested, we included some data points from 2012 (unpublished) and revised the Fig. 7 as below,
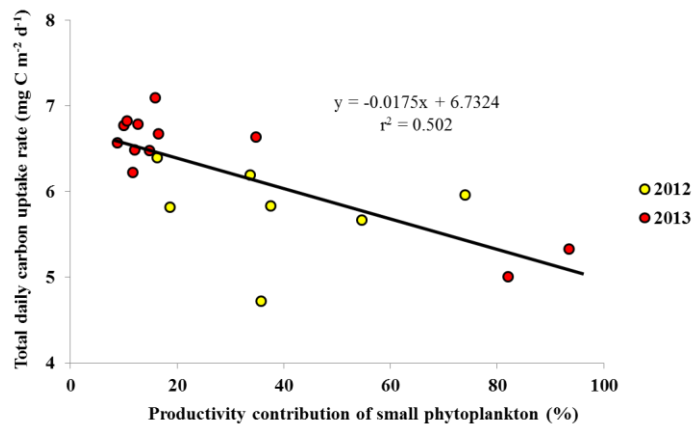
Fig. 7.