

Interactive comment on “Photosynthetic production in the Central Arctic during the record sea-ice minimum in 2012” by M. Fernández-Méndez et al.

Anonymous Referee #3

Received and published: 1 April 2015

Review of BGD-12-2897-2015 by Fernández-Méndez et al.

General comments

The manuscript presents results from a cruise in the Eurasian Basin of the Arctic Ocean in summer 2012. The study focuses on primary productivity, both as measured net primary production (NPP), from $^{14}\text{CO}_2$ uptake, and as new production estimated from seasonal drawdown in nutrients in the mixed layer. Samples from water column, sea ice and melt ponds were analysed and summarised for their contribution to the total primary production in this part of the Arctic Ocean. From the data the authors suggest the importance of ice algae and sub-ice algae to the primary production in the Arctic

C983

Ocean. The study is interesting and focuses on an important subject, and geographical area, where we need to improve our knowledge to understand future changes. I recommend the manuscript to be published after some rather minor revision.

The decision of the authors to put so many of the figures and tables in the Supplement is unfortunate, especially since they are referred to so frequently. I strongly recommend that all (or definitely most) of these to be included in the manuscript. (Figure S4 may be the least important.)

I miss the word ‘Ocean’ (as in ‘central Arctic Ocean’) in the title, and generally throughout the manuscript. This is a more correct term, but also makes it clearer. Also, the use of ‘Central Arctic’ is a bit confusing since the study only focuses on the Eurasian Basin (and adjacent shelf seas). This should maybe be changed for clarity.

Specific comments

P. 2899, L 20-28: When describing the area and general circulation it would be helpful to refer to a figure, with some of these features added. Figure 1 could partly do here, but then, for example, the Eurasian Basin should be noted.

P. 2901, L 5-9: The statement ‘nutrient availability … is probably decreasing…’ together with the sentence after makes me a bit confused. From the second sentence, as I read it, one could get the impression that although it has been hypothesised that nutrients may increase due to river runoff, this may not be enough to substantially increase primary production. However, could it partly counteract the potential decrease due to stronger stratification, so that the net effect may be no significant change? This should at least be clarified/rephrased. The word ‘probably’ (L 5) could be changed to ‘possibly’, or ‘may’, or something similar, but better is if the two sentences are rephrased.

P. 2902, L 6-7: FYI and MYI have not yet been defined. As far as I can see this is done first at P. 2904, and in Table 1, but needs to be defined here.

P. 2902, L 9-10: Data sources are important information that should not be put away in

C984

a Supplement (stresses one of the General comment above).

P. 2907, L 7: 'Only' duplicates of all treatments do not give strong statistics. The decision may have been due to available resources, but should possibly be commented on.

P. 2907, L 10-11 (and throughout): To me the use of 'negative control' (when no nutrients are added) is confusing, since no nutrients are removed. Why not simply use the term 'control (C)', since this is what it is?

P. 2908, L 14: What depth/layer is used as 'surface'?

P. 2908, L 21: I'm missing a reference to Dugdale and Goering (1967).

P. 2909, 3.1: To me much of this section feels like 'Method' material. It may be fine in the Results, but may be better placed earlier, to 'set the scene'.

P. 2912, L 17: The reference to Table 3 must be wrong. That table shows nutrient inventories. Should be Table 4/Table TS3. The different values presented on L 14-16, are they presented in any table/figure (I don't seem to find them)? There is also a very large uncertainty in the average value for the INPP in ice-covered waters. Any comment that could be added to this?

P. 2913, L 20-23: These stations do also show the shallowest depth of the Euphotic zone, which could be worth mentioning/discussing.

P. 2914, L 13-14: If only comparing two periods one cannot use the term 'trend'. Should be rephrased.

P. 2916, L 7-13: Is there any estimated uncertainty in the INPP from sedimentation of sub-ice algae? The potential total new production mentioned on line 12 does not have any extra uncertainty from this addition, but certainly this may be very large.

P. 2916, L 28-29 – P. 2917, L 1-2: These two sentences need some rephrasing. Suggestion: '..., they have their peak in production, and thus seem to adapt to higher light

C985

conditions. This would already have...'

P. 2917, L 8-9: Sentence stands a bit disconnected. Could be rephrased and merged with next sentence: Our nutrient addition experiment suggests... their biomass, which is in agreement with previous findings that sea-ice diatoms can store... (Kamp et al.,...).

P. 2917, L 23: The word 'normal' may not be the best choice, and would need to be explained. Maybe 'typical', or something related (during the last... time frame).

P. 2919, L 17-18: Are there any estimated uncertainty in these numbers?

P. 2919, 27-28 + P. 2920, L1-2: This is a repeat of what was said on page 2917.

P. 2920, L 17-19: This sentence is not very clear to me, but then the sentence after says the same thing, much better. Thus remove the first one, or do some merge.

P. 2921, L 21-27: One should be careful when comparing only two years. Was the same method used to estimate NPP in these two studies? Did they cover the same area? Also, as far as I can see the Vetrov and Romankevich did not present any uncertainty estimate of their average, making it more difficult to compare. In addition, their average value for the period does not tell whether there was some trend during these years. It comes down to a question of the significance of the difference, which is hard to evaluate.

P. 2924, L 24-27 + P. 2925, L 1: These sentences/statements are not very clear to me. In 'earlier sea-ice based NPP' you mean the timing? Some rephrasing could make this clearer.

Technical comments

Overall: typically citations in text should be arranged chronologically, not alphabetically as is done consistently in this manuscript.

P. 2902, L 16: Remove 'only'.

C986

P. 2902, L 22: Change 'the' to 'a' (CTD system).
P. 2903, L 5, and P. 2906, L 20: Chl a...
P. 2903, L 27: '... the amount of labelled...'
P. 2905, L 3: '...those where...'
P. 2905, L 4: NPP is already defined, on page 2903, so don't need to be written out here.
P. 2905, L 23-26: INPP has not been defined yet, and first comes on P. 2912 and Table 4 (as far as I can see).
P. 2912, L 8: (18 and 30, respectively).
P. 2912, L 26-27: Remove '... sampled with the peristaltic pump from the ice floe, ...' since this is described in the Methods.
P. 2914, L 10-28: I don't find a reference to Fig. S7. This would fit in the last paragraph on the page, but then figures from S7 and onwards need to be renumbered.
P. 2914, L 24: '... in 1982 as in 2012.'
P. 2920, L14-17: Many 'However' here, the first one could probably be removed.
P. 2924, L 27: '...Central Arctic Ocean Basins...'
Figures:
Fig. 2, Caption: Remove 'nitrate' after '> 3 μ M'.
Fig. 5: 'Central Arctic Ocean'. Add a notation about the different scales. Also, if this is the expected size of the figure in the printed version the text/values on the colour bar must be much larger. Now it's almost impossible to read them.
Supplement (which I hope will be moved to the manuscript):

C987

Table S2: Neither INPP nor MYI is explained/defined. It is of course in the manuscript, but would be helpful here. In addition the five geographical sections are the ones shown in Fig. S7, right? It could be helpful to refer to that here.

Fig. S6: Change 'nitrogen (A)' to 'nitrate (A)'.
Fig. S7: It would be helpful to see the sea-ice extent in 1982 depicted in (A).
Fig. S8: Make a note about the different scales for the Melt ponds panels.

Interactive comment on Biogeosciences Discuss., 12, 2897, 2015.

C988